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THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume; the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body upon any subject, either of Nature or Art, that comes before them. And therefore the

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thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

The Meteorological Journal hitherto kept by the Assistant Secretary at the Apartments of the Royal Society, by order of the President and Council, and published in the Philosophical Transactions, has been discontinued. The Government, on the recommendation of the President and Council, has established at the Royal Observatory at Greenwich, under the superintendence of the Astronomer Royal, a Magnetical and Meteorological Observatory, where observations are made on an extended scale, which are regularly published. These, which correspond with the grand scheme of observations now carrying out in different parts of the globe, supersede the necessity of a continuance of the observations made at the Apartments of the Royal Society, which could not be rendered so perfect as was desirable, on account of the imperfections of the locality and the multiplied duties of the observer.

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Adjudication of the Medals of the Royal Society for the year 1861 by the President and Council.

The Copley Medal to Professor Louis Agassiz, of Cambridge, Massachusetts, Foreign Member of the Royal Society, for his Researches in Palæontology and other branches of science, and particularly for his great works the "Poissons Fossiles," and "Les Poissons du vieux Grès Rouge d'Ecosse."

A Royal Medal to Dr. William B. Carpenter, F.R.S., for his Researches on the Foraminifera, contained in four Memoirs in the Philosophical Transactions, his investigations into the Structure of Shell, his observations on the embryonic Development of Purpura, and his various other writings in Physiology and Comparative Anatomy.

A Royal Medal to Professor James Joseph Sylvester, F.R.S., for his various Memoirs and Researches in Mathematical science.

The Bakerian Lecture was delivered by Professor J. Tyndall, F.R.S.: it was entitled "On the Absorption and Radiation of Heat by Gases and Vapours, and on the Physical Connexion of Radiation, Absorption, and Conduction."

The Croonian Lecture was delivered by Charles Edouard Brown-Séquard, Esq., F.R.S.: it was entitled "On the Relations between Muscular Irritability, Cadaveric Rigidity, and Putrefaction."

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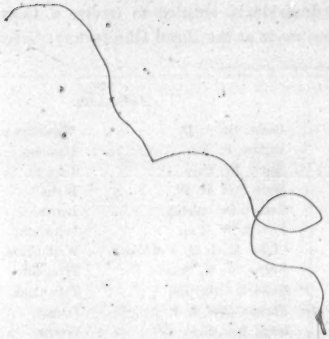


PHILOSOPHICAL TRANSACTIONS.

I. The Bakerian Lecture.—*On the Absorption and Radiation of Heat by Gases and Vapours, and on the Physical Connexion of Radiation, Absorption, and Conduction.* *By* John Tyndall, *Esq., F.R.S., Member of the Academies and Societies of Holland, Geneva, Göttingen, Zürich, Halle, Marburg, Breslau, la Société Philomathique of Paris, &c.; Professor of Natural Philosophy in the Royal Institution, and in the Government School of Mines.*

Received January 10,—Read February 7, 1861.

§ 1.

The researches on glaciers which I have had the honour of submitting from time to time to the notice of the Royal Society, directed my attention in a special manner to the observations and speculations of De Saussure, Fourier, M. Pouillet, and Mr. Hopkins, on the transmission of solar and terrestrial heat through the earth's atmosphere. This gave practical effect to a desire which I had previously entertained to make the mutual action of radiant heat and gases of all kinds the subject of an experimental inquiry.

Our acquaintance with this department of Physics is exceedingly limited. So far as my knowledge extends, the literature of the subject may be stated in a few words.

From experiments with his admirable thermo-electric apparatus, Melloni inferred that for a distance of 18 or 20 feet the absorption of radiant heat by atmospheric air is perfectly insensible*.

With a delicate apparatus of the same kind, Dr. Franz of Berlin found that the air contained in a tube 3 feet long absorbed 3·54 per cent. of the heat sent through it from an Argand lamp; that is to say, calling the number of rays which passed through the exhausted tube 100, the number which passed when the tube was filled with air was only 96·46†.

In the sequel I shall refer to circumstances which induce me to conclude that the

* La Thermochrose, p. 136.

† Poggendorff's Annalen, vol. xciv. p. 342.

result obtained by Dr. FRANZ is due to an inadvertence in his mode of observation. These are the only experiments of this nature with which I am acquainted, and they leave the field of inquiry now before us perfectly unbroken ground.

§ 2.

At an early stage of the investigation I experienced the need of a first-class galvanometer. My instrument was constructed by that excellent workman, SAUERWALD of Berlin. The needles are suspended independently of the shade; the latter is constructed so as to enclose the smallest possible amount of air, the disturbance of aërial currents being thereby practically avoided. The plane glass plate, which forms the cover of the instrument, is close to the needle, so that the position of the latter can be read off with ease and accuracy either by the naked eye or by a magnifying lens.

The wire of the coil belonging to this instrument was drawn from copper obtained from a galvano-plastic manufactory in the Prussian Capital; but it was not free from the magnetic metals. In consequence of its impurity in this respect, when the needles were perfectly astatic they deviated as much as 30° right and left of the neutral line. To neutralize this a "compensator" was made use of, by which the needle was gently drawn to zero in opposition to the magnetism of the coil.

But the instrument suffered much in point of delicacy from this arrangement, and accurate quantitative determinations with it were unattainable. I therefore sought to replace the Berlin coil by a less magnetic one. Mr. BECKER first supplied me with a coil which reduced the lateral deflection from 30° to 3°.

But even this small residue was a source of great annoyance to me, and for a time I almost despaired of obtaining pure copper wire. I knew that Professor MAGNUS had succeeded in obtaining it for his galvanometer, but the labour of doing so was immense*. Previous to undertaking a similar task, the thought occurred to me, that for my purpose a magnet furnished an immediate and perfect test as to the quality of the wire. Pure copper is *diamagnetic*; hence its repulsion or attraction by the magnet would at once declare its fitness or unfitness for the purpose which I had in view.

Fragments of the wire first furnished to me by M. SAUERWALD were strongly attracted by the magnet. The wire furnished by Mr. BECKER, when covered with its green silk, was also attracted, though in a much feebler degree.

I then removed the green silk covering from the latter and tested the naked wire. *It was repelled.* The whole annoyance was thus fastened on the green silk; some iron compound had been used in the dyeing of it; and to this the deviation of my needle from zero was manifestly due.

I had the green coating removed and the wire overspun with white silk, clean hands being used in the process. A perfect galvanometer is the result. The needle, when released from the action of a current, returns accurately to zero, and is perfectly free from all magnetic action on the part of the coil. In fact while we have been devising

* POGGENDORFF'S Annalen, vol. lxxxiii. p. 489; and Phil. Mag. 1852, vol. iii. p. 82.

agate plates and other elaborate methods to get rid of the great nuisance of a magnetic coil *, the means of doing so are at hand. Nothing is more easy to be found than diamagnetic copper wire. Out of eleven specimens, four of which were furnished by Mr. Becker, and seven taken at random from our laboratory, nine were found diamagnetic and only two magnetic.

Perhaps the only defect of those fine instruments with which Du Bois Raymond conducts his admirable researches in animal electricity is that above alluded to. The needle never comes to zero, but is drawn to it by a minute magnet. This defect may be completely removed. By the substitution of clean white silk for green, however large the coil may be, the compensator may be dispensed with, and a great augmentation of delicacy secured. The instrument will be rendered suitable for quantitative measurements; effects which are now beyond the reach of experiment will be rendered manifest; while the important results hitherto established will be obtained with a fraction of the length of wire now in use †.

§ 3.

Our present knowledge of the deportment of liquids and solids, would lead to the inference, that if gases and vapours exercised any appreciable absorptive power on radiant heat, the absorption would make itself most manifest on heat emanating from an obscure source. But an experimental difficulty occurs at the outset in dealing with such heat. How must we close the receiver containing the gases through which the calorific rays are to be sent? Melloni found that a glass plate one-tenth of an inch in thickness intercepted all the rays emanating from a source of the temperature of boiling water, and fully 94 per cent. of the rays from a source of 400° Centigrade. Hence a tube closed with glass plates would be scarcely more suitable for the purpose now under consideration, than if its ends were stopped by plates of metal.

Rock-salt immediately suggests itself as the proper substance; but to obtain plates of suitable size and transparency was exceedingly difficult. Indeed, had I been less efficiently seconded, the obstacles thus arising might have been insuperable. To the Trustees of the British Museum I am indebted for the material of one good plate of salt; to Mr. Harlin for another; while Mr. Lettsom, at the instance of Mr. Darker ‡, brought me a piece of salt from Germany from which two fair plates were taken. To Lady Murchison, Sir Emerson Tennant, Sir Philip Egerton, and Mr. Pattison my best thanks are also due for their friendly assistance.

The first experiments were made with a tube of tin polished inside, 4 feet long and 2·4

* See Melloni upon this subject, 'Thermochrose,' pp. 31–33.

† Mr. Becker, to whose skill and intelligence I have been greatly indebted, furnished me with several specimens of wire of the same fineness as that used by Du Bois Raymond, some covered with green silk and others with white. The former were invariably attracted, the latter invariably repelled. In all cases the *naked* wire was repelled.

‡ During the course of the inquiry I have often had occasion to avail myself of the assistance of this excellent mechanician.

inches in diameter, the ends of which were furnished with brass appendages to receive the plates of rock-salt. Each plate was pressed firmly against a flange by means of a bayonet joint, being separated from the flange by a suitable washer. Various descriptions of leather washers were tried for this purpose and rejected. The substance finally chosen was vulcanized india-rubber very lightly smeared with a mixture of bees-wax and spermaceti. A T-piece was attached to the tube, communicating on one side with a good air-pump, and on the other with the external air, or with a vessel containing the proper gas.

The tube being mounted horizontally, a LESLIE's cube containing hot water was placed close to one of its ends, while an excellent thermo-electric pile, connected with its galvanometer, was presented to the other. The tube being exhausted, the calorific rays sent through it fell upon the pile, a permanent deflection of 30° being the consequence. The temperature of the water was in the first instance purposely so arranged as to produce this deflection.

Dry air was now admitted into the tube, while the needle of the galvanometer was observed with all possible care. Even by the aid of a magnifying lens I could not detect the slightest change of position. Oxygen, hydrogen, and nitrogen, subjected to the same test, gave the same negative result. The temperature of the water was subsequently lowered so as to produce a deflection of 20° and 10° in succession, and then heightened till the deflection amounted to 40°, 50°, 60° and 70°; but in no case did the admission of air, or any of the above gases into the exhausted tube, produce any sensible change in the position of the needle.

It is a well-known peculiarity of the galvanometer, that its higher and lower degrees represent different amounts of calorific action. In my instrument, for example, the quantity of heat necessary to move the needle from 60° to 61° is about twenty times that required to move it from 11° to 12°. Now in the case of the small deflections above referred to, the needle was, it is true, in a sensitive position; but then the total amount of heat passing through the tube was so inconsiderable that a small per-centage of it, even if absorbed, might well escape detection. In the case of the large deflections, on the other hand, though the total amount of heat was large, and though the quantity absorbed might be proportionate, the needle was in such a position as to require a very considerable abstraction of heat to produce any sensible change in its position. Hence arose the thought of operating, if possible, with large quantities of heat, while the needle intended to reveal its absorption should continue to occupy its position of maximum delicacy.

The first attempt at solving this problem was as follows: my galvanometer is a differential one; the coil being composed of two wires wound side by side, so that a current could be sent through either of them independent of the other. The thermo-electric pile was placed at one end of the tin tube, and the ends of one of the galvanometer wires connected with it. A copper ball heated to low redness being placed at the other end of the tube, the needle of the galvanometer was propelled to its stops near 90°. The ends

of the second wire were now so attached to a second pile that when the latter was caused to approach the copper ball, the current thus excited passed through the coil in a direction opposed to the first one. Gradually, as the second pile was brought nearer to the source of heat, the needle descended from the stops, and when the two currents were nearly equal the position of the needle was close to zero.

Here then we had a powerful flux of heat through the tube; and if a column of gas four feet long exercised any sensible absorption, the needle was in the position best calculated to reveal it. In the first experiment made in this way, the neutralization of one current by the other occurred when the tube was filled with air; and after the exhaustion of the tube had commenced, the needle started suddenly off in a direction which indicated that a *less* amount of heat passed through the partially exhausted tube, than through the tube filled with air. The needle, however, soon stopped, turned, descended quickly to zero, and passed on to the other side, where its deflection became permanent. The air made use of in this experiment came direct from the laboratory, and the first impulsion of the needle was probably due to the aqueous vapour precipitated as a cloud by the sudden exhaustion of the tube. When, previous to its admission, the air was passed over chloride of calcium, or pumice-stone moistened with sulphuric acid, no such effect was observed. The needle moved steadily in one direction until its maximum deflection was attained, and this deflection showed that in all cases radiant heat was absorbed by the air within the tube.

These experiments were commenced in the spring of 1859, and continued without intermission for seven weeks. The course of the inquiry during this whole period was an incessant struggle with experimental difficulties. Approximate results were easily obtainable, but I aimed at exact measurements, which could not be made with a varying source of heat like the copper ball. I resorted to copper cubes containing fusible metal, or oil, raised to a high temperature; but was not satisfied with their action. I finally had a lamp constructed which poured a sheet of gas-flame along a plate of copper; and to keep the flame constant, a gas regulator specially constructed for me by Mr. HULET was made use of. It was also arranged that the radiating plate should form one of the walls of a chamber which could be connected with the air-pump and exhausted; so that the heat emitted by the copper plate might cross a vacuum before entering the experimental tube. With this apparatus I determined approximately the absorption of nine gases and twenty vapours during the summer of 1859. The results would furnish materials for a long memoir; but increased experience and improved methods have enabled me to substitute for them others of greater value; I shall therefore pass over the work of these seven weeks without further allusion to it.

On the 9th of September of the present year (1860) I resumed the inquiry. For three weeks I worked with the plate of copper as my source of heat, but finally rejected it on the score of insufficient constancy. I again resorted to the cube of hot oil, and continued to work with it up to Monday the 29th of October. During the seven weeks just referred to, I experimented from eight to ten hours daily; but these experiments, though

more accurate, must unhappily share the fate of the former ones. In fact the period was one of discipline,—a continued struggle against the difficulties of the subject and the defects of the locality in which the inquiry was conducted.

My reason for making use of the high sources of heat above referred to was, that the absorptive power of some of the gases which I had examined was so small that to make it clearly evident, a high temperature was essential. For other gases, and for *all* the vapours that had come under my notice, a source of lower temperature would have been not only sufficient, but far preferable. I was finally induced to resort to boiling water, which, though it gave greatly diminished effects, was capable of being preserved at so constant a temperature, that deflections which, with the other sources, would be masked by the errors of observation, became with it true quantitative measures of absorption.

§ 4.

The entire apparatus made use of in the experiments on absorption is figured on Plate I. S S′ is the *experimental tube* composed of brass, polished within, and connected, as shown in the figure, with the air-pump, A A. At S and S′ are the plates of rock-salt which close the tube air-tight. The length from S to S′ is 4 feet. C is a cube containing boiling water, in which is immersed the thermometer *t*. The cube is of cast copper, and on one of its faces a projecting ring was cast to which a brass tube of the same diameter as S S′, and capable of being connected air-tight with the latter, was carefully soldered. The face of the cube within the ring is the radiating plate, which is coated with lampblack. Thus between the cube C and the first plate of rock-salt there is *a front chamber* F, connected with the air-pump by the flexible tube D D, and capable of being exhausted independently of S S′. To prevent the heat of conduction from reaching the plate of rock-salt S, the tube F is caused to pass through a vessel V, being soldered to the latter where it enters it and issues from it. This vessel is supplied with a continuous flow of cold water through the influx tube *i i*, which dips to the bottom of the vessel; the water escapes through the efflux tube *e e*, and the continued circulation of the cold liquid completely intercepts the heat that would otherwise reach the plate S. The cube C is heated by the gas-lamp L. P is the thermo-electric pile placed on its stand at the end of the experimental tube, and furnished with two conical reflectors, as shown in the figure. C′ is the *compensating cube*, used to neutralize by its radiation* the effect of the rays passing through S S′. The regulation of this neutralization was an operation of some delicacy; to effect it the double screen H was connected with a winch and screw arrangement, by which it could be advanced or withdrawn through extremely minute spaces. For this most useful adjunct I am indebted to the kindness of my friend Mr. Gassiot. N N is the galvanometer with perfectly astatic needles, and perfectly non-magnetic coil; it is connected with the pile P by the wires *w w*; Y Y is a system of six chloride-of-calcium tubes, each 32 inches long; R is a

* It will be seen that in this arrangement I have abandoned the use of the differential galvanometer, and made the thermo-electric pile the differential instrument.

U-tube containing fragments of pumice-stone, moistened with strong caustic potash; and Z is a second similar tube, containing fragments of pumice-stone wetted with strong sulphuric acid. When *drying* only was aimed at, the potash tube was suppressed. When, on the contrary, as in the case of atmospheric air, both moisture and carbonic acid were to be removed, the potash tube was included. G G is a holder from which the gas to be experimented with was sent through the drying tubes, and thence through the pipe *p p* into the experimental tube S S'. The appendage at M and the arrangement at O O may for the present be disregarded; I shall refer to them particularly by and by.

The mode of proceeding was as follows:—The tube S S' and the chamber F being exhausted as perfectly as possible, the connexion between them was intercepted by shutting off the cocks *m*, *m'*. The rays from the interior blackened surface of the cube C passed first across the vacuum F, then through the plate of rock-salt S, traversed the experimental tube, crossed the second plate S', and being concentrated by the anterior conical reflector, impinged upon the adjacent face of the pile P. Meanwhile the rays from the hot cube C' fell upon the opposite face of the pile, and the position of the galvanometer needle declared at once which source was predominant. A movement of the screen H back or forward with the hand sufficed to establish an approximate equality; but to make the radiations perfectly equal, and thus bring the needle exactly to 0°, the fine motion of the screw above referred to was necessary. The needle being at 0°, the gas to be examined was admitted into the tube; passing, in the first place, through the drying apparatus. Any required quantity of the gas may be admitted; and here experiments on gases and vapours enjoy an advantage over those with liquids and solids, namely, the capability of changing the density at pleasure. When the required quantity of gas had been admitted, the galvanometer was observed, and from the deflection of its needle the absorption was accurately determined.

Up to about its 36th degree, the degrees of my galvanometer are all equal in value; that is to say, it requires the same amount of heat to move the needle from 1° to 2° as to move it from 35° to 36°. Beyond this limit the degrees are equivalent to larger amounts of heat. The instrument was accurately calibrated by the method recommended by Melloni (Thermochrose, p. 59), so that the precise value of its larger deflections are at once obtained by reference to a table. Up to the 36th degree, therefore, the simple deflections may be regarded as the expression of the absorption, but beyond this the absorption equivalent to any deflection is obtained from the table of calibration.

§ 5.

The air of the laboratory, freed from its moisture and carbonic acid, and permitted to enter until the tube was filled, produced a deflection of about

1°.

Oxygen obtained from chlorate of potash and peroxide of manganese produced a deflection of about

1°.

One specimen of nitrogen, obtained from the decomposition of nitrate of potash, pro-

duced a deflection of about

1°.

Hydrogen from zinc and sulphuric acid produced a deflection of about

1°.

Hydrogen obtained from the electrolysis of water produced a deflection of about

1°.

Oxygen obtained from the electrolysis of water, and sent through a series of eight bulbs containing a strong solution of iodide of potassium, produced a deflection of about

1°.

In the last experiment the electrolytic oxygen was freed from its ozone. The iodide of potassium was afterwards suppressed, and the oxygen, plus its ozone, admitted into the tube; the deflection produced was

4°.

Hence the small quantity of ozone which accompanied the oxygen in this case trebled the absorption of the oxygen itself*.

I have repeated this experiment many times, employing different sources of heat. With sources of high temperature the difference between the ozone and the ordinary oxygen comes out very strikingly. By careful decomposition a much larger amount of ozone might be obtained, and a corresponding large effect on radiant heat produced.

In obtaining the electrolytic oxygen I made use of two different vessels. To diminish the resistance of the acidulated water to the passage of the current, I placed in one vessel a pair of very large platinum plates, between which the current from a battery of ten of GROVE'S cells was transmitted. The oxygen bubbles liberated on so large a surface were extremely minute, and the gas thus generated, on being sent through iodide of potassium, scarcely coloured the liquid; the characteristic odour of ozone was also almost entirely absent. In the second vessel smaller plates were used. The bubbles of oxygen were much larger, and did not come into such intimate contact with either the platinum or the water. The oxygen thus obtained showed the characteristic reactions of ozone, and with it the above result was obtained.

The total amount of heat transmitted through the tube in these experiments produced a deflection of

71°·5.

Taking as unit of heat the quantity necessary to cause the needle to move from 0° to 1°, the number of units expressed by the above deflection is

308.

Hence the absorption by the above gases amounted to about 0·33 per cent.

I am unable at the present moment to range with certainty oxygen, hydrogen, nitrogen, and atmospheric air in the order of their absorptive powers, though I have made several hundred experiments with the view of doing so. Their proper action is so small that the slightest foreign impurity gives one a predominance over the other. In

* It will be seen further on that this result is in harmony with the supposition that ozone, obtained in the manner described, is a *compound* body.

preparing the gases, I have resorted to the methods which I found recommended in chemical treatises, but as yet only to discover the defects incidental to these methods. Augmented experience and the assistance of my friends will, I trust, enable me to solve this point by and by. An examination of the whole of the experiments induces me to regard hydrogen as the gas which exercises the lowest absorptive power.

We have here the cases of minimum gaseous absorption. It will be interesting to place in juxtaposition with the above results some of those obtained with olefiant gas, —the most highly absorbent permanent gas that I have hitherto examined. I select for this purpose an experiment made on the 21st of November.

The needle being steady at zero, in consequence of the equality of the actions on the opposite faces of the pile, the admission of olefiant gas gave a permanent deflection of

70°·3.

The gas being completely removed, and the equilibrium re-established, a plate of polished metal was interposed between one of the faces of the pile and the source of heat adjacent. The total amount of heat passing through the exhausted tube was thus found to produce a deflection of

75°.

Now a deflection of 70°·3 is equivalent to 290 units, and a deflection of 75° is equivalent to 360 units; hence more than seven-ninths of the total heat was cut off by the olefiant gas, or about 81 per cent.

The extraordinary energy with which the needle was deflected when the olefiant gas was admitted into the tube, was such as might occur had the plates of rock-salt become suddenly covered with an opake layer. To test whether any such action occurred, I polished a plate carefully, and projected against it for a considerable time a stream of the gas; there was no dimness produced. The plates of rock-salt, moreover, which were removed daily from the tube, usually appeared as bright when taken out as when they were put in.

The gas in these experiments issued from its holder, and had there been in contact with cold water. To test whether it had chilled the plates of rock-salt, and thus produced the effect, I filled a similar holder with atmospheric air, and allowed it to attain the temperature of the water; but its action was not thereby sensibly augmented.

In order to subject the gas to ocular examination, I had a glass tube constructed and connected with the air-pump. On permitting olefiant gas to enter it, not the slightest dimness or opacity was observed. To remove the last trace of doubt as to the possible action of the gas on the plates of rock-salt, the tin tube referred to at the commencement was perforated at its centre and a cock inserted into it; the source of heat was at one end of the tube, and the thermo-electric pile at some distance from the other. The plates of salt were entirely abandoned, the tube being open at its ends and consequently full of air. On allowing the olefiant gas to stream for a second or two into the tube through the central cock, the needle flew off and struck against its stops. It was held steadily for a considerable time between 80° and 90°.

A slow current of air sent through the tube gradually removed the gas, and the needle returned accurately to zero.

The gas within the holder being under a pressure of about 12 inches of water, the cock attached to the cube was turned quickly on and off; the quantity of gas which entered the tube in this brief interval was sufficient to cause the needle to be driven to the stops, and steadily held between 60° and 70°.

The gas being again removed, the cock was turned once half round as quickly as possible. The needle was driven in the first instance through an arc of 60°, and was held permanently at 50°.

The quantity of gas which produced this last effect, on being admitted into a graduated tube, was found not to exceed one-sixth of a cubic inch in volume.

The tube was now taken away, and both sources of heat allowed to act from some distance on the thermo-electric pile. When the needle was at zero, olefiant gas was allowed to issue from a common argand burner into the air between one of the sources of heat and the pile. The gas was invisible, nothing was seen in the air, but the needle immediately declared its presence, being driven through an arc of 41°. In the four experiments last described, the source of heat was a cube of oil heated to 250° Centigrade, the compensation cube being filled with boiling water*.

Those who like myself have been taught to regard transparent gases as almost perfectly diathermanous, will probably share the astonishment with which I witnessed the foregoing effects. I was indeed slow to believe it possible that a body so constituted, and so transparent to light as olefiant gas, could be so densely opake to any kind of calorific rays; and to secure myself against error, I made several hundred experiments with this single substance. By citing them at greater length, however, I do not think I could add to the conclusiveness of the proofs just furnished, that the case is one of true calorific absorption†.

§ 6.

Having thus established in a general way the absorptive power of olefiant gas, the question arises, "What is the relation which subsists between the density of the gas and the quantity of heat extinguished?"

I sought at first to answer this question in the following way:—An ordinary mercurial gauge was attached to the air-pump; the experimental tube being exhausted, and the needle of the galvanometer at zero, olefiant gas was admitted until it depressed the mercurial column 1 inch, the consequent deflection being noted; the gas was then admitted until a depression of 2 inches was observed, and thus the absorption effected by gas of 1, 2, 3, and more inches tension was determined. In the following Table the first column contains the tensions in inches, the second the deflections, and the third the absorption equivalent to each deflection.

* With a cube containing boiling water I have since made this experiment visible to a large audience.

† It is evident that the old mode of experiment might be applied to this gas. Indeed, several of the solids examined by Melloni are inferior to it in absorptive power. Had time permitted, I should have checked my results by experiments made in the usual way; this I intend to do on a future occasion.

TABLE I.—Olefiant Gas.

Tensions in inches.	Deflection.	Absorption.
1	56°	90
2	58·2	123
3	59·3	142
4	60·0	157
5	60·5	168
6	61·0	177
7	61·4	182
8	61·7	186
9	62·0	190
10	62·2	192
20	66·0	227

No definite relation between the density of the gas and its absorption is here exhibited. We see that an augmentation of the density *seven times* about *doubles* the amount of the absorption; while gas of 20 inches tension effects only $2\frac{1}{2}$ times the absorption of gas possessing 1 inch of tension.

But here the following reflections suggest themselves: it is evident that olefiant gas of 1 inch tension, producing so large a deflection as 56°, must extinguish a large proportion of the rays which are capable of being absorbed by the gas, and hence the succeeding measures having a less and less amount of heat to act upon must produce a continually smaller effect. But supposing the quantity of gas first introduced to be so inconsiderable that the number of rays extinguished by it is a vanishing quantity compared with the total number capable of absorption, we might reasonably expect that in this case a double quantity of gas would produce a double effect, a treble quantity a treble effect, or in general terms, that the absorption would, for a time, be proportional to the density.

To test this idea, a portion of the apparatus, which was purposely omitted in the description already given, was made use of: O O, Plate I., is a graduated glass tube, the end of which dips into the basin of water B. The tube can be stopped above by means of the stopcock r; $d\,d$ is a tube containing fragments of chloride of calcium. The tube O O being first filled with water to the cock r, had this water displaced by olefiant gas; and afterwards the tube S S′, and the entire space between the cock r and the experimental tube, was exhausted. The cock n being now closed and r' left open, the cock r at the top of the tube O O was carefully turned on and the gas permitted to enter the tube S S′ with extreme slowness. The water rose in O O, each of whose smallest divisions represents a volume of $\frac{1}{50}$th of a cubic inch. Successive measures of this capacity were admitted into the tube and the absorption in each case determined.

In the following Table the first column contains the quantity of gas admitted into the tube; the second contains the corresponding deflection, which, within the limits of the

Table, expresses the absorption; the third column contains the absorption, calculated on the supposition that it is proportional to the density.

TABLE II.—Olefiant Gas.

Unit-measure $\frac{1}{360}$th of a cubic inch.

Measures of gas.	Absorption. Observed.	Absorption. Calculated.
1	2·2	2·2
2	4·5	4·4
3	6·6	6·6
4	8·8	8·8
5	11·0	11·0
6	12·0	13·2
7	14·8	15·4
8	16·8	17·6
9	19·8	19·8
10	22·0	22·0
11	24·0	24·2
12	25·4	26·4
13	29·0	28·6
14	30·2	29·8
15	33·5	33·0

This Table shows the correctness of the foregoing surmise, and proves that for small quantities of gas the absorption is exactly proportional to the density.

Let us now estimate the tensions of the quantities of gas with which we have here operated. The length of the experimental tube is 48 inches, and its diameter 2·4 inches; its volume is therefore 218 cubic inches. Adding to this the contents of the cocks and other conduits which led to the tube, we may assume that each fiftieth of a cubic inch of the gas had to diffuse itself through a space of 220 cubic inches. The tension, therefore, of a single measure of the gas thus diffused would be $\frac{1}{11,000}$th of an atmosphere,—a tension capable of depressing the mercurial column connected with the pump $\frac{1}{360}$th of an inch, or about $\frac{1}{15}$th of a millimetre!

But the absorptive energy of olefiant gas, extraordinary as it is shown to be by the above experiments, is far exceeded by that of some of the vapours of volatile liquids. A glass flask was provided with a brass cap furnished with an interior thread, by means of which a stopcock could be screwed air-tight on to the flask. Sulphuric ether being placed in the latter, the space above the liquid was completely freed of air by means of a second air-pump. The flask, with its closed stopcock, was now attached to the experimental tube; the latter was exhausted and the needle brought to zero. The cock was then turned on so that the ether-vapour slowly entered the experimental tube. An assistant observed the gauge of the air-pump, and when it had sunk an inch, the stopcock was

promptly closed. The galvanometric deflection consequent on the partial cutting off of the calorific rays was then noted; a second quantity of the vapour, sufficient to depress the gauge another inch, was then admitted, and in this way the absorptions of five successive measures, each possessing within the tube 1 inch of tension, were determined.

In the following Table the first column contains the tensions in inches; the second the deflection due to each, and the third the amount of heat absorbed, expressed in the units already referred to. For the purpose of comparison, I have placed the corresponding absorption of olefiant gas in the fourth column.

TABLE III.—Sulphuric Ether.

Tensions in inches.	Deflections.	Absorption.	Corresponding absorption by olefiant gas.
1	64·8	214	90
2	70·0	282	123
3	72·0	315	142
4	73·0	330	154
5	73·0	330	163

For these tensions the absorption of radiant heat by the vapour of sulphuric ether is more than twice the absorption of olefiant gas. We also observe, that in the case of the former the successive absorptions approximate more quickly to a ratio of equality. In fact the absorption produced by 4 inches of the vapour was sensibly the same as that produced by 5.

But reflections similar to those which we have already applied to olefiant gas are also applicable to ether. Supposing we make our unit-measure small enough, the number of rays first destroyed will vanish in comparison with the total number, and for a time the fact will probably manifest itself that the absorption is directly proportional to the density. To examine whether this is the case, the other portion of the apparatus, omitted in the general description, was made use of. K is a small flask with a brass cap, which is closely screwed on to the stopcock c'. Between the cocks c' and c, which latter is connected with the experimental tube, is the chamber M, the capacity of which was accurately determined. The flask k was partially filled with ether, and the air above the liquid removed. The stopcock c' being shut off and c turned on, the tube S S′ and the chamber M are exhausted. The cock c is now shut off, and c' being turned on, the chamber M becomes filled with pure ether vapour. By turning c' off and c on, this quantity of vapour is allowed to diffuse itself through the experimental tube, and its absorption determined; successive measures are thus sent into the tube, and the effect produced by each is noted. Measures of various capacities were made use of, according to the requirements of the vapours examined.

In the first series of experiments made with this apparatus, I omitted to remove the air from the space above the liquid; each measure therefore sent in to the tube was a mixture of vapour and air. This diminished the effect of the former; but the pro-

portionality, for small quantities, of density to absorption exhibits itself so decidedly as to induce me to give the observations. The first column, as usual, contains the measures of vapour, the second the observed absorption, and the third the calculated absorption. The galvanometric deflections are omitted, their equivalents being contained in the second column. In fact as far as the eighth observation, the absorptions are merely the record of the deflections.

TABLE IV.—Mixture of Ether Vapour and Air.

Unit-measure $\frac{1}{50}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	4·5	4·5
2	9·2	9·0
3	13·5	13·5
4	18·0	18·0
5	22·8	23·5
6	27·0	27·0
7	31·8	31·5
8	36·0	36·0
9	39·7	40·0
10	45·0	45·0
20	81·0	90·0
21	82·8	95·0
22	84·0	99·0
23	87·0	104·0
24	88·0	108·0
25	90·0	113·0
26	93·0	117·0
27	94·0	122·0
28	95·0	126·0
29	98·0	131·0
30	100·0	135·0

Up to the 10th measure we find that density and absorption augment in precisely the same ratio. While the former varies from 1 to 10, the latter varies from 4·5 to 45. At the 20th measure, however, a deviation from proportionality is apparent, and the divergence gradually augments from 20 to 30. In fact 20 measures tell upon the rays capable of being absorbed; the quantity destroyed becoming so considerable, that every additional measure encounters a smaller number of such rays, and hence produces a diminished effect.

With ether vapour alone the results recorded in the following Table were obtained. Wishing to determine the absorption exercised by vapour of very low tension, the capacity of the unit-measure was reduced to $\frac{1}{100}$th of a cubic inch.

Table V.—Sulphuric Ether.

Unit-measure $\frac{1}{100}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	5·0	4·6
2	10·3	9·2
4	19·2	18·4
5	24·5	23·0
6	29·5	27·0
7	34·5	32·2
8	38·0	36·8
9	44·0	41·4
10	46·2	46·2
11	50·0	50·6
12	52·8	55·2
13	55·0	59·8
14	57·2	64·4
15	59·4	69·0
16	62·5	73·6
17	65·5	77·2
18	68·0	83·0
19	70·0	87·4
20	72·0	92·0
21	73·0	96·7
22	73·0	101 2
23	73·0	105·8
24	77·0	110·4
25	78·0	115·0
26	78·0	119·6
27	80·0	124·2
28	80·5	128·8
29	81·0	133·4
30	81·0	138·0

We here find that the proportion between density and absorption holds sensibly good for the first eleven measures, after which the deviation gradually augments.

I have examined some specimens of ether which acted still more energetically on the thermal rays than those above recorded. No doubt for smaller measures than $\frac{1}{100}$th of a cubic inch the above law holds still more rigidly true; and in a suitable locality it would be easy to determine with perfect accuracy $\frac{1}{10}$th of the absorption produced by the first measure; this would correspond to $\frac{1}{1000}$th of a cubic inch of vapour. But on

entering the tube the vapour had only the tension due to the temperature of the laboratory, namely 12 inches. This would require to be multiplied by 2·5 to bring it up to that of the atmosphere. Hence the $\frac{1}{1000}$th of a cubic inch, the absorption of which I have affirmed to be capable of measurement, would, on being diffused through a tube possessing a capacity of 220 cubic inches, have a tension of $\frac{1}{220} \times \frac{1}{2 \cdot 5} \times \frac{1}{1000} = \frac{1}{500.000}$th part of an atmosphere!

I have now to record the results obtained with thirteen other vapours. The method of experiment was in all cases the same as that just employed in the case of ether, the only variable element being the size of the unit-measure; for with many substances no sensible effect could be obtained with a unit volume so small as that used in the experiments last recorded. With bisulphide of carbon, for example, it was necessary to augment the unit-measure 50 times, to render the measurements satisfactory.

TABLE VI.—Bisulphide of Carbon.

Unit-measure $\frac{1}{2}$ a cubic inch.

	Absorption.	
Measures.	Observed.	Calculated.
1	2·2	2·2
2	4·9	4·4
3	6·5	6·6
4	8·8	8·8
5	10·7	11·0
6	12·5	13·0
7	13·8	15·4
8	14·5	17·6
9	15·0	19·0
10	15·6	22·0
11	16·2	24·2
12	16·8	26·4
13	17·5	28·6
14	18·2	30·8
15	19·0	33·0
16	20·0	35·2
17	20·0	37·4
18	20·2	39·6
19	21·0	41·8
20	21·0	44·0

As far as the sixth measure the absorption is proportional to the density; after which the effect of each successive measure diminishes. Comparing the absorption effected by a quantity of vapour which depressed the mercury column half an inch, with that

effected by vapour possessing one inch of tension, the same deviation from proportionality is observed.

By mercurial gauge.

Tension.	Absorption.
½ inch	14·8
1 inch	18·8

These numbers simply express the galvanometric deflections, which, as already stated, are strictly proportional to the absorption as far as 36° or 37°. Did the law of proportion hold good, the absorption due to 1 inch of tension ought of course to be 29·6 instead of 18·8.

Whether for equal volumes of the vapours at their maximum density, or for equal tensions as measured by the depression of the mercurial column, bisulphide of carbon exercises the lowest absorptive power of all the vapours which I have hitherto examined. For very small quantities, a volume of sulphuric ether vapour, at its maximum density in the measure, and expanded thence into the tube, absorbs 100 times the quantity of radiant heat intercepted by an equal volume of bisulphide of carbon vapour at its maximum density. These are the extreme limits of the scale as far as my inquiries have hitherto proceeded. The action of every other vapour is less than that of sulphuric ether, and greater than that of bisulphide of carbon.

A very singular phenomenon was repeatedly observed during the experiments with bisulphide of carbon. After determining the absorption of the vapour, the tube was exhausted as perfectly as possible, the trace of vapour left behind being exceedingly minute. Dry air was then admitted to cleanse the tube. On again exhausting, after the first few strokes of the pump a jar was felt and a kind of explosion heard, while dense volumes of blue smoke immediately issued from the cylinders. The action was confined to the latter, and never propagated backwards into the experimental tube.

It is only with bisulphide of carbon that this effect has been observed. It may, I think, be explained in the following manner:—To open the valve of the piston, the gas beneath it must have a certain tension, and the compression necessary to produce this appears sufficient to cause the combination of the constituents of the bisulphide of carbon with the oxygen of the air. Such a combination certainly takes place, for the odour of sulphurous acid is unmistakeable amid the fumes.

To test this idea I tried the effect of compression in the air-syringe. A bit of tow or cotton wool moistened with bisulphide of carbon, and placed in the syringe, emitted a bright flash when the air was compressed. By blowing out the fumes with a glass tube, this experiment may be repeated twenty times with the same bit of cotton.

It is not necessary even to let the moistened cotton remain in the syringe. If the bit of tow or cotton be thrown into it, and out again as quickly as it can be ejected, on compressing the air the luminous flash is seen. Pure oxygen produces a brighter flash than atmospheric air. These facts are in harmony with the above explanation.

TABLE VII.—Amylene.

Unit-measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	3·4	4·3
2	8·4	8·6
3	12·0	12·9
4	16·5	17·2
5	21·6	21·5
6	26·5	25·8
7	30·6	30·1
8	35·3	34·4
9	39·0	38·7
10	44·0	43·0

For these quantities the absorption is proportional to the density, but for large quantities the usual deviation is observed, as shown by the following observations:—

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{2}$ inch	60°	157
1 inch	65	216

Did the proportion hold good, the absorption for an inch of tension ought of course to be 314 instead of 216.

TABLE VIII.—Iodide of Ethyl.

Unit-measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	5·4	5·1
2	10·3	10·2
3	16·8	15·3
4	22·2	20·4
5	26·6	25·5
6	31·8	30·6
7	35·6	35·9
8	40·0	40·8
9	44·0	45·9
10	47·5	51·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{2}$ inch	56·3	94
1 inch	58·2	120

TABLE IX.—Iodide of Methyl.

Unit-measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	3·5	3·4
2	7·0	6·8
3	10·3	10·2
4	15·0	13·6
5	17·5	17·0
6	20·5	20·4
7	24·0	23·8
8	26·3	27·2
9	30·0	30·6
10	32·3	34·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{2}$ inch	48·5	60
1 inch	56·5	96

TABLE X.—Iodide of Amyl.

Unit measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	0·6	0·57
2	1·0	1·1
3	1·4	1·7
4	2·0	2·3
5	3·0	2·9
6	3·8	3·4
7	4·5	4·0
8	5·0	4·6
9	5·0	5·1
10	5·8	5·7

The deflections here are very small; the substance, however, possesses so feeble a volatility, that the tension of a measure of its vapour, when diffused through the experimental tube, must be infinitesimal. With the specimen which I examined, it was not practicable to obtain a tension sufficient to depress the mercury gauge $\frac{1}{2}$ an inch; hence no observations of this kind are recorded.

TABLE XI.—Chloride of Amyl.

Unit-measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	1·3	1·3
2	3·0	2·6
3	3·8	3·9
4	5·1	5·2
5	6·8	6·5
6	8·5	7·8
7	9·0	9·1
8	10·9	10·4
9	11·3	11·7
10	12·3	13·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{3}$ inch	$5\overset{\circ}{9}$	137
1 inch	not practicable.	

TABLE XII.—Benzol.

Unit-measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption. Observed.	Calculated.
1	4·5	4·5
2	9·5	9·0
3	14·0	13·5
4	18·5	18·0
5	22·5	22·5
6	27·5	27·0
7	31·6	31·5
8	35·5	36·0
9	39·0	40·0
10	44·0	45·0
11	47·0	49·0
12	49·0	54·0
13	51·0	58·5
14	54·0	63·0
15	56·0	67·5
16	59·0	72·0
17	63·0	76·5
18	67·0	81·0
19	69·0	85·5
20	72·0	90·0

Up to the 10th measure, or thereabouts, the proportion between density and absorption holds good, from which onwards the deviation from the law gradually augments.

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{2}$ inch	$54\frac{2}{4}$	78
1 inch	57	103

TABLE XIII.—Methylic Alcohol.

Unit-measure $\frac{1}{10}$th of a cubic inch.

	Absorption.	
Measures.	Observed.	Calculated.
1	10·0	10·0
2	20·0	20·0
3	30·0	30·0
4	40·5	40·0
5	49·0	50·0
6	53·5	60·0
7	59·2	70·0
8	71·5	80·0
9	78·0	90·0
10	84·0	100·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{2}$ inch	$58\frac{8}{8}$	133
1 inch	60·5	168

TABLE XIV.—Formic Ether.

Unit-measure $\frac{1}{10}$th of a cubic inch.

	Absorption.	
Measures.	Observed.	Calculated.
1	8	7·5
2	16	15·0
3	22·5	22·5
4	30·0	30·0
5	35·2	37·5
6	39·5	45·0
7	45·0	52·5
8	48·0	60·0
9	50·2	67·5
10	53·5	75·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
½ inch	58°·8	133
1 inch	62·5	193

TABLE XV.—Propionate of Ethyl.

Unit-measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption.	
	Observed.	Calculated.
1	7·0	7·0
2	14·0	14·0
3	21·8	21·0
4	28·8	28·0
5	34·4	35·0
6	38·8	42·0
7	41·0	49·0
8	42·5	56·0
9	44·8	63·0
10	46·5	70·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
½ inch	60°·5	168
1 inch	not practicable.	

TABLE XVI.—Chloroform.

Unit-measure $\frac{1}{10}$th of a cubic inch.

Measures.	Absorption.	
	Observed.	Calculated.
1	4·5	4·5
2	9·0	9·0
3	13·8	13·5
4	18·2	18·0
5	22·3	22·5
6	27·0	27·0
7	31·2	31·5
8	35·0	36·0
9	39 0	40·5
10	40·0	45·0

Subsequent observations lead me to believe that the absorption by chloroform is a little higher than that given in the above Table.

TABLE XVII.—Alcohol.

Unit-measure $\frac{1}{2}$ a cubic inch.

No. of measures.	Absorption. Observed.	Calculated.
1	4·0	4·0
2	7·2	8·0
3	10·5	12·0
4	14·0	16·0
5	19·0	20·0
6	23·0	24·0
7	28·5	28·0
8	32·0	32·0
9	37·5	36·0
10	41·5	40·0
11	45·8	44·0
12	48·0	48·0
13	50·4	52·0
14	53·5	56·0
15	55·8	60·0

By mercurial gauge.

Tension.	Deflection.	Absorption.
$\frac{1}{2}$ inch	60°	157
1 inch	not practicable.	

The difference between the measurements when equal *tensions* and when equal *volumes* at the maximum density are made use of, is here strikingly exhibited.

In the case of alcohol I was obliged to resort to a unit-measure of $\frac{1}{2}$ a cubic inch to obtain an effect about equal to that produced by benzol with a measure possessing only $\frac{1}{10}$th of a cubic inch in capacity; and yet for equal tensions of 0·5 of an inch alcohol cuts off precisely twice as much heat as benzol. There is also an enormous difference between alcohol and sulphuric ether when equal measures at the maximum density are compared; but to bring the alcohol and ether vapours up to a common tension, the density of the former must be many times augmented. Hence it follows that when *equal tensions* of these two substances are compared, the difference between them diminishes considerably. Similar observations apply to many of the substances whose deportment is recorded in the foregoing Tables; to the iodide and chloride of amyl, for example, and to the propionate of ethyl. Indeed it is not unlikely that with

equal tensions the vapour of a perfectly pure specimen of the substance last mentioned would be found to possess a higher absorptive power than that of ether itself.

It has been already stated that the tube made use of in these experiments was of brass polished within, for the purpose of bringing into clearer light the action of the feebler gases and vapours. Once, however, I wished to try the effect of chlorine, and with this view admitted a quantity of the gas into the experimental tube. The needle was deflected with prompt energy, but on pumping out*, it refused to return to zero. To cleanse the tube, dry air was introduced into it ten times in succession; but the needle pointed persistently to the 40th degree from zero. The cause of this was easily surmised; the chlorine had attacked the metal and partially destroyed its reflecting power; thus the absorption by the sides of the tube itself cut off an amount of heat competent to produce the deflection mentioned above. For subsequent experiments the interior of the tube had to be repolished.

Though no other vapour with which I had experimented produced a permanent effect of this kind, it was necessary to be perfectly satisfied that this source of error had not vitiated the experiments. To check the results, therefore, I had a length of 2 feet of similar brass tube coated carefully on the inside with lampblack, and determined by means of it the absorptions of all the vapours which I had previously examined, at a common tension of 0·3 of an inch. A general corroboration was all I sought, and I am satisfied that the few discrepancies which the measurements exhibit would disappear, or be accounted for, in a more careful examination.

In the following Table the results obtained with the blackened and with the bright tubes are placed side by side, the tension in the former being three-tenths, and in the latter five-tenths of an inch.

TABLE XVIII.

Vapour.	Absorption. Bright tube, 0·5 tension.	Blackened tube, 0·3 tension.	Absorption with bright tube proportional to
Bisulphide of Carbon . . .	5·0	21	23
Iodide of Methyl	15·8	60	71
Benzol	17·5	78	79
Chloroform	17·5	89	79
Iodide of Ethyl	21·5	94	97
Wood-spirit	26·5	123	120
Methylic Alcohol	29·0	133	131
Chloride of Amyl	30·0	137	135
Amylene	31·8	157	143

* Dense dark fumes rose from the cylinders on this occasion; a similar effect was produced by sulphuretted hydrogen.

The order of absorption is here shown to be the same in both tubes, and the quantity absorbed in the bright tube is, in general, about 4½ times that absorbed in the black one. In the third column, indeed, I have placed the products of the numbers contained in the first column by 4·5. These results completely dissipate the suspicion that the effects observed with the bright tube could be due to a change of the reflecting power of its inner surface by the contact of the vapours.

With the blackened tube the order of absorption of the following substances, commencing with the lowest, stood thus:—

Alcohol,
Sulphuric ether,
Formic ether,
Propionate of ethyl;

whereas with the bright tube they stood thus:—

Formic ether,
Alcohol,
Propionate of ethyl,
Sulphuric ether.

As already stated, these differences would in all probability disappear, or be accounted for on re-examination. Indeed very slight differences in the purity of the specimens used, would be more than sufficient to produce the observed differences of absorption*.

§ 7. *Action of permanent Gases on Radiant Heat.*

The deportment of oxygen, nitrogen, hydrogen, atmospheric air, and olefiant gas has been already recorded. Besides these I have examined carbonic oxide, carbonic acid, sulphuretted hydrogen, and nitrous oxide. The action of these gases is so much feebler than that of any of the vapours referred to in the last section, that in examining the relationship between absorption and density the measures used with the vapours were abandoned, and the quantities of gas admitted were measured by the depression of the mercurial gauge.

TABLE XIX.—Carbonic Oxide.

Tension in inches.	Absorption.	
	Observed.	Calculated.
0·5	2·5	2·5
1·0	5·6	5·0
1·5	8·0	7·5
2·0	10·0	10·0
2·5	12·0	12·5
3·0	15·0	15·0
3·5	17·5	17·5

* In illustration of this I may state, that of two specimens of methylic alcohol with which I was furnished by two of my chemical friends, one gave an absorption of 84 and the other of 203. The former specimen

Up to a tension of $3\frac{1}{2}$ inches the absorption by carbonic oxide is proportional to the density of the gas. But this proportion does not obtain with large quantities of the gas, as shown by the following Table:—

Tension in inches.	Deflection.	Absorption.
5	18°0	18
10	32·5	32·5
15	41·0	45

TABLE XX.—Carbonic Acid.

Tension in inches.	Absorption.	
	Observed.	Calculated.
0·5	5·0	3·5
1·0	7·5	7·0
1·5	10·5	10·5
2·0	14·0	14·0
2·5	17·8	17·5
3·0	21·8	21·0
3·5	24·5	24·5

Here we have the proportion exhibited, but not so with larger quantities.

Tension in inches.	Deflection.	Absorption.
5	25°0	25
10	36·0	36
15	42·5	48

TABLE XXI.—Sulphuretted Hydrogen.

Tension in inches.	Absorption.	
	Observed.	Calculated.
0·5	7·8	6
1·0	12·5	12
1·5	18·0	18
2·0	24·0	24
2·5	30·0	30
3·0	34·5	36
3·5	36·0	42
4·0	36·5	48
4·5	38·0	54
5·0	40·0	60

had been purified with great care, but the latter was not pure. Both specimens, however, went under the common name of methylic alcohol. I have had a special apparatus constructed with a view to examine the influence of ozone on the interior of the experimental tube.

The proportion here holds good up to a tension of 2·5 inches, when the deviation from it commences and gradually augments. Though these measurements were made with all possible care, I should like to repeat them. Dense fumes issued from the cylinders of the air-pump on exhausting the tube of this gas, and I am not at present able to state with confidence that a trace of such in a very diffuse form within the tube, did not interfere with the purity of the results.

TABLE XXII.—Nitrous Oxide.

Tension in inches.	Absorption.	
	Observed.	Calculated.
0·5	14·5	14·5
1·0	23·5	29·0
1·5	30·0	43·5
2·0	35·5	58·0
2·5	41·0	71·5
3·0	45·0	87·0
3·5	47·7	101·5
4·0	49·0	116·0
4·5	51·5	130·5
5·0	54·0	145·0

Here the divergence from proportionality makes itself manifest from the commencement.

I promised at the first page of this memoir to allude to the results of Dr. FRANZ, and I will now do so. With a tube 3 feet long and blackened within, an absorption of 3·54 per cent. by atmospheric air was observed in his experiments. In my experiments, however, with a tube 4 feet long and polished within, which makes the distance traversed by the reflected rays more than 4 feet, the absorption is only one-tenth of the above amount. In the experiments of Dr. FRANZ, carbonic acid appears as a feebler absorber than oxygen. According to my experiments, for small quantities the absorptive power of the former is about 150 times that of the latter; and for atmospheric tensions, carbonic acid probably absorbs nearly 100 times as much as oxygen.

The differences between Dr. FRANZ and myself admit, perhaps, of the following explanation. His source of heat was an argand lamp, and the ends of his experimental tube were stopped with plates of glass. Now MELLONI has shown that fully 61 per cent. of the heat-rays emanating from a Locatelli lamp are absorbed by a plate of glass one-tenth of an inch in thickness. Hence in all probability the greater portion of the rays issuing from the lamp of Dr. FRANZ was expended in heating the two glass ends of his experimental tube. These ends thus became secondary sources of heat which radiated against his pile. On admitting air into the tube, the partial withdrawal by conduction and convection of the heat of the glass plates would produce an effect exactly the same as that of true absorption. By allowing the air in my tube to come into contact with the radiating plate, I have often obtained a deflection of twenty or thirty degrees; the effect

being due to the cooling of the plate and not to absorption. It is also certain that had I used heat from a luminous source, I should have found the absorption of 0·33 per cent. considerably diminished.

§ 8.

I have now to refer briefly to a point of considerable interest as regards the effect of our atmosphere on solar and terrestrial heat. In examining the separate effects of the air, carbonic acid, and aqueous vapour of the atmosphere on the 20th of last November, the following results were obtained:—

Air sent through the system of drying tubes and through the caustic potash tube produced an absorption of about

1.

Air direct from the laboratory, containing therefore its carbonic acid* and aqueous vapour, produced an absorption of

15.

Deducting the effect of the gaseous acids, it was found that the quantity of aqueous vapour diffused through the atmosphere on the day in question, produced an absorption at least equal to thirteen times that of the atmosphere itself.

It is my intention to repeat and extend these experiments on a future occasion†; but even at present conclusions of great importance may be drawn from them. It is exceedingly probable that the absorption of the solar rays by the atmosphere, as established by M. Pouillet, is mainly due to the watery vapour contained in the air. The vast difference between the temperature of the sun at midday and in the evening, is also probably due in the main to that comparatively shallow stratum of aqueous vapour which lies close to the earth. At noon the depth of it pierced by the sunbeams is very small; in the evening very great in comparison.

The intense heat of the sun's direct rays on high mountains is not, I believe, due to his beams having to penetrate only a small depth of air, but to the comparative absence of aqueous vapour at those great elevations.

But this aqueous vapour, which exercises such a destructive action on the obscure rays, is comparatively transparent to the rays of light. Hence the differential action, as regards the heat coming from the sun to the earth, and that radiated from the earth into space, is vastly augmented by the aqueous vapour of the atmosphere.

De Saussure, Fourier, M. Pouillet, and Mr. Hopkins regard this interception of the terrestrial rays as exercising the most important influence on climate. Now if, as the above experiments indicate, the chief influence be exercised by the aqueous vapour, every variation of this constituent must produce a change of climate. Similar remarks would apply to the carbonic acid diffused through the air; while an almost inappreciable admixture of any of the hydrocarbon vapours would produce great effects on the terrestrial rays and produce corresponding changes of climate. It is not therefore necessary

* And a portion of sulphurous acid produced by the two gas-lamps used to heat the cubes.

† The peculiarities of the locality in which this experiment was made render its repetition under other circumstances necessary.

to assume alterations in the density and height of the atmosphere, to account for different amounts of heat being preserved to the earth at different times; a slight change in its variable constituents would suffice for this. Such changes in fact may have produced all the mutations of climate which the researches of geologists reveal. However this may be, the facts above cited remain; they constitute true causes, the *extent* alone of the operation remaining doubtful.

The measurements recorded in the foregoing pages constitute only a fraction of those actually made; but they fulfil the object of the present portion of the inquiry. They establish the existence of enormous differences among colourless gases and vapours as to their action upon radiant heat; and they also show, that when the quantities are sufficiently small, the absorption in the case of each particular vapour is exactly proportional to the density.

These experiments furnish us with purer cases of molecular action than have been hitherto attained in experiments of this nature. In both solids and liquids the cohesion of the particles is implicated; they mutually control and limit each other. A certain action over and above that which belongs to them separately, comes into play and embarrasses our conceptions. But in the cases above recorded the molecules are perfectly free, and we fix upon them individually the effects which the experiments exhibit. Thus the mind's eye is directed more firmly than ever on those distinctive physical qualities whereby a ray of heat is stopped by one molecule and unimpeded by another.

§ 9. *Radiation of Heat by Gases.*

It is known that the quantity of light emitted by a flame depends chiefly on the incandescence of solid matter; the brightness of an ignited jet of ordinary gas, for example, being chiefly due to the solid particles of carbon liberated in the flame.

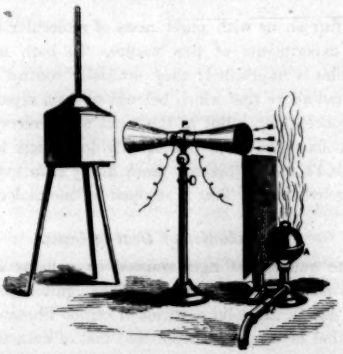
Melloni drew a parallel between this action and that of radiant heat. He found the radiation from his alcohol lamp greatly augmented by plunging a spiral of platinum wire into the flame. He also found that a bundle of wire placed in the current of hot air ascending from an argand chimney gave a copious radiation, while when the wire was withdrawn no trace of radiant heat could be detected by his apparatus. He concluded from this experiment that air possesses the power of radiation in so feeble a degree, that our best thermoscopic instruments fail to detect this power*.

These are the only experiments hitherto published upon this subject; and I have now to record those which have been made in connexion with the present inquiry. The pile furnished with its conical reflector was placed upon a stand, with a screen of polished tin in front of it. An alcohol lamp was placed behind the screen, so that its flame was entirely hidden by the latter; on rising above the screen, the gaseous column radiated its heat against the pile and produced a considerable deflection. The same effect was produced when a candle or an ordinary jet of gas was substituted for the alcohol lamp.

* La Thermochrose, p. 94.

The heated products of combustion acted on the pile in the above experiments, but the radiation from pure air was easily demonstrated by placing a heated iron spatula or metal sphere behind the screen. A deflection was thus obtained, which, when the spatula was raised to a red heat, amounted to more than sixty degrees. This action was due solely to the radiation of the air; no radiation from the spatula to the pile was possible, and no portion of the heated air itself approached the pile so as to communicate its warmth by contact to the latter. These effects are so easily produced, that I am at a loss to account for the inability of so excellent an experimenter as MELLONI to obtain them.

My next care was to examine whether different gases possessed different powers of radiation, and for this purpose the following arrangement was devised. P in the woodcut



represents the thermo-electric pile with its two conical reflectors; S is a double screen of polished tin; A is an argand burner consisting of two concentric rings perforated with orifices for the escape of the gas; C is a heated copper ball; the tube *t t* leads to a gas-holder containing the gas to be examined. When the ball C is placed on the argand burner, it of course heats the air in contact with it; an ascending current is established, which acts on the pile as in the experiments last described. It was found necessary to neutralize this radiation from the heated air, and for this purpose a large Leslie's cube L, filled with water a few degrees above the temperature of the air, was allowed to act on the opposite face of the pile.

When the needle was thus brought to zero, the cock of the gas-holder was turned on; the gas, passed through the burner, came into contact with the ball, and ascended afterwards in a heated column in front of the pile. The galvanometer was now observed, and the limit of the arc through which its needle was urged was noted. It is needless to remark that the ball was entirely hidden by the screen from the thermo-electric pile;

and that even were this not the case, the mode of neutralization adopted would still give us the pure action of the gas.

The results of the experiments are given in the following Table, the figure appended to the name of each gas marking the number of degrees through which the radiation from the latter urged the needle of the galvanometer*:—

	°
Air	0
Oxygen	0
Nitrogen	0
Hydrogen	0
Carbonic oxide	12
Carbonic acid	18
Nitrous oxide	29
Olefiant gas	53

The radiation from air, it will be remembered, was neutralized by the large Leslie's cube, and hence the 0° attached to it merely denotes that the propulsion of air from the gas-holder through the argand burner did not augment the effect. Oxygen, hydrogen, and nitrogen, sent in a similar manner over the ball, were equally ineffective. The other gases, however, not only exhibit a marked action, but also marked differences of action. Their radiative powers follow precisely the same order as their powers of absorption. In fact, the deflections actually produced by their respective absorptions at 5 inches tension are as follow:—

Air	A fraction of a degree.
Oxygen	" " "
Nitrogen	" " "
Hydrogen	" " "
Carbonic oxide	18°
Carbonic acid	25°
Nitrous oxide	44°
Olefiant gas	61°

It would be easy to give these experiments a more elegant form, and to arrive at greater accuracy, which I intend to do on a future occasion, but my object now is simply to establish the general order of their radiative powers. An interesting way of exhibiting both radiation and absorption is as follows:—When the polished face of a Leslie's cube is turned towards a thermo-electric pile the effect produced is inconsiderable, but it is greatly augmented when a coat of varnish is laid upon the polished surface. Instead of the coat of varnish, a film of gas may be made use of. Such a cube, containing boiling water, had its polished face turned towards the pile, and its effect on

* I have also rendered these experiments on radiation visible to a large audience. They may be readily introduced in lectures on radiant heat.

the galvanometer neutralized in the usual manner. The needle being at 0°, a film of olefiant gas, issuing from a narrow slit, was passed over the metal. The increase of radiation produced a deflection of 45°. When the gas was cut off, the needle returned accurately to 0°.

The absorption by a film may be shown by filling the cube with cold water, but not so cold as to produce the precipitation of the aqueous vapour of the atmosphere. A gilt copper ball, cooled in a freezing mixture, was placed in front of the pile, and its effect was neutralized by presenting a beaker containing a little iced water to the opposite face of the pile. A film of olefiant gas was sent over the ball, but the consequent deflection proved that the absorption, instead of being greater, was less than before. The ball, in fact, had been coated by a crust of ice, which is one of the best absorbers of radiant heat. The olefiant gas, being warmer than the ice, partially neutralized its absorption. When, however, the temperature of the ball was only a few degrees lower than that of the atmosphere, and its surface quite dry, the film of gas was found to act as a film of varnish; it augmented the absorption.

A remarkable effect, which contributed at first to the complexity of the experiments, can now be explained. Conceive the experimental tube exhausted and the needle at zero; conceive a small quantity of alcohol or ether vapour admitted; it cuts off a portion of the heat from one source, and the opposite source triumphs. Let the consequent deflection be 45°. If dry air be now admitted till the tube is filled, its effect of course will be slightly to augment the absorption and make the above deflection greater. But the following action is really observed:—when the air first enters, the needle, instead of ascending, descends; it falls to 26°, as if a portion of the heat originally cut off had been restored. At 26°, however, the needle stops, turns, moves quickly upwards, and takes up a permanent position a little higher than 45°. Let the tube now be exhausted, the withdrawal of the mixed air and vapour ought of course to restore the equilibrium with which we started; but the following effects are observed:—When the exhaustion commences the needle moves upwards from 45° to 54°; it then halts, turns, and descends speedily to 0°, where it permanently remains.

After many trials to account for the anomaly I proceeded thus:—A thermo-electric couple was soldered to the external surface of the experimental tube, and its ends connected with a galvanometer. When air was admitted, a deflection was produced, which showed that the air, on entering the vacuum, was heated. On exhausting, the needle was also deflected, showing that the interior of the tube was chilled. These are indeed known effects; but I was desirous to make myself perfectly sure of them. I subsequently had the tube perforated and thermometers screwed into it air-tight. On filling the tube the thermometric columns rose, on exhausting it they sank, the range between the maximum and minimum amounting in the case of air to 5° Fahr.

Hence the following explanation of the above singular effects. The absorptive power of the vapour referred to is very great, and its radiative power is equally so. The heat generated by the air on its entrance is communicated to the vapour, which thus

becomes a temporary source of radiant heat, and diminishes the deflection produced in the first instance by its presence. The reverse occurs when the tube is exhausted; the vapour is chilled, its great absorptive action on the heat radiated from the adjacent face of the pile comes more into play, and the original effect is augmented. In both cases, however, the action is transient; the vapour soon loses the heat communicated to it, and soon gains the heat which it has lost, and matters then take their normal course.

§ 10. *On the Physical Connexion of Radiation, Absorption, and Conduction.*

Notwithstanding the great accessions of late years to our knowledge of the nature of heat, we are as yet, I believe, quite ignorant of the atomic conditions on which radiation, absorption, and conduction depend. What are the specific qualities which cause one body to radiate copiously and another feebly? Why, on theoretic grounds, must the equivalence of radiation and absorption exist? Why should a highly diathermanous body, as shown by Mr. BALFOUR STEWART, be a bad radiator, and an adiathermanous body a good radiator? How is heat conducted? and what is the strict physical meaning of good conduction and bad conduction? Why should good conductors be, in general, bad radiators, and bad conductors good radiators? These, and other questions, referring to facts more or less established, have still to receive their complete answers. It is less with a hope of furnishing such than of shadowing forth the possibility of uniting these various effects by a common bond, that I submit the following reflections to the notice of the Royal Society.

In the experiments recorded in the foregoing pages, we have dealt with *free* atoms, both simple and compound, and it has been found that in all cases absorption takes place. The meaning of this, according to the dynamical theory of heat, is that no atom is capable of existing in vibrating ether without accepting a portion of its motion. We may, if we wish, imagine a certain roughness of the surface of the atoms which enables the ether to *bite* them and carry the atom along with it. But no matter what the quality may be which enables any atom to accept motion from the agitated ether, the same quality must enable it to impart motion to still ether when it is plunged in the latter and agitated. It is only necessary to imagine the case of a body immersed in water to see that this must be the case. There is a polarity here as rigid as that of magnetism. From the existence of absorption, we may on theoretic grounds infallibly infer a capacity for radiation; from the existence of radiation, we may with equal certainty infer a capacity for absorption; and each of them must be regarded as the measure of the other*.

This reasoning, founded simply on the mechanical relations of the ether and the atoms inmersed in it, is completely verified by experiment. Great differences have been shown to exist among gases as to their powers of absorption, and precisely similar differences as regards their powers of radiation. But what specific property is it which

* This was written long before KIRCHHOFF's admirable papers on the relation of emission to absorption were known to me.

makes one free molecule a strong absorber, while another offers scarcely any impediment to the passage of radiant heat? I think the experiments throw some light upon this question. If we inspect the results above recorded, we shall find that the *elementary* gases hydrogen, oxygen, nitrogen, and the *mixture* atmospheric air, possess absorptive and radiative powers beyond comparison less than those of the *compound* gases. Uniting the atomic theory with the conception of an ether, this result appears to be exactly what ought to be expected. Taking DALTON'S idea of an elementary body as a single sphere, and supposing such a sphere to be set in motion in still ether, or placed without motion in moving ether, the communication of motion by the atom in the first instance, and the acceptance of it in the second, must be less than when a number of such atoms are grouped together and move as a system. Thus we see that hydrogen and nitrogen, which, when *mixed* together, produce a small effect, when *chemically united* to form ammonia, produce an enormous effect. Thus oxygen and hydrogen, which, when mixed in their electrolytic proportions, show a scarcely sensible action, when chemically combined to form aqueous vapour, exert a powerful action. So also with oxygen and nitrogen, which, when mixed, as in our atmosphere, both absorb and radiate feebly, when united to form oscillating systems, as in nitrous oxide, have their powers vastly augmented. Pure atmospheric air, of 5 inches tension, does not effect an absorption equivalent to more than the one-fifth of a degree, while nitrous oxide of the same tension effects an absorption equivalent to fifty-one such degrees. Hence the absorption by nitrous oxide at this tension is about 250 times that of air. No fact in chemistry carries the same conviction to my mind that air is a *mixture* and not a *compound*, as that just cited. In like manner, the absorption by carbonic oxide of this tension is nearly 100 times that of oxygen alone; the absorption by carbonic acid is about 150 times that of oxygen; while the absorption by olefiant gas of this tension is 1000 times that of its constituent hydrogen. Even the enormous action last mentioned is surpassed by the vapours of many of the volatile liquids, in which the atomic groups are known to attain their highest degree of complexity.

I have hitherto limited myself to the consideration that the compound molecules present broad sides to the ether, while the simple atoms with which we have operated do not; that in consequence of these differences the ether must swell into billows when the former are moved, while it merely trembles into ripples when the latter are agitated; that in the interception of motion also the former, other things being equal, must be far more influential than the latter. But another important consideration remains. All the gases and vapours, whose deportment we have examined, are transparent to light; that is to say, the waves of the visible spectrum pass among them without sensible absorption. Hence it is plain that their absorptive power depends on the periodicity of the undulations which strike them. At this point the present inquiry connects itself with the experiments of NIEPCE, the observation of FOUCAULT, the surmises of ÄNGSTROM, STOKES, and THOMSON, and those splendid researches of KIRCHHOFF and BUNSEN, which so immeasurably extend our experimental range. By KIRCHHOFF it has been conclusively

shown that every atom absorbs in a special degree those waves which are synchronous with its own periods of vibration. Now, besides presenting broader sides to the ether, the association of simple atoms to form groups must, as a general rule, render their motions through the ether more sluggish, and tend to bring the periods of oscillation into isochronism with the slow undulations of obscure heat, thus enabling the molecules to absorb more effectually such rays as have been made use of in our experiments.

Let me here state briefly the grounds which induce me to conclude that an agreement in period alone is not sufficient to cause powerful absorption and radiation; that in addition to this the molecules must be so constituted as to furnish *points d'appui* to the ether. The heat of contact is accepted with extreme freedom by rock-salt, but a plate of the substance once heated requires a great length of time to cool. This surprised me when I first noticed it. But the effect is explained by the experiments of Mr. Balfour Stewart, by which it is proved that the radiative power of heated rock-salt is extremely feeble. Periodicity can have no influence here, for the ether is capable of accepting and transmitting impulses of all periods; and the fact that rock-salt requires more time to cool than alum, simply proves that the molecules of the former glide through the ether with comparatively small resistance, and thus continue moving for a longer time; while those of the latter presenting broad sides to the ether, speedily communicate to it the motion which we call heat. This power of gliding through still ether, possessed by the rock-salt molecules, must of course enable the moving ether to glide round them, and no coincidence of period could, I think, make such a body a powerful absorber.

Many chemists, I believe, are disposed to reject the idea of an atom, and to adhere to that of equivalent proportions merely. They figure the act of combination as a kind of interpenetration of one substance by another. But this is a mere masking of the fundamental phenomenon. The value of the atomic theory consists in its furnishing the physical explanation of the law of equivalents;—assuming the one the other follows; and assuming the act of chemical union as Dalton figured it, we see that it blends harmoniously with the perfectly independent conception of an ether, and enables us to reduce the phenomena of radiation and absorption to the simplest mechanical principles.

Considerations similar to the above may, I think, be applied to the phenomena of *conduction*. In the Philosophical Transactions for 1853, I have described an instrument used in examining the transmission of heat through cubes of wood and other substances. When engaged with this instrument, I had also cubes of various crystals prepared, and determined with it their powers of conduction. With one exception, I found that the conductivity augmented with the diathermancy. The exception was furnished by a cube of very perfect rock-crystal, which conducted slightly better than my cube of rock-salt. The latter, however, had a very high conductive power; in fact rock-salt, calcareous spar, glass, selenite, and alum, stood in my experiments, as regards conductivity, exactly in their order of diathermancy in the experiments of Melloni. I have already adduced considerations which show that the molecules of rock-salt glide with

facility through the ether, but the ease of motion which these molecules enjoy must facilitate their mutual collision. Their motion, instead of being expended on the ether which exists between them, and communicated by it to the external ether, is in great part transferred directly from particle to particle, or in other words, is freely conducted. When a molecule of alum, on the contrary, approaches a neighbour molecule, it produces a swell in the intervening ether, which swell is in part transmitted, not to the molecules, but to the general ether of space, and thus lost as regards conduction. This lateral waste prevents the motion from penetrating the alum to any great extent, and the substance is what we call a bad conductor*.

Such considerations as these could hardly occur without carrying the mind to the kindred question of electric conduction; but the speculations have been pursued sufficiently far for the present, and must now abide the judgment of those competent to decide whether they are the mere emanations of fancy, or a fair application of principles which are acknowledged to be secure.

The present paper, I may remark, embraces only the first section of these researches.

* In the above considerations regarding conduction, I have limited myself to the illustration furnished by two compound bodies, but the elementary atoms also differ among themselves as regards their powers of accepting motion from the ether and of communicating motion to it. I should infer, for example, that the atoms of platinum encounter more resistance in moving through the ether than the atoms of silver. It is needless to say that the physical texture of a substance also has a great influence.

II. *On an Extension of* Arbogast's *Method of Derivations.*

By Arthur Cayley, *Esq., F.R.S.*

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Arbogast's Method of Derivations was devised by him with a view to the development of a function $\varphi(a+bx+cx^2+\ldots)$, but it is at least as useful for the formation of only the literal parts of the coefficients, or, what is the same thing, the combinations of a given degree and weight in the letters $(a, b, c, d, \ldots)$, the weights of the successive letters being 0, 1, 2, 3, &c. Thus instead of applying the method to finding the coefficients

$$a^4,\ 4a^3b,\ 4a^3c+6a^2b^2,\ \&c.,$$

we may apply it merely to finding the sets of terms

$$a^4,\ a^2b,\ a^2c,\ \&c.$$
$$a^2b^2$$

To derive any column from the one which immediately precedes it, we operate on a letter by changing it into its immediate successor in the alphabet, and we must in each term operate on the last letter, and also, when the last but one letter in the term is the immediate antecessor in the alphabet of the last letter (but in this case only), operate on the last but one letter. Thus a^2c gives a^2d, but a^2b^2 gives a^2bc and ab^3, and the next succeeding column is therefore

$$a^2d$$
$$a^2bc$$
$$ab^3.$$

If the series of letters is finite, and the last letter of the term is also the last letter of the series, then it is impossible to operate on the last letter of the term, but the last but one letter (when the foregoing rule applies to it) is still to be operated on; and if the rule does not apply, then the term does not give rise to a term in the succeeding column; the operations will at length terminate, and a complete series of columns be obtained. Thus, if the letters are (a, b, c, d), and the operations are (as before) performed upon a^4, the entire series of columns is

a^4	a^2b	a^2c a^2b^2	a^2d a^2bc ab^3	a^2bd a^2c^2 ab^2c b^4	a^2cd ab^2d abc^2 b^3c	a^2d^2 $abcd$ ac^3 b^3d b^2c^2	abd^2 ac^2d b^2cd bc^3	acd^2 b^2d^2 bc^2d c^4	ad^3 bcd^2 c^2d	bd^3 c^2d^2	cd^3	c^4

Nothing can be more convenient than the process when the entire series of columns is required; but it is very desirable to have a process for the formation of any column apart from the others; and the object of the present memoir is to investigate a rule for the purpose. But as ARBOGAST's rule, applied as above, depends upon very similar principles, I will commence by showing how this rule is to be demonstrated. If we take any combination b^3d and operate backwards on the last letter (viz. by changing it into its immediate antecessor in the alphabet), we obtain b^3c, which is a term in the next preceding column; b^3d is therefore obtainable from a term in the next preceding column, viz. b^3c, and the process is to operate on the last letter. If, instead of b^3d, the term is abc^3 (the last letter here entering as a power), the operation backwards on the last letter gives ab^3c, which is also a term of the preceding column; and it is to be noticed that the last but one letter b is here the immediate antecessor of the last letter c (and would have been so even if b had not entered into the given term abc^3, thus ac^3 operated on backwards would have given abc). Hence abc^3 is obtained by operating on a term in the next preceding column, viz. the term ab^3c, but in this case the operation is performed on the last but one letter. *Every* term is thus obtained from the next preceding column, viz. the terms are obtained by operating on the last letter, and (when the last but one letter is the immediate antecessor in the alphabet of the last letter) then also on the last but one letter, of each term of the next preceding column, and the correctness of the rule is thus demonstrated. It is to be observed that the terms are operated upon in order, the operation on the last but one letter (when it is operated on) being made immediately after that upon the last letter of the same term, and that the terms of a column are thus obtained in the proper alphabetical order.

I pass now to the above mentioned question of the formation of a single column by itself; it will be convenient, by way of illustration, to write down the columns

a^3e^3	ade^3
$abde^3$	bce^3
ac^3e^3	bd^3e^3
acd^3e	c^3de^3
ad^4	cd^3e
b^3ce^3	d^4
b^3d^3e	
bc^3de	
bcd^3	
c^4e	
c^3d^3	

which belong to the set (a, b, c, d, e), and are of the degree 5 and the weights 12 and 15 respectively.

Some definitions and explanations are required. I speak of the first and last letters *of the set*, simply as the first and the last letter: there is frequent occasion to speak of the last letter; and to avoid confusion, the last letter *of a term* will be spoken of as the ultimate letter; it is necessary also to consider the penultimate, antepenultimate, and pro-antepenultimate letters of the term. It will be convenient to distinguish between the ultimate letter and the ultimate, which may be either the ultimate letter or a power of such letter; and similarly for the penultimate, &c. Thus in the term bcd^3, the ultimate letter is d and the ultimate is d^3, the penultimate and the penultimate letter are each of them c; of course the ultimate, penultimate, &c. letters are always distinct from each other. We have also to consider the pairs of letters contained in a term; cd^3e contains the pairs (c, d), (c, e), (d, d), (d, e), and so in other cases; the letters of a pair are taken in the natural order. A pair not containing either the first letter or the last letter is *expansible*; thus the set being as before (a, b, c, d, e), the pairs (b, c), (d, d) are expansible: they are *expanded* by retreating the prior and advancing the posterior letter each one step; thus the just mentioned pairs (b, c), (d, d), are expanded into (a, d) and (c, e) respectively.

A pair composed of two distinct non-contiguous letters is *contractible*; it is *contracted* by advancing the prior and retreating the posterior letter each one step: thus (a, d), (c, e) are contractible pairs, and they give by contraction the pairs (b, c), (d, d) respectively: the processes of expansion and contraction are obviously converse to each other.

The expression the *last expansible pair* of a term hardly requires explanation; (d, d) is the last expansible pair of the term bcd^3, or of the term $b^3d^2e^3$, (c, d) the last expansible pair of the term c^3de (the set being always (a, b, c, d, e)), and so in all other cases. The expression the *last expansion*, in regard to any term, means the expansion of the last expansible pair of such term.

The expansion or contraction of any pair of a term leaves unaltered as well the weight as the degree, the resulting term belongs therefore to the same column as the original one. But the effect of an expansion is to diminish, and that of a contraction to increase the alphabetical rank, or rank in the column, the ranks being reckoned as the first or lowest, second, third, and so on, up to the last or highest rank. In particular, by performing upon any term the last expansion, we diminish the rank in the column, and by a succession of last expansions we bring the term up to the head of the column. Such term is not susceptible of any further expansion; it may therefore contain the first and last letters or either of them, and it may also contain a single intermediate letter in the first power only; thus the first and last letters being a, e, the head term of a column is of the form $a^m e^\mu$ or $a^m c e^\mu$, c being some intermediate letter, and the powers a^m, e^μ being both or either of them omittable. In like manner, by a succession of contractions of any term we obtain the term at the foot of the column; such term is not susceptible of any further contraction, and it must therefore be composed either of a single letter or of two contiguous letters, that is it must be of the form c^m,

or of the form $c^m d^n$, where c, d are any two contiguous letters, not excluding the case where the single letter or each or either of the contiguous letters is a first or a last letter.

It is to be observed that the passage up from any term to the term at the head of the column (or top term) by means of a succession of last expansions, is a perfectly unique one; but as no selection has been made of a like unique process of contraction, this is not the case with respect to the passage down from any term to the term at the foot of the column (or bottom term) by a succession of contractions.

Every term gives by the last expansion a term above it; it can therefore be obtained from such term above it by means of a contraction. But the contraction of the upper term is by hypothesis such that, performing upon the contracted term the last expansion, we obtain the upper term; a contraction, which, performed on any term, gives a lower term, which by performing upon it the last expansion reproduces the first mentioned or upper term, may be called *a reversible contraction*. And it is clear that if we perform on the top term all the reversible contractions, and on each of the resulting terms all the reversible contractions, and so on as long as the process is possible, we obtain without repetition all the terms of the column. The column is in fact similar to a genealogical tree in the male line, each lower term issuing from a single upper term, while each upper term generates a lower term or terms, or does not generate any such term, and the top term being the common origin of the entire series. It may be added that when the order of the reversible contractions of the same term is duly fixed, the alphabetical arrangement of the terms in the column agrees with the order as of primogeniture (an elder son and his issue male preceding all the younger sons and their respective issue male) in the genealogical tree.

It only remains then to inquire under what conditions a contraction is reversible. Now as regards any term, in order that a contraction performed on it may be reversible, it is necessary and sufficient that the pair produced by the contraction should be the last expansible pair of the contracted term. There are several cases to be considered. First, if the contraction affects the ultimate and penultimate letters of the term: this implies that the ultimate and penultimate letters are not contiguous. Let the term terminate in $e^m h$, the contracted term will terminate in $e^{m-1} fg$, and (f, g), the pair produced by the contraction, is the last expansible pair of the contracted term; the contraction is in this case reversible. If, however, the term terminate in $e^m h^p$ $(p>1)$, the contracted term will terminate in $e^{m-1} fgh^{p-1}$, and the last expansible pair is not as before (f, g), but it is (according as $p=2$ or $p>2$) (g, h) or (h, h): unless indeed h is the last letter, in which case (f, g) remains the last expansible pair of the contracted term. In the example e^m has been written, but the case $m=1$ is not excluded; moreover, the penultimate letter e is removed three steps from the ultimate letter h, but the result would have been the same if instead of e we had had any preceding letter, or had had the letter f; by hypothesis it is not g, the letter contiguous to the ultimate letter h. The

conclusion is that a contraction on the ultimate and penultimate letters (these being non-contiguous) is reversible if the ultimate is a simple letter, or if, being a power, it is a power of the last letter.

Next let the contraction affect the ultimate and antepenultimate letters. The two letters are here separated by the penultimate letter, and are therefore not contiguous. Suppose that the termination is $f^l g^m k$ (f and g contiguous), the contracted term terminates in $f^{l-1} g g^m j$, and (g,j) the pair arising from the contraction is the last expansible pair of the contracted term; the contraction is therefore reversible. In the example f^l has been written, but the case $l=1$ is not excluded; moreover the ultimate letter k is taken non-contiguous to the penultimate letter g; but if the ultimate letter had been the contiguous letter h, the only difference is that the pair would be (g, g), and the conclusion is not altered. But suppose the termination of the term is $e^l g^m k$ (e, g, non-contiguous), then the contracted term terminates in $e^{l-1} f g^m j$, where the pair arising from the contraction is (f,j), but the last expansible pair is (g,j); the contraction therefore is not reversible. The case $l=1$ is not excluded; nor is it necessary that the ultimate letter should be non-contiguous to the penultimate; if the ultimate letter had been h, the pair arising from the contraction would have been (f, g), but the last expansible pair (g, g), and the contraction is still not reversible. In each of the cases considered the ultimate has been a simple letter: if in the first case the ultimate had been $k^p (p>1)$, then the contracted pair would terminate in jk^{p-1}, and (according as $p=2$ or $p<2$) the last expansible pair would be (j, k) or (k, k), which is not the pair (g,j) produced by the contraction; the contraction is therefore not reversible, unless indeed k is the last letter, in which case it continues reversible. In the second case the contraction, which is not reversible when the ultimate is the simple letter k, remains not reversible when the ultimate is a power of such letter. The conclusion is that a contraction on the ultimate and antepenultimate letters is reversible, if the penultimate and antepenultimate letters are contiguous, and the ultimate is a simple letter, or if, being a power, it is a power of the last letter.

A contraction on the ultimate and pro-antepenultimate letters, or on the ultimate letter and any letter preceding the pro-antepenultimate letter, is never reversible. To show this, it will be sufficient to consider the case where the term terminates in $efgh$, the contracted term terminates in $ffgg$, the pair arising from the contraction being (f, g), and the last expansible pair being (g, g); and *à fortiori*, if the letters or any of them occur as powers, or are non-contiguous.

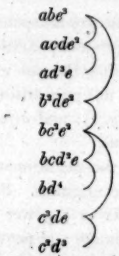
Consider, next, a contraction on the penultimate and antepenultimate letters; this assumes that these letters are non-contiguous. Such a contraction may be reversible if only the ultimate is the last letter or a power thereof; and the condition is then similar to that in the case of the ultimate and penultimate letters; only as the penultimate cannot be a power of the last letter, it must be a simple letter. And the conditions in order that the contraction may be reversible then are that the ultimate is the last letter or a power thereof, and the penultimate a simple letter.

The next case is that of a contraction on the penultimate and pro-antepenultimate letters. Such contraction may be reversible if the ultimate is the last letter or a power thereof; and the condition is then similar to that for the case of the ultimate and antepenultimate letters; only as the penultimate cannot be a power of the last letter, it must be a simple letter. The conditions, in order that the contraction may be reversible, are that the ultimate may be the last letter or a power thereof, the penultimate a simple letter, and the antepenultimate and pro-antepenultimate letters contiguous.

A contraction on the penultimate letter and on any letter preceding the pro-antepenultimate letter, or upon any two letters preceding the penultimate letter, is never reversible. If the ultimate, penultimate, antepenultimate and pro-antepenultimate letters are denoted by U, P, A, P′ respectively, then by what precedes, the following contractions, viz. UP, UA, PA, PP′, may be reversible, and they will be so under the conditions shown in the annexed Table. It is to be noticed that the conditions for UA, PA are mutually exclusive, and consequently that the number of reversible contractions to be performed upon any term is at most 3. The Table is

	P	A	P′
U	U, P, non-contiguous letters. The ultimate a simple letter, or a power of the last letter.	P, A, contiguous letters. The ultimate a simple letter, or a power of the last letter.	
P		P, A, non-contiguous letters. Penultimate a simple letter. Ultimate the last letter, or a power thereof.	A, P′, contiguous letters. Penultimate a simple letter. Ultimate the last letter, or a power thereof.

The contractions are to be applied in the order UP, UA, PA, PP′, but all the contracted terms originating in a prior contraction of a given term are to be exhausted before proceeding to a posterior contraction of the same term. As an example of the process, the set being (a, b, c, d, e), I will take the column



The top term abe^3 is given. The contractions applicable to it are UP, UA. And UP gives $acde^2$. The only contraction applicable to this is UA, giving ad^3e. And there is

not any contraction applicable to ad^2e. We revert therefore to UA on abe^3, this gives b^3de^3. The only applicable contraction is PA, giving bc^3e^3. The contractions applicable to this are UP, UA. And UP gives bcd^2e. The only contraction applicable to this is UA, giving bd^4, and there is not any contraction applicable to bd^4. We revert therefore to UA on bc^3e, this gives c^3de, and the only contraction applicable to this is UA, giving c^3d^2. There is not any contraction applicable to this, and the process is therefore complete. It may be remarked that the example presents no instance of the contraction PP′; in fact the only terms having a pro-antepenultimate are $acde^3$, in which A, P′ are not contiguous letters, and bcd^2e, in which the penultimate is not a simple letter, so that the contraction PP′ is in each case excluded. It must be confessed that a considerable amount of practice would be required before the process could be readily made use of.

III. *On the Equation for the Product of the Differences of all but one of the Roots of a given Equation.* By Arthur Cayley, *Esq.*, *F.R.S.*

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It is easy to see that for an equation of the order n, the product of the differences of all but one of the roots will be determined by an equation of the order n, the coefficients of which are alternately rational functions of the coefficients of the original equation, and rational functions multiplied by the square root of the discriminant. In fact, if the equation be $\phi v = (a, \ldots)(v, 1)^n = a(v-\alpha)(v-\beta)\ldots$, then putting for the moment $a=1$, and disregarding numerical factors, $\sqrt{\square}$, the square root of the discriminant, is equal to the product of the differences of the roots, and $\phi'\alpha$ is equal to $(\alpha-\beta)(\alpha-\gamma)\ldots$, consequently the product of the differences of the roots, all but α, is equal to $\sqrt{\square} \div \phi'\alpha$, and the expression $\frac{1}{\phi'\alpha}$ is the root of an equation of the order n, the coefficients of which are rational functions of the coefficients of the original equation. I propose in the present memoir to determine the equation in question for equations of the orders three, four, and five: the process employed is similar to that in my memoir "On the Equation of Differences for an Equation of any Order, and in particular for Equations of the Orders Two, Three, Four, and Five*," viz. the last coefficient of the given equation is put equal to zero, so that the given equation breaks up into $v=0$ and into an equation of the order $\overline{n-1}$ called the reduced equation; and this being so, the required equation breaks up into an equation of the order $\overline{n-1}$ (which however is not, as for the equation of differences, that which corresponds to the reduced equation) and into a linear equation; the equation of the order $\overline{n-1}$ is calculated by the method of symmetric functions; and combining it with the linear equation, which is known, we have the required equation, except as regards the terms involving the last coefficient, which terms are found by the consideration that the coefficients of the required equation are seminvariants. The solution leads immediately to that of a more general question; for if the product of the differences of all the roots except α, of the given equation

$$\phi v = (*)(v, 1)^n = a(v-\alpha)(v-\beta)\ldots = 0$$

(which product is a function of the degree $\overline{n-2}$ in regard to each of the roots $\beta, \gamma, \delta\ldots$), is multiplied by $(x-\alpha y)^{n-2}$, the function so obtained will be the root of an equation of the order n, the coefficients of which are covariants of the quantic $(*)(x, y)^n$, and these coefficients can be at once obtained by writing, in the place of the seminvariants of the former result, the covariants to which they respectively belong. In the case of the

* Philosophical Transactions, vol. cl. p. 112 (1860).

quintic equation, one of these covariants is, in regard to the coefficients, of the degree 6, which exceeds the limit of the tabulated covariants, the covariant in question has therefore to be now first calculated. The covariant equations for the cubic and the quartic might be deduced from the formulæ Nos. 119 and 142 of my Fifth memoir on Quantics*; they are in fact the bases of the methods which are there given for the solution of the cubic and the quartic equations respectively; and it was in this way that I was led to consider the problem which is here treated of.

1. The notation $\zeta(\alpha, \beta, \gamma..)$ is used (after Professor SYLVESTER) to denote the product of the squared differences of $(\alpha, \beta, \gamma..)$, and the notation $\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma..)$ to denote the product of the differences taken in a determinate order, viz.

$$\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma, \delta..)=(\alpha-\beta)(\alpha-\gamma)(\alpha-\delta)..$$
$$(\beta-\gamma)(\beta-\delta)..$$
$$(\gamma-\delta)..$$
$$..$$

2. The product of the differences of the roots of an equation depends, as already noticed, on the square root of the discriminant; and in order to fix the numerical factors and signs, it will be convenient, in regard to the equations

$$(a, b, c, d\,(x, 1)^3=0,$$
$$(a, b, c, d, e\,(x, 1)^4=0,$$
$$(a, b, c, d, e, f\,(x, 1)^5=0,$$

to write as follows:—

$$\zeta^{\frac{1}{2}}(\alpha, \beta)= \frac{1}{a}\sqrt{-(4ac-b^2)} \qquad =\frac{1}{a}\sqrt{-\Box},$$

$$\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma)= \frac{1}{a^2}\sqrt{-(27a^2d^2+4ac^3+..)} =\frac{1}{a^2}\sqrt{-\Box},$$

$$\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma, \delta)=-\frac{1}{a^3}\sqrt{256a^3e^3-27a^3d^4+..} =-\frac{1}{a^3}\sqrt{\Box},$$

$$\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma, \delta, \varepsilon)=-\frac{1}{a^4}\sqrt{3125a^4f^4+256a^3e^3+..}=-\frac{1}{a^4}\sqrt{\Box},$$

where it is to be observed, for example, that writing in the last equation $\varepsilon=0$, and therefore $f=0$, we have $\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma, \delta, 0)=-\frac{e}{a^4}\sqrt{256a^3e^3+..}$, which agrees with the equation $\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma, \delta, 0)=\alpha\beta\gamma\delta\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma, \delta)=\frac{e}{a}\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma, \delta)$, if for $\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma, \delta)$ we substitute the value given by the last equation but one.

For the cubic equation $(a, b, c, d\,(x, 1)^3=0$;

3. We have to find the equation for $\theta=\zeta^{\frac{1}{2}}(\alpha, \beta)=\alpha-\beta$; the roots are

$$\theta_1=\beta-\gamma, \quad \theta_2=\gamma-\alpha, \quad \theta_3=\alpha-\beta.$$

* Philosophical Transactions, vol. cxlviii. pp. 415–427 (1858).

To apply the method above explained, write $\gamma=0$, and therefore also $d=0$; the roots thus become

$$\theta_1=\beta, \quad \theta_2=-\alpha, \quad \theta_3=\alpha-\beta,$$

and we have the quadric and linear equations

$$(\theta+\alpha)(\theta-\beta)=0, \quad \theta-(\alpha-\beta)=0,$$

where (α, β) are the roots of the equation

$$(a, b, c)(v, 1)^2=0.$$

Hence, writing

$$Z=4ac-b^2,$$

we have

$$\alpha-\beta=\frac{1}{a}\sqrt{-Z},$$

and the two equations become

$$\theta^2 a+\theta\sqrt{-Z}-c=0, \quad \theta a-\sqrt{-Z}=0;$$

or multiplying the two equations together,

$$\theta^3 a^2+\theta^2 0+\theta(3ac-b^2)+c\sqrt{-Z}=0,$$

which is what the required equation becomes, on putting therein $d=0$; the coefficients of the complete equation are seminvariants, and the terms in d are to be inserted by means of this property. The coefficient $3ac-b^2$ is reduced to zero by the operator

$$3a\partial_b+2b\partial_c+c\partial_d,$$

it is therefore a seminvariant, and remains unaltered. The coefficient $c\sqrt{-Z}$ is what $\sqrt{-\square}$ becomes ($\square$ being the discriminant of the cubic equation) on putting therein $d=0$, it is therefore to be changed into $\sqrt{-\square}$. Hence

4. For the cubic equation $(a, b, c, d)(v, 1)^3$ the equation for $\theta\left(=\zeta^{\frac{1}{2}}(\alpha, \beta)\right)$ is $0=$

$$\left(\begin{array}{c|c|c|c} a^2\times & & & \sqrt{-\square}\times \\ +1 & 0 & \begin{matrix}+3 & ac\\ -1 & b^2\end{matrix} & +1 \end{array}\right)(\theta, 1)^3.$$

For the quartic equation $(a, b, c, d, e)(v, 1)^4=0$;

5. Here

$$\theta=-\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma)=-(\alpha-\beta)(\alpha-\gamma)(\beta-\gamma),$$

the roots are

$$\begin{aligned}\theta_1&= \ \zeta^{\frac{1}{2}}(\beta, \gamma, \delta),\\ \theta_2&=-\zeta^{\frac{1}{2}}(\gamma, \delta, \alpha),\\ \theta_3&= \ \zeta^{\frac{1}{2}}(\delta, \alpha, \beta),\\ \theta_4&=-\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma),\end{aligned}$$

the signs being in this case (and indeed for an equation of any even order) alternately positive and negative; in fact, if the equation is represented by $\varphi v=0$, then the roots divided by $\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma, \delta)$ should respectively be $\varphi'\alpha$, $\varphi'\beta$, $\varphi'\gamma$, $\varphi'\delta$, and this will be the case if the signs are taken as above.

6. Putting now $\delta=0$ (and therefore $e=0$) the roots become

$$\theta_1=\beta\gamma(\beta-\gamma),$$
$$\theta_2=\gamma\alpha(\gamma-\alpha),$$
$$\theta_3=\alpha\beta(\alpha-\beta),$$
$$\theta_4=-\zeta_0^4(\alpha,\beta,\gamma),$$

where (α, β, γ) are the roots of $(a, b, c, d)(v, 1)^3=0$. Let Z denote the discriminant of the cubic function, then $\zeta_0^4(\alpha, \beta, \gamma)=\frac{1}{a^2}\sqrt{-Z}$, and we have thus the linear equation; the cubic equation is

$$\Pi_3\{\theta-\beta\gamma(\beta-\gamma)\}=0,$$

the coefficients of which can be calculated by the method of symmetric functions (see Annex No. 1).

7. The cubic equation being thus obtained, we have the two equations

$\theta^3.a^4$	$\theta.a^3$
$+\theta^2.-a^2\sqrt{-Z}$	$+\sqrt{-Z}$
$+\theta.-9a^2d^2+4abcd-b^3d$	
$+\quad d^2\sqrt{-Z}$	
$=0$	$=0;$

and multiplying these together, the resultant equation is

$$\theta^4.a^5$$
$$+\theta^3.0$$
$$+\theta^2.a^2(-9a^2d^2+4abcd-b^3d+Z)$$
$$+\theta.\ (-8a^3d+4abc-b^2)d\sqrt{-Z}$$
$$-d^2Z=0,$$

where the coefficients have to be completed by adding the terms which contain e. We have $\sqrt{\square}$ in the place of $d\sqrt{-Z}$, and $\square$ in the place of $-d^2Z$. The coefficient $-8a^3d+4abc-b^3$ is a seminvariant, and requires no alteration. The coefficient

$$-\ 9a^2d^2+\ 4abcd-\ b^3d+Z$$

is

$$-\ 9a^2d^2+\ 4abcd\qquad\qquad -b^3d$$
$$+27a^3d^2-18abcd+4ac^3+4b^3d-b^2c^2;$$

that is,

$$+18a^2d^2$$
$$-14abcd$$
$$+\ 4ac^3$$
$$+\ 3b^3d$$
$$-\ 1b^2c^2,$$

and the terms in e to be added to this, in order to make it a seminvariant, are easily found to be

$$-16a^3ce$$
$$+\ 6ab^3e.$$

8. Hence, for the quartic equation $(a, b, c, d)(x, 1)^4$, the equation for $\theta(=\zeta^4(\alpha, \beta, \gamma))$ is $0=$

$a^4\times$	$a^3\sqrt{\square}\times$	$a^3\times$	$\sqrt{\square}\times$	$\square\times$
$+1\ a^3$	$0\ a$	$-16\ a^3ce$ $+18\ a^3d^2$ $+\ 6\ ab^3e$ $-14\ abcd$ $+\ 4\ ac^3$ $+\ 3\ b^3d$ $-\ 1\ b^3c^2$	$-8\ a^3d$ $+4\ abc$ $-1\ b^3$	$+\ 1$

$(\qquad\qquad)(x, 1)^4.$

For the quintic equation $(a, b, c, d, e, f)(x, 1)^5=0$;

9. We have $\theta=\zeta^4(\alpha, \beta, \gamma, \delta)$, the roots being

$$\theta_1=\zeta^4(\beta, \gamma, \delta, \varepsilon), \quad \text{which for } \varepsilon=0 \text{ becomes } \beta\gamma\delta\zeta^4(\beta, \gamma, \delta),$$
$$\theta_2=\zeta^4(\gamma, \delta, \varepsilon, \alpha), \quad \text{,,} \quad \text{,,} \quad -\gamma\delta\alpha\zeta^4(\gamma, \delta, \alpha),$$
$$\theta_3=\zeta^4(\delta, \varepsilon, \alpha, \beta), \quad \text{,,} \quad \text{,,} \quad \delta\alpha\beta\zeta^4(\delta, \alpha, \beta),$$
$$\theta_4=\zeta^4(\varepsilon, \alpha, \beta, \gamma), \quad \text{,,} \quad \text{,,} \quad -\alpha\beta\gamma\zeta^4(\alpha, \beta, \gamma),$$
$$\theta_5=\zeta^4(\alpha, \beta, \gamma, \delta), \quad \text{,,} \quad \text{,,} \quad \zeta^4(\alpha, \beta, \gamma, \delta).$$

10. The linear equation is $\theta a^3+\sqrt{Z}=0$; the quartic equation may be written $\Pi_4(\theta-\theta_1)=0$, for the determination of which see Annex No. 2. The two equations are

$\theta^4\ .\ a^9$	$\theta\ .\ a^3$
$+\theta^3-a^6\sqrt{Z}$	$+\ \sqrt{Z}$
$+\theta^2\ .\ a^3Me$	
$+\theta\ .\ Ne^3\sqrt{Z}$	
$+\quad e^3Z$	
$=0$	$=0;$

and multiplying these together, the resulting equation is

$$\theta^5\ .\ a^{12}$$
$$+\theta^4\ .\ 0$$
$$+\theta^3\ .\ a^6(Me-Z)$$
$$+\theta^2\ .\ a^3(Ne+M)e\sqrt{Z}$$
$$+\theta\ .\ \ (N+a^3e)e^3Z$$
$$+\qquad e^3Z\sqrt{Z}=0,$$

where the coefficients have to be completed by the addition of the terms in f. We have $\sqrt{\square}$ in the place of $e\sqrt{Z}$, and therefore $\square$ in the place of e^2Z.

11. The value of $Me-Z$ is

$+96-256=$	$-160\ a^3e^3$
$-60+192=$	$+132\ a^2bde^2$
$-40+128=$	$+88\ a^2c^2e^2$
$+27-144=$	$-117\ a^2cd^2e$
$+27=$	$+27\ a^2d^4$
$+47-144=$	$-97\ ab^2ce^2$
$+6=$	$+6\ ab^2d^2e$
$-18+80=$	$+62\ abc^2de$
$-18=$	$-18\ abcd^3$
$+4-16=$	$-12\ ac^4e$
$+4=$	$+4\ ac^3d^2$
$-9+27=$	$+18\ b^4e^2$
$+4-18=$	$-14\ b^3cde$
$+4=$	$+4\ b^3d^2$
$-1+4=$	$+3\ b^2c^2e$
$-1=$	$-1\ b^2c^2d^2$

and the terms in f are found to be

$+300$	a^3def
-130	a^2bcef
-120	a^2bd^2f
$+40$	a^2c^2df
$+28$	ab^2cef
$+66$	ab^2cdf
-24	abc^3f
-16	b^4df
$+6$	b^3c^2f
-125	a^2cf^2
$+50$	$a^2b^2f^2$

12. The value of $Ne+M$ is

$-16+96=$	$+80\ a^3e^2$
$+6-60=$	$-54\ a^2bde$
$+4-40=$	$-36\ a^2c^2e$
$+27=$	$+27\ a^2cd^2$
$-5+47=$	$+42\ ab^2ce$
$-18=$	$-18\ abc^2d$
$+4=$	$+4\ ac^4$
$+1-9=$	$-8\ b^4e$
$+4=$	$+4\ b^3cd$
$-1=$	$-1\ b^2c^2$

and the terms in f to be added thereto are found to be

-50	a^2df
$+30$	a^2bef
-8	ab^2f

13. The value of $N+a^2e$ is

$$\begin{array}{|r|l|}\hline -15 & a^3e \\ +6 & a^2bd \\ +4 & a^2c^3 \\ -5 & ab^2c \\ +1 & b^4 \\ \hline\end{array}$$

which is a seminvariant, and requires no addition.

14. Hence, for the quintic equation

$$(a, b, c, d, e, f)(v, 1)^5=0,$$

the equation for $\theta\left(=\zeta^4(\alpha, \beta, \gamma, \delta)\right)$ is $0=$

$a^{12}\times$		$a^8\times$	$a^3\sqrt{\square}\times$	$\square\times$	$\square\sqrt{\square}\times$
$+1$	0	$-125\ a^2cf^3$	$-50\ a^3df$	$-15\ a^2e$	$+\ 1$
		$+300\ a^2def$	$+80\ a^3e^2$	$+\ 6\ a^2bd$	
		$-160\ a^2e^3$	$+30\ a^2bcf$	$+\ 4\ a^2c^3$	
		$+\ 50\ a^2b^2f^2$	$-54\ a^2bde$	$-\ 5\ ab^2c$	
		$+132\ a^2bde^2$	$-36\ a^2c^2e$	$+\ 1\ b^4$	
		$-130\ a^2bcef$	$+27\ a^2cd^2$		
		$-120\ a^2bd^2f$	$-\ 8\ ab^3f$		
		$+\ 40\ a^2c^2df$	$+42\ ab^2ce$		
		$+\ 88\ a^2c^3e^2$	$-18\ abc^2d$		
		$-117\ a^2cd^2e$	$+\ 4\ ac^4$		
		$+\ 27\ a^2d^4$	$-\ 8\ b^3e$		
		$+\ 28\ ab^2ef$	$+\ 4\ b^2cd$		
		$-\ 97\ ab^2ce^2$	$-\ 1\ b^2c^2$		
		$+\ 6\ ab^2d^2e$			
		$+\ 62\ abc^2de$			
		$+\ 66\ ab^2cdf$			
		$-\ 18\ abcd^3$			
		$-\ 24\ abc^3f$			
		$-\ 12\ ac^4e$			
		$+\ 4\ ac^3d^2$			
		$-\ 16\ b^4df$			
		$+\ 18\ b^4e^2$			
		$-\ 14\ b^3cde$			
		$+\ 4\ b^3d^3$			
		$+\ 6\ b^3c^2f$			
		$+\ 3\ b^3c^2e$			
		$-\ 1\ b^3c^3d^2$			

$$(\qquad\qquad\qquad

The equation in θ is thus $(\theta-6)^3(\theta+12)(\theta^2+108)=0$, or multiplying out it is

$$(1, 0, 0, +432, -11664, +466567(\theta, 1)^4=0,$$

which in fact (observing that $\sqrt{\square}=36$) is what the preceding formula becomes for the equation $(1, 1, 1, 1, 1, 1\mathcal{X}v, 1)^5=0$.

The analogous verifications for the cubic and the quartic equations are as follows;

16. For the cubic, if the assumed equation is $v^3+v^2+v+1=0$, the roots whereof are $-1, i, -i$ ($i^2=-1$), then we should have $\zeta^4(\alpha, \beta, \gamma)=\sqrt{-\square}=4i$, which will be the case if $\alpha, \beta, \gamma=-1, i, -i$, respectively, and the roots $\beta-\gamma, \gamma-\alpha, \alpha-\beta$ of the equation in θ then are $2i, -i+1, -i-1$, so that the equation in θ is $(\theta^2+2i\theta-2)(\theta-2i)=0$, or

$$(1, 0, 2, 4i\ \mathcal{X}\theta, 1)^3=0,$$

which (observing that $\sqrt{\square}=4i$) is what the formula for the equation in θ becomes for the equation $(1, 1, 1, 1\mathcal{X}v, 1)^3=0$.

17. For the quartic equation, taking this to be $v^4+v^3+v^2+v+1=0$, the roots are $\omega, \omega^2, \omega^3, \omega^4$, where ω is an imaginary fifth root of unity ($\omega^4+\omega^3+\omega^2+\omega+1=0$), and putting $\alpha, \beta, \gamma, \delta$ equal to $\omega, \omega^2, \omega^3, \omega^4$ respectively, we have

$$-\sqrt{\square}=\zeta^4(\alpha, \beta, \gamma, \delta)=-5(\omega+\omega^4-\omega^2-\omega^3),$$

giving, as it should do, $\square=125$. The equation in θ is therefore by the formula

$$(1, 0, 0, -25\overline{\omega+\omega^4-\omega^2-\omega^3}, 125\mathcal{X}\theta, 1)^4=0.$$

But the roots are

$$\theta_1=\ \zeta^4(\beta, \gamma, \delta)=\ (\omega^2-\omega^3)(\omega^3-\omega^4)(\omega^2-\omega^4)=\ 2-\omega+\omega^2-2\omega^3=\ 2-\mathrm{X},$$

$$\theta_2=-\zeta^4(\gamma, \delta, \alpha)=-(\omega^3-\omega^4)(\omega^2-\omega)(\omega^4-\omega)=-1+3\omega+2\omega^2+\omega^3=-1+\mathrm{Y},$$

$$\theta_3=\ \zeta^4(\delta, \alpha, \beta)=\ (\omega^4-\omega)(\omega^4-\omega^2)(\omega-\omega^2)=-4-3\omega-2\omega^2-\omega^3=-4-\mathrm{Y},$$

$$\theta^4=-\zeta^4(\alpha, \beta, \gamma)=-(\omega-\omega^2)(\omega-\omega^3)(\omega^2-\omega^3)=\ 3+\omega-\omega^2+2\omega^3=\ 3+\mathrm{X},$$

if, for shortness,

$$\mathrm{X}=\omega-\omega^2+2\omega^3, \quad \mathrm{Y}=3\omega+2\omega^2+\omega^3.$$

The equation in θ is therefore

$$(\theta-2+\mathrm{X})(\theta+1-\mathrm{Y})(\theta+4+\mathrm{Y})(\theta-3-\mathrm{X})=0,$$

where the left-hand side is the product of the factors

$$(\theta-2+\mathrm{X})(\theta-3-\mathrm{X})=\theta^2-5\theta+6-\ \mathrm{X}-\mathrm{X}^2=\theta^2-5\theta+10-5(\omega+\omega^4)$$

and

$$(\theta+1-\mathrm{Y})(\theta+4+\mathrm{Y})=\theta^2+5\theta+4-3\mathrm{Y}-\mathrm{Y}^2=\theta^2+5\theta+10-5(\omega^2+\omega^3);$$

and the equation in θ is, therefore, as it should be,

$$(1, 0, 0, -25\overline{\omega+\omega^4-\omega^2-\omega^3}, 125\mathcal{X}\theta, 1)^4=0.$$

Passing from the denumerate to the standard forms;

18. For the cubic equation $(a, b, c, d)(v, 1)^3=0$, the equation for $\theta\left(=\zeta^1(\beta, \gamma)\right)$ is $0=$

$a^2\times$		$9\times$	$\sqrt{-27\,\square}\times$
$+1$	0	$+1\ ac$ $-1\ b^2$	$+1$

$(\theta, 1)^3$.

19. For the quartic equation $(a, b, c, d, e)(v, 1)^4=0$, the equation for $\theta\left(=\zeta^1(\alpha, \beta, \gamma)\right)$ is $0=$

$a^4\times$		$96a^2\times$	$512\sqrt{\square}\times$	$256\,\square\times$
$+1$	0	$-1\ a^2ce$ $+3\ a^2d^2$ $+1\ ab^2e$ $-14\ abcd$ $+9\ ac^3$ $+8\ b^2d$ $-6\ b^2c^2$	$-1\ a^2d$ $+3\ abc$ $-2\ b^3$	$+1$

$(\theta, 1)^4$.

20. For the quintic equation $(a, b, c, d, e, f)(v, 1)^5=0$, the equation for $\theta\left(=\zeta^1(\alpha, \beta, \gamma, \delta)\right)$ is $0=$

$a^{12}\times$		$625\ a^4\times$	$12500\sqrt{\square}\,a^2\times$	$15625\,\square\times$	$76125\,\square\sqrt{\square}$
$+1$	0	$-2\ a^3cf^2$ $+24\ a^3def$ $-32\ a^3e^3$ $+2\ a^2b^2f^2$ $+264\ a^2bde^2$ $-52\ a^2bcef$ $-96\ a^2bd^2f$ $+64\ a^2c^2df$ $+352\ a^2c^2e^2$ $-936\ a^2cd^2e$ $+432\ a^2d^4$ $+28\ ab^3ef$ $-970\ ab^2ce^2$ $+120\ ab^2d^2e$ $+264\ ab^2cdf$ $+2480\ abc^2de$ $-1440\ abcd^3$ $-192\ abc^3f$ $-960\ ac^4e$ $+640\ ac^3d^2$ $-160\ b^4df$ $+450\ b^4e^2$ $-1400\ b^3cde$ $+800\ b^3d^3$ $+120\ b^3c^2f$ $+600\ b^3c^3e$ $-400\ b^3c^3d^2$	$-1\ a^3df$ $+4\ a^3e^2$ $+3\ a^2bcf$ $-27\ a^2bde$ $-36\ a^2c^2e$ $+54\ a^2cd^2$ $-2\ ab^2f$ $+105\ ab^2ce$ $-180\ abc^2d$ $+80\ ac^4$ $-50\ b^4e$ $+100\ b^3cd$ $-50\ b^3c^3$	$-3\ a^2e$ $+12\ a^2bd$ $+16\ a^2c^2$ $-50\ ab^2c$ $+25\ b^4$	$+1$

$(\theta\,1)^5$.

21. I remark, with respect to the equation in θ, for the cubic, that it leads at once to the equation of differences. In fact we have

$$a^2\theta^2+9(ac-b^2)\theta+\sqrt{-27\,\square}=\Pi_3\{\theta-(\alpha-\beta)\},$$

whence changing the sign of θ,

$$a^2\theta^2+9(ac-b^2)\theta-\sqrt{-27\,\square}=\Pi_3\{\theta+(\alpha-\beta)\};$$

or multiplying the two equations and putting u for θ^2,

$$u\{a^2u+9(ac-b^2)\}^2+27\,\square=\Pi_3\{u-(\alpha-\beta)^2\},$$

that is, the equation of differences is

$$a^4u^3+18(ac-b^2)a^2u^2+81(ac-b^2)^2u+27\,\square=0;$$

but this mode of composition of the equation of differences is peculiar to the case of the cubic.

If in the several equations in θ we substitute for the seminvariants the covariants to which they respectively belong, we obtain as follows:—

22. For the cubic equation $(a, b, c, d\,(x, 1)^3=0$, the equation for $\left(\Theta=(\beta-\gamma)(x-\alpha y)\right)$ is

$$\left(\begin{array}{cccc} U^2\times & & 9\times & \sqrt{-27}\,\square\times \\ +1 & 0 & H & +1 \end{array}\right)(\Theta, 1)^3=0.$$

23. For the quartic equation $(a, b, c, d, e\,(x, 1)^4=0$, the equation for

$$\Theta\left(=(\beta-\gamma)(\gamma-\delta)(\delta-\beta)(x-\alpha y)^3\right) \text{ is}$$

$$\left(\begin{array}{cccc} U^3\times & & 96U^2\times & 512\sqrt{\square}\times & 256\,\square\times \\ +1 & & \begin{array}{c}-3JU \\ +2IH\end{array} & -\Phi & +1 \end{array}\right)(\Theta, 1)^4=0.$$

24. And for the quintic equation $(a, b, c, d, e, f\,(x, 1)^5=0$, the equation for

$$\Theta\left(=(\beta-\gamma)(\beta-\delta)(\beta-\epsilon)(\gamma-\delta)(\gamma-\epsilon)(\delta-\epsilon)(x-\alpha y)^5\right) \text{ is}$$

$$\left\{\begin{array}{l} U^5, \\ 0\ , \\ 625U^6(A, B, C, D, E, F, G\,(x, y)^6, \\ 12500\sqrt{\square}\,U^3\{4U(\text{No. }14)^3-U(\text{No. }20)-50(\text{No. }15)(\text{No. }16)\}, \\ 15625\ \square\ \{-3U^3(\text{No. }14)+25(\text{No. }15)^3\}, \\ 76125\ \square\sqrt{\square} \end{array}\right\}(\Theta, 1)^5=0,$$

where the covariant which enters into the coefficient of Θ^3 being of the sixth degree in the coefficients, is not given in the Tables.

Its value (completed for me, from the first term, by Mr. Davis) is

$-2\ a^2cf^2$	$+18\ a^3df^2$	$-30\ a^2ef^2$	$-10\ a^2f^3$	$-30\ a^2bf^3$	$+18\ a^2cf^3$	$-2\ a^2df^3$
$+24\ a^3def$	$-48\ a^2ef$	$+210\ a^2bdf^2$	$+30\ a^2bef^2$	$+210\ a^2cef^2$	$-66\ a^2def^2$	$+2\ a^2e^2f^2$
$-32\ a^4e^2$	$-66\ a^2bef^2$	$+60\ a^2be^2f$	$+680\ a^2cdf^2$	$+180\ a^2d^2f^2$	$+48\ a^2e^2f$	$+24\ abcf^3$
$+2\ a^2b^2f^2$	$+348\ a^2bdef$	$+180\ a^2c^2f^2$	$-320\ a^2ce^2f$	$-840\ a^2def^2$	$-48\ ab^2f^3$	$-52\ abdef^2$
$+264\ a^2bde^2$	$+48\ a^2be^2$	$+120\ a^2cdef$	$-1320\ a^2d^2ef$	$+480\ a^2e^4$	$+348\ abcef^2$	$+28\ abe^2f$
$-52\ a^2bcef$	$+624\ a^2c^2ef$	$-480\ a^2ce^3$	$+960\ a^2de^3$	$+60\ ab^2ef^3$	$+624\ abd^2f^3$	$-96\ ac^2ef^2$
$-96\ a^2bd^2f$	$-936\ a^2cdef$	$-1080\ a^2d^2f$	$-320\ ab^2df^3$	$+120\ abcdf^2$	$+720\ abe^4$	$+64\ acd^2f^2$
$+64\ a^2c^2df$	$-576\ a^2cde^2$	$+1080\ a^2d^2e^2$	$+290\ ab^2e^2f$	$-540\ abcef$	$-1596\ abde^2f$	$+264\ acde^2f$
$+352\ a^2c^3e^2$	$+648\ a^2d^2e$	$-840\ ab^2cf^3$	$-1320\ abc^2f^3$	$+960\ abd^2ef$	$-936\ ac^3df^2$	$-160\ ace^4$
$-936\ a^2cd^2e$	$+48\ ab^2f^3$	$-540\ ab^2def$	$+4160\ abcdef$	$-600\ abde^3$	$-48\ ac^3e^2f$	$-192\ ad^3ef$
$+432\ a^2d^4$	$-1596\ ab^2cef$	$+450\ ab^2e^3$	$-3200\ abce^3$	$-1080\ ac^2f^2$	$+3504\ acd^2ef$	$+120\ ad^2e^2$
$+28\ ab^2ef$	$+210\ ab^2de^2$	$+960\ abc^2ef$	$+960\ abd^3f$	$+4560\ ac^2def$	$-1728\ ad^4f$	$-32\ b^2f^3$
$-970\ ab^2ce^2$	$-48\ ab^2d^2f$	$+4560\ abcd^2f$	$-600\ abd^2e^2$	$-2400\ ac^2e^3$	$-1920\ acde^2$	$+264\ b^2cef^2$
$+120\ ab^2d^2e$	$+3504\ abc^2df$	$-4200\ abcde^2$	$+960\ ac^3ef$	$-2880\ acd^2f$	$+1080\ ad^2e^2$	$+352\ b^2d^2f^2$
$+264\ ab^2cdf$	$+720\ abc^2e^2$	$-2880\ ac^3df$	$-2560\ ac^2d^2f$	$+1800\ acd^2e^2$	$+48\ b^2ef^2$	$-970\ b^2de^2f$
$+2480\ abc^2de$	$-2160\ abcd^2e$	$+2400\ ac^3e^2$	$+1600\ ac^2de^2$	$-480\ b^2df^2$	$-576\ b^2cdf^2$	$+450\ b^2e^4$
$-192\ abc^3f$	$-1728\ ac^4f$	$+480\ b^2f^2$	$+960\ bc^3f^2$	$+450\ b^2e^2f$	$+210\ b^2ce^2f$	$-936\ bc^2df^2$
$-1440\ abcd^3$	$+960\ ac^3de$	$-600\ b^2cef$	$-3200\ bc^2def$	$+1080\ b^2c^2f^2$	$+720\ b^2d^2ef$	$+2480\ bcd^2ef$
$-960\ ac^4e$	$+720\ b^2ef$	$-2400\ b^2d^2f$	$+2250\ b^2e^3$	$-4200\ b^2cdef$	$-450\ b^2de^3$	$+120\ bc^2e^2f$
$+640\ ac^2d^2$	$-1920\ b^2cdf$	$+2250\ b^2de^2$	$-600\ b^2c^2ef$	$+2250\ b^2ce^4$	$+648\ bc^3f^2$	$-1400\ bcde^3$
$-160\ b^4df$	$-450\ b^2ce^2$	$+1800\ b^2c^2df$	$+1600\ bc^2d^2f$	$+2400\ b^2d^2f$	$-2160\ bc^2def$	$-960\ bd^4f$
$+450\ b^4e^2$	$+1200\ b^2d^2e$	$-1500\ b^2c^3e^2$	$-1000\ b^2cde^2$	$-1500\ b^2d^2e^2$	$+1200\ bc^2e^3$	$+600\ bd^3e^2$
$-1400\ b^3cde$	$+1080\ b^2c^2f$				$+960\ bcd^2ef$	$+432\ c^4f^2$
$+120\ b^3c^2f$	$-600\ b^2c^2de$				$-600\ bcd^2e^2$	$-1440\ c^3def$
$+800\ b^3d^3$						$+800\ c^2e^3$
$+600\ b^3c^2e$						$+640\ c^2d^3f$
$-400\ b^3c^2d^2$						$-400\ c^2d^2e^2$

($\chi x, y^6$.

In the following two Annexes, the notation of the symmetric functions is the same as in my "Memoir on the Symmetric Functions of the Roots of an Equation*," and the values of the symmetric functions are taken from that memoir, the powers of a being restored by the principle of homogeneity. The suffixes of the Σ indicate the number of terms in the sum; thus in the first Annex $\Sigma_3\gamma(\beta-\gamma)(\gamma-\alpha)=\Sigma_3(\beta\gamma^3-\alpha\beta\gamma-\gamma^3+\alpha\gamma^3)$; the terms $\Sigma_3(\beta\gamma^3+\alpha\gamma^3)$ are equal to $\Sigma_6\alpha^3\beta$, the complete symmetric function; the correct result will be obtained (though of course neither of these equations is true) by writing $\Sigma_3\beta\gamma^3=\frac{1}{2}\Sigma_6\alpha^3\beta$, $\Sigma_3\alpha\gamma^3=\frac{1}{2}\Sigma_6\alpha^3\beta$, and so in similar cases; the insertion of the suffix to the Σ very much facilitates the calculation, and is a check on its accuracy.

Annex No. 1, containing the calculation of the equation $\Pi_3(\theta-\theta_1)=0$, where

$$\theta_1=\beta\gamma(\beta-\gamma),\ \theta_2=\gamma\alpha(\gamma-\alpha),\ \theta_3=\alpha\beta(\alpha-\beta),$$

α, β, γ being the roots of the cubic equation $(a, b, c, d\chi(v, 1)^3=0$.

We have

$$\Sigma_3\theta_1=\Sigma\beta\gamma(\beta-\gamma)=-(\alpha-\beta)(\beta-\gamma)(\gamma-\alpha)=\zeta^3(\alpha, \beta, \gamma)=\frac{1}{a^3}\sqrt{-Z},$$

where $Z=27a^2d^2+$ &c. is the discriminant of the cubic.

$$\Sigma_3\theta_1\theta_2=\Sigma_3\beta\gamma(\beta-\gamma)\gamma\alpha(\gamma-\alpha)=\alpha\beta\gamma\Sigma_3\gamma(\beta-\gamma)(\gamma-\alpha),$$

where $\alpha\beta\gamma=-\frac{d}{a}$ and

$$\Sigma_3\gamma(\beta-\gamma)(\gamma-\alpha)=\Sigma_3(\beta\gamma^3-\alpha\beta\gamma-\gamma^3+\alpha\gamma^3)$$

* Philosophical Transactions, vol. cxlvii. (1857) pp. 415–456.

$$\begin{array}{rrl} =-\Sigma_\alpha\alpha^3 & =-\ (\ 3) & =\frac{1}{a^3}\left|\begin{array}{l} 3a^3d-3abc+1b^3 \\ +3a^3d-1abc \\ +3a^3d \end{array}\right. \\ +\Sigma_\alpha\alpha^2\beta & +\ (21) & \\ -3\alpha\beta\gamma & -3(13) & \end{array}$$

$$=\frac{1}{a^3}(9a^3d-4abc+1b^3),$$

and therefore

$$\Sigma_3\theta_1\theta_2=-\frac{1}{a^3}(9a^3d^2-4abcd+1b^3d).$$

And lastly,

$$\begin{aligned} \Sigma_1\theta_1\theta_2\theta_3, \text{ or } \theta_1\theta_2\theta_3 &= \alpha^3\beta^3\gamma^3(\alpha-\beta)(\beta-\gamma)(\gamma-\alpha) \\ &= -\alpha^3\beta^3\gamma^3\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma) \\ &= -\frac{d^2}{a^3}\sqrt{-Z}. \end{aligned}$$

So that the equation is the one given above, No. 7.

Annex No. 2, containing the calculation of the equation $\Pi_1(\theta-\theta_1)=0$, where $\theta_1=\beta\gamma\delta\zeta^{\frac{1}{2}}(\beta, \gamma, \delta)$, $\theta_2=-\gamma\delta\alpha\zeta^{\frac{1}{2}}(\gamma, \delta, \alpha)$, $\theta_3=\delta\alpha\beta\zeta^{\frac{1}{2}}(\delta, \alpha, \beta)$, and $\theta_4=-\alpha\beta\gamma\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma)$, α, β, γ, δ being the roots of the quartic equation $(a, b, c, d, e)(v, 1)^4=0$.

$$\begin{aligned} \Sigma_4\theta_1 = {}^*\Sigma_4\beta\gamma\delta\zeta^{\frac{1}{2}}(\beta, \gamma, \delta) &= -(\alpha-\beta)(\alpha-\gamma)(\alpha-\delta)(\beta-\gamma)(\beta-\delta)(\gamma-\delta) \\ &= -\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma, \delta) \\ &= \frac{1}{a^3}\sqrt{Z}, \end{aligned}$$

where $Z=256a^3e^3+$ &c. is the discriminant of the quartic.

$$\begin{aligned} \Sigma_6\theta_1\theta_2=\Sigma_6\theta_3\theta_4=\Sigma_6\delta\alpha\beta\zeta^{\frac{1}{2}}(\delta, \alpha, \beta)\times -\alpha\beta\gamma\zeta^{\frac{1}{2}}(\alpha, \beta, \gamma) \\ =\Sigma_6\delta\alpha\beta(\delta-\alpha)(\delta-\beta)(\alpha-\beta)\times -\alpha\beta\gamma(\alpha-\beta)(\alpha-\gamma)(\beta-\gamma) \\ =-\alpha\beta\gamma\delta\Sigma_6\alpha\beta(\alpha-\beta)^2(\alpha-\gamma)(\alpha-\delta)(\beta-\gamma)(\beta-\delta), \end{aligned}$$

where $\alpha\beta\gamma\delta=-\frac{e}{a^3}$, and

* The signs of θ_1, θ_2, θ_3, θ_4 are taken account of implicitly.

$$\Sigma_6\alpha\beta(\alpha-\beta)^2(\alpha-\gamma)(\alpha-\delta)(\beta-\gamma)(\beta-\delta)$$

$$=\Sigma_6\alpha\beta(\alpha-\beta)^2\{\alpha^2\beta^2-\alpha\beta(\alpha+\beta)(\gamma+\delta)+(\alpha^2+\beta^2)\gamma\delta+\alpha\beta(\gamma+\delta)^2-\gamma\delta(\alpha+\beta)(\gamma+\delta)+\gamma^2\delta^2\}$$

$$=\Sigma_6\alpha^2\beta^2(\alpha-\beta)^2, \text{ viz.} \quad \Sigma_{12}\alpha^3\beta^3$$
$$-2\Sigma_6\alpha^4\beta^4$$

$$-\Sigma_6\alpha^2\beta^2(\alpha-\beta)^2(\alpha+\beta)(\gamma+\delta), \text{ viz.} \quad -\Sigma_{24}\alpha^3\beta^2\gamma$$
$$+\Sigma_{24}\alpha^4\beta^3\gamma$$

$$+\alpha\beta\gamma\delta\Sigma_6(\alpha-\beta)^2(\alpha^2+\beta^2), \text{ viz.} \quad +\frac{e}{a}3\Sigma_4\alpha^4$$
$$-2\frac{e}{a}\Sigma_{12}\alpha^3\beta$$
$$+2\frac{e}{a}\Sigma_6\alpha^2\beta^2$$

$$+\Sigma_6\alpha^2\beta^2(\alpha-\beta)^2(\gamma+\delta)^2, \text{ viz.} \quad +2\Sigma_{12}\alpha^4\beta^2\gamma^2$$
$$-2\Sigma_{12}\alpha^3\beta^2\gamma^2$$
$$+2\frac{e}{a}\Sigma_{12}\alpha^3\beta$$
$$-4\frac{e}{a}\Sigma_6\alpha^2\beta^2$$

$$-\alpha\beta\gamma\delta\Sigma_6(\alpha-\beta)^2(\alpha+\beta)(\gamma+\delta), \text{ viz.} \quad -2\frac{e}{a}\Sigma_{12}\alpha^3\beta$$
$$+2\frac{e}{a}\Sigma_{12}\alpha^2\beta\gamma$$

$$+\alpha\beta\gamma\delta\Sigma_6(\alpha-\beta)^2\gamma\delta, \text{ viz.} \quad +\frac{e}{a}\Sigma_{12}\alpha^2\beta\gamma$$
$$-12\frac{e^2}{a^2}$$

$$=\begin{array}{l}\Sigma_{12}\alpha^3\beta^3\\ -2\Sigma_6\alpha^4\beta^4\\ -\Sigma_{24}\alpha^3\beta^2\gamma\\ +\Sigma_{24}\alpha^4\beta^3\gamma\\ +2\Sigma_{12}\alpha^4\beta^2\gamma^2\\ -2\Sigma_{12}\alpha^3\beta^2\gamma^2\\ +\frac{e}{a}3\Sigma_4\alpha^4\\ -2\frac{e}{a}\Sigma_{12}\alpha^3\beta\\ -2\frac{e}{a}\Sigma_6\alpha^2\beta^2\\ +3\frac{e}{a}\Sigma_{12}\alpha^2\beta\gamma\\ -12\frac{e^2}{a^2}\end{array} \quad \begin{array}{l}(53)\\ -2(4^3)\\ -1(521)\\ +1(431)\\ +2(42^2)\\ -2(3^22)\\ +3e(4)\\ -2e(31)\\ -2e(2^2)\\ +3e(21^2)\\ -12e^2,\end{array}$$

where for a moment a is put equal to unity.

The value of the last-mentioned expression is then calculated as follows:—

e^3	−4	−12	− 8	− 8	−8	−8	−12	−8	−4	−12	−12	=	−96
bde	+1	+16	+10	+10		+2	+12	+2	+4	+ 3		=	+60
c^2e	+8	+ 8	+ 4		+8	+4	+ 6	+4	−2			=	+40
b^2ce	−9	− 8	−11	− 1	−4		−12	−2				=	−47
b^4e	+3		+ 3				+ 3					=	+ 9
cd^2	−7	− 8	− 5	− 1	−4	−2						=	−27
b^2d^2	+3	− 4	+ 1	− 2	+2							=	0
bc^2d	+6	+ 8	+ 3	+ 1								=	+18
b^3cd	−3		− 1									=	− 4
c^4	−2	− 2										=	− 4
b^2c^2	+1											=	+ 1

And restoring the powers of a by the principle of homogeneity, and putting

$$\mathrm{M}=\begin{array}{|rl|}\hline +96 & a^3e^2\\ -60 & a^2bde\\ -40 & a^2c^2e\\ +27 & a^2cd^2\\ +47 & ab^2ce\\ -18 & abc^2d\\ +\ 4 & ac^4\\ -\ 9 & b^4e\\ +\ 4 & b^3cd\\ -\ 1 & b^2c^3\\ \hline\end{array}$$

we have

$$\Sigma\theta_1\theta_2=\frac{1}{a^6}\mathrm{M}e.$$

Next,

$$\begin{aligned}\Sigma_{\alpha\beta\gamma\delta}\theta_1\theta_2\theta_3&=\Sigma_{\alpha\beta\gamma\delta}\beta\gamma\delta\zeta^{\frac{1}{2}}(\beta,\gamma,\delta)\times-\gamma\delta\alpha\zeta^{\frac{1}{2}}(\gamma,\delta,\alpha)\times\delta\alpha\beta\zeta^{\frac{1}{2}}(\delta,\alpha,\beta)\\ &=\Sigma_{\alpha\beta\gamma\delta}\beta\gamma\delta(\beta-\gamma)(\beta-\delta)(\gamma-\delta)\times-\gamma\delta\alpha(\gamma-\delta)(\gamma-\alpha)(\delta-\alpha)\times\delta\alpha\beta(\delta-\alpha)(\delta-\beta)(\alpha-\beta)\\ &=\quad\alpha^3\beta^3\gamma^2\delta^2(\alpha-\beta)(\alpha-\gamma)(\alpha-\delta)(\beta-\gamma)(\beta-\delta)(\gamma-\delta)\Sigma_\alpha\alpha(\alpha-\beta)(\alpha-\gamma)(\alpha-\delta)\\ &=\quad\alpha^3\beta^3\gamma^2\delta^2\zeta^{\frac{1}{2}}(\alpha,\beta,\gamma,\delta)\Sigma_\alpha\alpha(\alpha-\beta)(\alpha-\gamma)(\alpha-\delta)\\ &=-\frac{e^2}{a^5}\sqrt{\mathrm{Z}}\Sigma_\alpha\alpha(\alpha-\beta)(\alpha-\gamma)(\alpha-\delta),\end{aligned}$$

and observing that

$$(a,b,c,d,e\text{Y}(v,1)^4=a(v-\alpha)(v-\beta)(v-\gamma)(v-\delta),$$

and therefore

$$4av^3+bv^3+2cv+d=a(v-\beta)(v-\gamma)(v-\delta)+\ \&c.,$$

which, putting $v=\alpha$, gives

$$4a\alpha^3+3b\alpha^2+2c\alpha+d=a(\alpha-\beta)(\alpha-\gamma)(\alpha-\delta),$$

we have

$$\begin{aligned}\Sigma_\alpha\alpha(\alpha-\beta)(\alpha-\gamma)(\alpha-\delta)&=\frac{1}{a}(4a\Sigma\alpha^4+3b\Sigma\alpha^3+2c\Sigma\alpha^2+d\Sigma\alpha)\\ &\doteq 4(4)+3b(3)+2c(2)+d(1),\end{aligned}$$

where for a moment a is put equal to 1. This is calculated by

e	-16				$=$	-16
bd	$+16$	-9		-1	$=$	$+\ 6$
c^2	$+\ 8$		-4		$=$	$+\ 4$
b^2c	-16	$+9$	$+2$		$=$	$-\ 5$
b^4	$+\ 4$	-3			$=$	$+\ 1$

or restoring the powers of a, and putting

$$\mathrm{N}=\begin{array}{|rl|}\hline -16 & a^3e\\ +\ 6 & a^2bd\\ +\ 4 & a^2c^2\\ -\ 5 & ab^2c\\ +\ 1 & b^4\\ \hline\end{array}$$

we have

$$\Sigma_4\theta_1\theta_2\theta_3=\frac{1}{a^3}Ne^3\sqrt{Z}.$$

Lastly,

$$\begin{aligned}\Sigma_1\theta_1\theta_2\theta_3\theta_4 \text{ or } \theta_1\theta_2\theta_3\theta_4&=\alpha^2\beta^2\gamma^2\delta^2\zeta^{\frac{1}{2}}(\beta,\gamma,\delta)\zeta^{\frac{1}{2}}(\gamma,\delta,\alpha)\zeta^{\frac{1}{2}}(\delta,\alpha,\beta)\zeta^{\frac{1}{2}}(\alpha,\beta,\gamma)\\&=\alpha^2\beta^2\gamma^2\delta^2\zeta(\alpha,\beta,\gamma,\delta)\\&=\frac{e^3}{a^3}Z,\end{aligned}$$

and the equation $\Pi_4(\theta-\theta_1)=0$ is thus found to be the one given above, No. 10.

IV. *On the Synthesis of Succinic and Pyrotartaric Acids.*

By MAXWELL SIMPSON, *M.B. Communicated by Dr.* FRANKLAND, *F.R.S.*

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SUCCINIC acid bears the same relation to the diatomic alcohol glycol that propionic acid bears to ordinary alcohol. Propionic acid can be obtained by treating the cyanide of the alcohol radical with potash. Can succinic acid be obtained by treating the cyanide of the glycol radical with the same reagent? or is it an isomeric acid that is formed under those circumstances?

$$\underbrace{C_4H_5Cy}_{\text{Cyanide of Ethyle.}}+O_2\left\{\begin{matrix}K\\H\end{matrix}\right.+2HO=O_2\left\{\begin{matrix}C_6H_5O_2\\K\end{matrix}\right.+\underbrace{NH_3}_{\text{Propionate of Potash.}}$$

$$\underbrace{C_4H_4Cy_2}_{\text{Cyanide of Ethylene.}}+2\left\{O_2\left\{\begin{matrix}K\\H\end{matrix}\right.\right.+4HO=O_4\left\{\begin{matrix}C_8H_4O_4\\K_2\end{matrix}\right.+\underbrace{2NH_3}_{\text{Succinate of Potash?}}$$

The following experiments were performed with the view of determining this point.

Preparation of Cyanide of Ethylene.

As a preliminary step to the formation of succinic acid in this way, it became of course necessary to prepare the cyanide of ethylene. This body I obtained by submitting bromide of ethylene to the action of cyanide of potassium. The process was thus conducted:—A mixture of two equivalents of the cyanide and one of the bromide, together with a considerable quantity of alcohol, was exposed in well-corked soda-water bottles to the temperature of a water-bath for about sixteen hours. To prevent the caking of the salt, it is advisable to have some coarsely-powdered glass in the bottles, and to agitate them occasionally. At the expiration of this time the bottles were opened, and the alcohol separated and distilled. A semifluid residue was thus obtained, which was filtered at 100° Cent. It was very dark in colour, owing to the presence of a considerable quantity of a tarry matter, which embarrassed me for a long time. This I at last succeeded in removing, by exposing the residue to a powerful freezing mixture, and pressing it, while in the mixture, between folds of bibulous paper, as long as the paper was stained. After this treatment there remained a crystalline mass, which was almost white. This was finally washed with a small quantity of ether, and dissolved in the same fluid. The residue obtained on evaporating the etherial solution is the body in question. It was dried at 100° Cent., and analysed. The nunbers obtained agree

with the formula of cyanide of ethylene ($C_4 H_4 Cy_2$), as will be seen from the following Table:—

	Theory.		Experiment.	
		per cent.	I.	II.
C_8 . .	48·00	60·00	59·20	——
H_4 . .	4·00	5·00	5·55	——
N_2 . .	28·00	35·00	——	34·00*
	80·00	100·00		

This is, I believe, the first example of a diatomic cyanide. It has the following properties:—Below the temperature of 37° Cent. it is a crystalline solid of a light-brown colour, above that temperature it is a fluid oil. It cannot be distilled. Nevertheless it bears a tolerably high temperature without suffering much decomposition. Its specific gravity at 45° Cent. is 1·023. It is very soluble in water and alcohol, and sparingly soluble in ether. It has an acrid disagreeable taste. It is neutral to test-paper. Gently warmed with potassium, it is decomposed, cyanide of potassium being formed in large quantity. Its solution in water is not precipitated by nitrate of silver.

Action of Potash on Cyanide of Ethylene. Formation of Succinic Acid.

An alcoholic solution of crude cyanide of ethylene was prepared in the manner already described, and introduced, together with some sticks of caustic potash, into a large balloon with a reversed LIEBIG'S condenser attached to it. Heat was then applied by means of a water-bath, which caused torrents of ammonia to be evolved. As soon as the evolution of this gas had ceased, the alcohol was distilled off, and in order to secure the complete decomposition of the cyanide, the residue was treated with a solution of potash, and exposed to heat as long as the slightest evolution of ammonia could be detected. A considerable excess of strong nitric acid was then cautiously added, and the whole evaporated to dryness. The nitric acid destroys all the impurities present, and leaves a mixture of nitrate of potash and a free acid, easily separable by means of alcohol, which dissolves the latter but not the former. The acid obtained, on distilling off the alcohol, was twice crystallized from water, dried at 100° Cent., and analysed. The numbers obtained agree perfectly with the formula of succinic acid, as the following Table will show:—

	Theory.		Experiment.	
		per cent.	I.	II.
C_8 . .	48·00	40·67	40·54	40·30
H_6 . .	6·00	5·08	5·07	5·02
O_8 . .	64·00	54·25	——	——
	118·00	100·00		

* A slight loss occurred in this analysis.

A silver-salt* was also prepared, and gave on analysis the following results:—

	Theory.		Experiment.		
		per cent.	I.	II.	III.
C_8 . .	48·00	14·46	14·58	14·49	——
H_4 . .	4·00	1·20	1·48	1·44	——
O_8 . .	64·00	19·28	——	——	——
Ag_2 . .	216·00	65·06	——	——	63·87
	332·00	100·00			

The acid possessed also all the properties of succinic acid. It melted a few degrees above 180° Cent., and sublimed on the application of a higher temperature. It was very soluble in water and alcohol, and sparingly soluble in ether. It gave, when neutralized, a reddish-brown precipitate with perchloride of iron. Moreover, on digesting this precipitate with ammonia, and filtering, an acid could be detected in the filtered liquor, which gave white precipitates with nitrate of silver, and with a mixture of chloride of barium and alcohol. On passing a stream of muriatic acid gas through a solution of the acid in absolute alcohol, an oil insoluble in water was obtained, which distilled between 220° and 225° Cent. This was evidently succinic ether.

The above is an easy and a productive process, and yields the acid at once in a state of purity. From 1500 grains of bromide of ethylene I obtained 480 grains of succinic acid, or nearly 33 per cent.

I have studied in addition the action of some acids, and of nitrate of silver on cyanide of ethylene.

Action of Nitric Acid on Cyanide of Ethylene.

When cyanide of ethylene and nitric acid are evaporated together on a water-bath, a white crystalline mass is obtained. This proved to be a mixture of succinic acid and nitrate of ammonia. The acid can be completely separated from the nitrate by means of ether, of which, however, a large quantity is requisite. A silver-salt of the acid was prepared and analysed. It gave 64·07 instead of 65·06 per cent. of silver.

Action of Muriatic Acid on Cyanide of Ethylene.

A mixture of cyanide of ethylene and an excess of strong muriatic acid was exposed for a few hours in a sealed tube to the temperature of 100° Cent. On cooling, the contents of the tube became a mass of crystals, which I ascertained to be a mixture of

* The acid from which this salt was prepared, was obtained in a somewhat different manner. Instead of liberating it from its combination with potash by means of nitric acid, muriatic acid was employed. The whole was then evaporated at a gentle heat, and the residue repeatedly digested with absolute alcohol. On evaporating the alcohol, the succinic acid was obtained free from the chloride of potassium which accompanied it. The silver-salt evidently contained a trace of an acid having a higher atomic weight than succinic acid.

succinic acid and muriate of ammonia. The following equation will explain the reaction:—

$$C_4H_4Cy_2+2HCl+8HO=C_8H_6O_8+2(NH_4Cl).$$

This reaction enabled me to determine the amount of nitrogen in the cyanide of ethylene in a very easy manner. It was simply necessary to perform the above experiment on a weighed quantity of the cyanide, and afterwards to ascertain the amount of nitrogen in the mixed crystals by means of bichloride of platinum in the usual way.

Action of Nitrate of Silver on Cyanide of Ethylene.

About three equivalents of crystallized nitrate of silver were rubbed up in a mortar with one equivalent of pure cyanide of ethylene and a considerable quantity of ether. The ether was then poured off, and the residual salt dissolved in boiling alcohol. On cooling, the alcohol became a mass of brilliant pearly plates. These were washed with ether, dried at 100° Cent., and analysed. The numbers obtained lead to the formula $C_4H_4Cy_2+4(AgO,NO_5)$, as will be seen from the following Table:—

	Theory.		Experiment.	
		per cent.	I.	II.
C_8 . . .	48·00	6·31	6·29	——
H_4 . . .	4·00	0·53	0·66	——
N_6 . . .	84·00	11·06	——	——
O_{24} . .	192·00	25·26	——	——
Ag_4 . . .	432·00	56·84	——	56·68
	760·00	100·00		

That this body is simply formed by a union of the cyanide and nitrate in these proportions without decomposition, is confirmed by the fact that, when the crystals are treated with strong hydrochloric acid and the whole evaporated on a water-bath, succinic acid can be detected in the residue. The acid is evidently formed by the action of the liberated nitric acid on the cyanide of ethylene. The crystals are soluble in water and alcohol, but insoluble in ether. When heated they explode like gunpowder. They do not, however, detonate on percussion. This compound may possibly throw some light on the constitution of the fulminates. It gives a remarkable silver-salt, when treated with nitrous acid (NO_3), which is insoluble in alcohol, and so fusible, that it can be melted under that liquid. This I am at present engaged in studying.

I have endeavoured to substitute two equivalents of bromine for the two of cyanogen in the cyanide of ethylene, so as to regenerate bromide of ethylene, but without success, the molecule being completely broken up by the action of the bromine.

We are now in a condition to answer the question proposed at the commencement of this paper. Succinic acid *can* be obtained from glycol in the same manner as propionic acid from ordinary alcohol; the bromide of ethylene, the point from which I started, being capable of derivation from the diatomic alcohol.

We are now enabled, thanks to the researches of Messrs. PERKIN and DUPPA, and of M. KEKULÉ*, to build up three highly complex organic acids (succinic, paratartaric, and malic) from a simple hydrocarbon; and what is more important, we are enabled to do this by processes, every stage of which is perfectly intelligible.

The question now arises, Is the foregoing reaction capable of general application? Can the homologues of succinic acid be obtained in a similar manner? With the view of determining this point, I have endeavoured to prepare pyrotartaric acid from the cyanide of propylene, the radical of propylglycol.

Preparation of Cyanide of Propylene.

This body, which forms the first step in my process for pyrotartaric acid, I succeeded in obtaining in the following manner:—A mixture of one equivalent of bromide of propylene and two of cyanide of potassium, together with a considerable quantity of alcohol, was exposed to the temperature of a water-bath in well-corked soda-water-bottles for about sixteen hours. Their contents were then filtered, and the alcohol distilled off the filtered liquor. A liquid residue was thus obtained, which was very black, and by no means of a promising appearance. This was filtered at 100° Cent., and digested with ether, which left a large quantity of a black tarry matter undissolved. The residue obtained on evaporating the etherial solution was then submitted to distillation. Almost the entire liquid passed over between 265° and 290° Cent. The fraction distilling between 277° and 290° was analysed and gave the following numbers:—

	Theory.		Experiment.	
		per cent.	I.	II.
C_{10} . . .	60·00	63·82	61·95	——
H_6 . . .	6·00	6·38	6·54	——
N_2 . . .	28·00	29·80	——	29·49
	94·00	100·00		

I do not think it possible to obtain this body in a state of greater purity, unless by fractional distillation *in vacuo*, since it suffers partial decomposition when distilled in air, as evidenced by a slight evolution of ammonia during the process.

The properties of this cyanide very much resemble those of the preceding. It differs however in its physical state, which is that of a liquid at the ordinary temperature of the air. It is soluble in water, alcohol, and ether. It has an acrid taste. It is colourless and neutral to test-paper. It is decomposed with great facility by potassium, cyanide of potassium being formed in large quantity. Its solution in water gives no precipitate with nitrate of silver. Heated with potash, an acid is formed, and ammonia evolved.

* Quarterly Journal of the Chemical Society, July 1860; and Bulletin de la Société Chimique de Paris, 10 Août 1860, p. 208.

Formation of Pyrotartaric Acid.

A mixture of one volume of cyanide of propylène, distilling between 265° and 290° C., and about 1½ volume of strong hydrochloric acid, was exposed in a glass tube hermetically sealed to the temperature of a water-bath for about six hours.

Long before the expiration of this time the contents of the tube had become a mass of crystals. These were dried at 100° Cent., and dissolved in absolute alcohol. The residue obtained on evaporating the alcoholic solution was then twice crystallized from water, and finally dissolved in ether, in order to remove the last traces of the ammoniacal salt formed in the process. The body obtained on distilling off the ether is the acid in question. It was dried at 100° Cent. and analysed. The numbers obtained correspond with the formula of pyrotartaric acid, as will be seen from the following Table:—

	Theory.		Experiment.	
		per cent.	I.	II.
C_{10} . .	60·00	45·45	44·60	44·58
H_8 . .	8·00	6·06	5·83	5·70
O_8 . . .	64·00	48·49	——	——
	132·00	100·00		

It had also all the properties ascribed to this acid by Pelouze and Arppe. The crystals were colourless and very soluble in water, alcohol and ether. It had an agreeable acid taste, and expelled carbonic acid from its combinations with the alkalies. It became semifluid at 100° Cent., and melted completely a few degrees above that temperature. Long-continued ebullition in a glass tube converted it into an oil, which was insoluble in cold water, and no longer affected litmus paper, but which gradually dissolved in boiling water, recovering at the same time its acid reaction. Lime-water was not affected by a solution of this acid. Neutralized by ammonia, it gave a white curdy precipitate with nitrate of silver. Acetate of lead caused no precipitate with the neutralized acid. On the addition, however, of alcohol to the mixed solutions, a bulky white precipitate was obtained, which melted into oily drops on heating the liquid in which it was suspended. The following equation will explain the reaction which gives rise to this acid:—

$$C_6 H_6 Cy_2 + 2H Cl + 8HO = C_{10} H_8 O_8 + 2(N H_4 Cl).$$

Pyrotartaric acid bears the same relation to propylglycol that butyric acid bears to propylic alcohol:—

$\underbrace{C_6 H_8 O_4}$	$\underbrace{C_6 H_8 O_2}$
Propylglycol.	Propylic Alcohol.
$\underbrace{C_{10} H_8 O_8}$	$\underbrace{C_8 H_8 O_4}$
Pyrotartaric Acid.	Butyric Acid.

The claims of this acid to be considered the homologue of succinic acid, which some

chemists do not recognize, are now, I think, fairly established, seeing that these two acids can be obtained by similar processes from homologous hydrocarbons.

The foregoing reaction, which, I think, we may now look upon as capable of general application, will no doubt place in our hands some of the missing acids of the succinic series.

It is highly probable that there exists a series of isomeric acids running parallel to these, which may be obtained by similar processes from the diatomic radicals contained in the aldehydes. Thus from the cyanide of ethylidene ($C_4 H_4 Cy_2$) we may hope to get an isomer of succinic acid.

The reactions I have just described lend, I think, some support to FRANKLAND and KOLBE'S view of the constitution of these acids, namely, that they are composed of two equivalents of carbonic acid, in which two equivalents of oxygen are replaced by a diatomic radical. However this may be, it is convenient, at all events, to formulate these bodies according to the carbonic acid type:—

$$\underbrace{2HO, C_4 H_4'' \left[\begin{matrix} C_2 O_2 \\ C_2 O_2 \end{matrix}\right] O_2}_{\text{Succinic Acid.}} \qquad \underbrace{2HO, C_6 H_6'' \left[\begin{matrix} C_2 O_2 \\ C_2 O_2 \end{matrix}\right] O_2}_{\text{Pyrotartaric Acid.}}$$

I propose to continue my researches in this direction, and to extend them to the cyanides of the triatomic radicals.

V. *On the Calculus of Symbols, with Applications to the Theory of Differential Equations.*

By W. H. L. Russell, *Esq.*, *A.B.* *Communicated by* Arthur Cayley, *Esq.*, *F.R.S.*

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The calculus of generating functions, discovered by Laplace, was, as is well known, highly instrumental in calling the attention of mathematicians to the analogy which exists between differentials and powers. This analogy was perceived at length to involve an essential identity, and several analysts devoted themselves to the improvement of the new methods of calculation which were thus called into existence. For a long time the modes of combination assumed to exist between different classes of symbols were those of ordinary algebra; and this sufficed for investigations respecting functions of differential coefficients and constants, and consequently for the integration of linear differential equations, with constant coefficients. The laws of combination of ordinary algebraical symbols may be divided into the commutative and distributive laws; and the number of symbols in the higher branches of mathematics, which are commutative with respect to one another, is very small. It became then necessary to invent an algebra of non-commutative symbols. This important step was effected by Professor Boole, for certain classes of symbols, in his well-known and beautiful memoir published in the Transactions of this Society for the year 1844, and the object of the paper which I have now the honour to lay before the Society is to perfect and develope the methods there employed.

For this purpose I have constructed systems of multiplication and division for functions of non-commutative symbols, subject to the same laws of combination as those assumed in Professor Boole's memoir, and I thus arrive at equations of great utility in the integration of linear differential equations with variable coefficients.

I then proceed to develope certain general theorems, which will, I hope, be found interesting. I have applied the methods of multiplication, as just explained, to deduce theorems for non-commutative symbols analogous to the binomial and multinomial theorems of ordinary algebra.

Lastly, I have shown how to employ the equations deduced in the earlier part of this paper in the integration of linear differential equations. I have, for this purpose, made use of methods closely resembling the method of divisors which has so long been used in resolving ordinary algebraical equations. The whole paper will, I hope, be found to be a step upwards in the important subject of which it treats. I shall just observe, that the symbolical combinations used in this paper may also be applied to the calculus of finite differences, as may be seen in Professor Boole's memoir.

Section I. *On the Principles of Symbolical Algebra.*

Let (ϱ) and (π) be two functional symbols combining according to the law $\varrho^n f(\pi)u = f(\pi-n)\varrho^n u$, where (u) is the subject. We shall suppose throughout this paper that $\varrho=x$, $\pi=x\frac{d}{dx}$.

Let P, Q, and R be three functions of (π) and (ϱ), such that PQ acting on any subject is equivalent to R acting on the same subject, or PQ=R. We shall say that P externally multiplies Q, and is an external factor of R. In like manner we shall say that Q internally multiplies P, and is an internal factor of R. We shall also say that R is externally divisible by P, internally by Q.

We easily see the truth of the following symbolical equations depending on the laws of combination assumed above:—

$$(\varrho^a\pi^\beta)(\varrho^\alpha\pi^b)u = \varrho^{a+\alpha}(\pi+a)^\beta\pi^b u$$
$$(\varrho^a\pi^\beta)(\varrho^\alpha\pi^b)^{-1}u = \varrho^{a-\alpha}(\pi-a)^\beta\pi^{-b}u.$$

We shall commence with instances of symbolical multiplication and division, when the multipliers and divisors are monomials.

The following is an instance of external multiplication:—

$$\varrho\pi(\varrho^3+\varrho\pi+\pi^2)=\varrho^3(\pi+2)+\varrho^2(\pi^2+\pi)+\varrho\pi^2;$$

the following is an instance of internal multiplication:—

$$(2\varrho^3-3\varrho\pi^2+(\pi^2+\pi))\varrho\pi^2=2\varrho^3\pi^2-3\varrho^2(\pi^2+\pi)^2+\varrho\pi^2(\pi+1)(\pi+2);$$

the following of external division:—

$$(\varrho^2\pi)^{-1}(\varrho^4(\pi+2)-2\varrho^2\pi(\pi+1)+3\varrho^2\pi^2)=\varrho^2-2\varrho\pi+3\pi^2;$$

the following of internal division:—

$$(\varrho^3\pi+3\varrho^2(\pi^2+\pi)+\varrho\pi(\pi+1)^2)(\varrho\pi)^{-1}=\varrho^2+3\varrho\pi+\pi^2.$$

We shall now consider cases where the multipliers and divisors are polynomials.

The following are instances of external multiplication:—

$$\begin{array}{l}\varrho+\pi\\ \varrho-\pi\\ \hline \varrho^2+\varrho\pi\\ \quad -\varrho(\pi+1)-\pi^2\\ \hline \varrho^2 \;-\; \varrho \;-\; \pi^2\end{array}$$

$$\begin{array}{l}\varrho\pi^2-(\pi+1)\\ \varrho\pi-\pi^2\\ \hline \varrho^3(\pi+1)\pi^2-\varrho\pi(\pi+1)\\ \qquad -\varrho(\pi+1)^2\pi^2+\pi^2(\pi+1)\\ \hline \varrho^3(\pi+1)\pi^2-\varrho\pi(\pi+1)(\pi^2+\pi+1)+\pi^2(\pi+1).\end{array}$$

The following are instances of internal multiplication:—

$$\begin{array}{l} \varrho+\pi \\ \varrho-\pi \\ \hline \varrho^2+\varrho(\pi+1) \\ \quad -\varrho\pi \qquad -\pi^2 \\ \hline \varrho^2+\varrho \qquad -\pi^2 \end{array}$$

$$\begin{array}{l} \varrho\pi^2-(\pi+1) \\ \varrho\pi-\pi^2 \\ \hline \varrho^2(\pi+1)^2\pi^2-\varrho\pi(\pi+2) \\ \qquad\qquad -\varrho\pi^4 \qquad +\pi^2(\pi+1) \\ \hline \varrho^2(\pi+1)^2\pi^2-\varrho\pi(\pi^3+\pi+2)+\pi^2(\pi+1). \end{array}$$

The results of the four last examples may be written thus:—

$$(\varrho-\pi)(\varrho+\pi)=\varrho^2-\varrho-\pi^2$$
$$(\varrho\pi-\pi^2)(\varrho\pi^2-(\pi+1))=(\varrho^2\pi-\varrho(\pi^3+\pi+1)+\pi)\pi(\pi+1)$$
$$(\varrho+\pi)(\varrho-\pi)=\varrho^2+\varrho-\pi^2$$
$$(\varrho\pi^2-(\pi+1))(\varrho\pi-\pi^2)=(\varrho^2(\pi^3+\pi)-\varrho(\pi^3-\pi+2)+\pi)\pi(\pi+1).$$

I shall now give some examples of external division, the divisor being a polynomial.

$$\begin{array}{l} \varrho+\pi\big)\varrho^3+2\varrho^2(\pi+1)+\varrho\pi(2\pi+1)+\pi^2\big(\varrho^2+\varrho\pi+\pi^2 \\ \quad \underline{\varrho^3+\ \varrho^2(\pi+2)} \\ \qquad \varrho^2\pi+\varrho\pi(2\pi+1) \\ \qquad \underline{\varrho^2\pi+\varrho\pi(\ \pi+1)} \\ \qquad\quad \varrho\pi^2+\pi^3 \\ \qquad\quad \underline{\varrho\pi^2+\pi^3} \end{array}$$

$$\begin{array}{l} \varrho\pi+\pi^2\big)\varrho^2(\pi+2)+\varrho^2(2\pi+3)-\varrho(3\pi^2+3\pi+1)+\pi^2\big(\varrho^2-\varrho(\pi+1)+\pi^2 \\ \quad \underline{\varrho^2(\pi+2)+\varrho^2(\ \pi+2)^2} \\ \qquad -\varrho^2(\ \pi+1)^2-\varrho(3\pi^2+3\pi+1) \\ \qquad \underline{-\varrho^2(\ \pi+1)^2-\varrho(\ \pi+1)^2} \\ \qquad\quad \varrho\pi^2+\pi^4 \\ \qquad\quad \underline{\varrho\pi^2+\pi^4} \end{array}$$

We shall next consider some examples of internal division.

$$\begin{array}{l} \varrho+\pi\big)\varrho^3+\varrho^2(2\pi+1)+\varrho(2\pi^2+2\pi+1)+\pi^2\big(\varrho^2+\varrho\pi+\pi^2 \\ \quad \underline{\varrho^3+\varrho^2\pi} \\ \qquad \varrho^2(\pi+1)+\varrho(2\pi^2+2\pi+1) \\ \qquad \underline{\varrho^2(\pi+1)+\varrho\pi^2} \\ \qquad\qquad \varrho(\pi+1)^2+\pi^2 \\ \qquad\qquad \underline{\varrho(\pi+1)^2+\pi^2} \end{array}$$

$$\begin{array}{l}\varrho\pi+\pi^2)\varrho^3\pi-2\varrho^3\pi+\varrho(\pi^3+\pi)+\pi^4\big(\varrho^3-\varrho(\pi+1)+\pi^3\\ \quad\underline{\varrho^3\pi+\varrho^3\pi^2}\\ \qquad\underline{-\varrho^3(\pi^3+2\pi)+\varrho(\pi^3+\pi)}\\ \qquad\quad\underline{-\varrho^3(\pi^3+2\pi)-\varrho(\pi+1)\pi^3}\\ \qquad\qquad\underline{\varrho\pi(\pi+1)^3+\pi^4}\\ \qquad\qquad\quad\underline{\varrho\pi(\pi+1)^3+\pi^4}\end{array}$$

The results of the four last examples may be written thus:

$$(\varrho+\pi)^{-1}(\varrho^3+2\varrho^2(\pi+1)+\varrho\pi(2\pi+1)+\pi^3)=\varrho^2+\varrho\pi+\pi^2$$

$$(\varrho\pi+\pi^2)^{-1}(\varrho^3(\pi+2)+\varrho^2(2\pi+3)-\varrho(3\pi^2+3\pi+1)+\pi^3)=\varrho^2-\varrho(\pi+1)+\pi^2$$

$$(\varrho^3+\varrho^2(2\pi+1)+\varrho(2\pi^2+2\pi+1)+\pi^3)(\varrho+\pi)^{-1}=\varrho^2+\varrho\pi+\pi^2$$

$$(\varrho^3\pi-2\varrho^2\pi+\varrho(\pi^2+\pi)+\pi^3)(\varrho\pi+\pi^2)^{-1}=\varrho^2-\varrho(\pi+1)+\pi^2.$$

I now come to two propositions of great importance.

First, to determine the condition that $\varrho\psi_1(\pi)+\psi_0(\pi)$ shall divide the symbolical function

$$\varrho^n\varphi_n(\pi)+\varrho^{n-1}\varphi_{n-1}(\pi)+\varrho^{n-2}\varphi_{n-2}(\pi)+\&c.+\varrho\varphi_1(\pi)+\varphi_0(\pi)$$

internally without a remainder,

$$\begin{array}{l}\varrho\psi_1(\pi)+\psi_0(\pi)\big)\varrho^n\varphi_n(\pi)+\varrho^{n-1}\varphi_{n-1}(\pi)+\varrho^{n-2}\varphi_{n-2}(\pi)+\ldots+\varrho^2\varphi_2(\pi)+\varrho\varphi_1(\pi)+\varphi_0(\pi)\\ \quad\underline{\varrho^n\varphi_n(\pi)+\varrho^{n-1}\frac{\psi_0(\pi)}{\psi_1(\pi-1)}\varphi_n(\pi-1)}\\ \qquad\underline{\varrho^{n-1}\Big\{\varphi_{n-1}(\pi)-\frac{\psi_0(\pi)}{\psi_1(\pi-1)}\varphi_n(\pi-1)\Big\}+\varrho^{n-2}\varphi_{n-2}(\pi)}\\ \qquad\underline{\varrho^{n-1}\Big\{\varphi_{n-1}(\pi)-\frac{\psi_0(\pi)}{\psi_1(\pi-1)}\varphi_n(\pi-1)\Big\}+\varrho^{n-2}\Big\{\frac{\psi_0(\pi)}{\psi_1(\pi-1)}\varphi_{n-1}(\pi-1)-\frac{\psi_0(\pi)\psi_0(\pi-1)}{\psi_1(\pi-1)\psi_1(\pi-2)}\varphi_n(\pi-2)\Big\}}\\ \qquad\qquad\varrho^{n-2}\Big\{\varphi_{n-2}(\pi)-\frac{\psi_0(\pi)}{\psi_1(\pi-1)}\varphi_{n-1}(\pi-1)+\frac{\psi_0(\pi)\psi_0(\pi-1)}{\psi_1(\pi-1)\psi_1(\pi-2)}\varphi_n(\pi-2)\Big\}+\&c.\end{array},$$

where the symbolical quotient is

$$\begin{array}{l}\varrho^{n-1}\frac{\varphi_n(\pi-1)}{\psi_1(\pi-1)}+\varrho^{n-2}\Big\{\frac{\varphi_{n-1}(\pi-1)}{\psi_1(\pi-1)}-\frac{\psi_0(\pi-1)}{\psi_1(\pi-1)\psi_1(\pi-2)}\varphi_n(\pi-2)\Big\}+\varrho^{n-3}\Big\{\frac{\varphi_{n-2}(\pi-1)}{\psi_1(\pi-1)}\\ -\frac{\psi_0(\pi-1)}{\psi_1(\pi-1)\psi_1(\pi-2)}\varphi_{n-1}(\pi-2)+\frac{\psi_0(\pi-1)\psi_0(\pi-2)}{\psi_1(\pi-1)\psi_1(\pi-2)\psi_1(\pi-3)}\varphi_n(\pi-3)\Big\}+\&c.\end{array}$$

The required condition is found by equating the remainder to zero; and we have

$$\begin{array}{l}\varphi_0(\pi)-\frac{\psi_0(\pi)}{\psi_1(\pi-1)}\varphi_1(\pi-1)+\frac{\psi_0(\pi)\psi_0(\pi-1)}{\psi_1(\pi-1)\psi_1(\pi-2)}\varphi_2(\pi-2)-\frac{\psi_0(\pi)\psi_0(\pi-1)\psi_0(\pi-2)}{\psi_1(\pi-1)\psi_1(\pi-2)\psi_1(\pi-3)}\varphi_3(\pi-3)+\&c.\\ \pm\frac{\psi_0\pi\psi_0(\pi-1)\psi_0(\pi-2)\ldots\psi_0(\pi-n+1)}{\psi_1(\pi-1)\psi_1(\pi-2)\psi_1(\pi-3)\ldots\psi_1(\pi-n)}\varphi_n(\pi-n)=0,\end{array}$$

where $\varrho\psi_1(\pi)+\psi_0(\pi)$ is an internal factor of $\varrho^n\varphi_n(\pi)+\varrho^{n-1}\varphi_{n-1}(\pi)+\&c.+\varphi_0(\pi)$.

Hence we see how we may resolve the symbolical function

$$\varrho^n\varphi_n(\pi)+\varrho^{n-1}\varphi_{n-1}(\pi)+\varrho^{n-2}\varphi_{n-2}(\pi)+\ldots\ldots+\varrho\varphi_1(\pi)+\varphi_0(\pi)$$

into factors in all possible cases.

Put

$$\psi_0(\pi)=A_0+B_0\pi+C_0\pi^2+\&c.,$$
$$\psi_1(\pi)=A_1+B_1\pi+C_1\pi^2+\&c.,$$

and substitute in the above equation, and equate the resulting coefficients of π to zero. We shall thus be furnished with equations for determining the values of A_0, B_0, &c., A_1, B_1, &c. in all cases in which the above symbolical function is capable of resolution. We thus obtain the values of $\psi_0(\pi)$, $\psi_1(\pi)$, and of the symbolical quotient. We next ascertain if the symbolical quotient admits of an internal factor, and repeating the process we at length resolve the above symbolical function into factors of the form

$$(\varrho\psi_1^{(n)}(\pi)+\psi_0^{(n)}(\pi))(\varrho\psi_1^{(n-1)}(\pi)+\psi_0^{(n-1)}(\pi))\ldots(\varrho\psi_1(\pi)+\psi_0(\pi)).$$

To determine the condition that $\varrho\psi_1(\pi)+\psi_0(\pi)$ shall divide the symbolical function

$$\varrho^n\varphi_n(\pi)+\varrho^{n-1}\varphi_{n-1}(\pi)+\varrho^{n-2}\varphi_{n-2}(\pi)+\&c.+\varrho\varphi_1(\pi)+\varphi_0(\pi)$$

externally without a remainder,

$$\begin{array}{l}
\varrho\psi_1(\pi)+\psi_0(\pi))\varrho^n\varphi_n(\pi)+\varrho^{n-1}\varphi_{n-1}(\pi)+\varrho^{n-2}\varphi_{n-2}(\pi)+\&c.+\varrho^2\varphi_2(\pi)+\varrho\varphi_1(\pi)+\varphi_0(\pi)\\
\qquad\qquad \varrho^n\varphi_n(\pi)+\varrho^{n-1}\dfrac{\psi_0(\pi+n-1)}{\psi_1(\pi+n-1)}\varphi_n(\pi)\\
\qquad\qquad\qquad\qquad \varrho^{n-1}\left\{\varphi_{n-1}(\pi)-\dfrac{\psi_0(\pi+n-1)}{\psi_1(\pi+n-1)}\varphi_n(\pi)\right\}+\varrho^{n-2}\varphi_{n-2}(\pi)\\
\qquad\qquad\qquad\qquad \varrho^{n-1}\left\{\varphi_{n-1}(\pi)-\dfrac{\psi_0(\pi+n-1)}{\psi_1(\pi+n-1)}\varphi_n(\pi)\right\}+\varrho^{n-2}\left\{\dfrac{\psi_0(\pi+n-2)}{\psi_1(\pi+n-2)}\varphi_{n-1}(\pi)-\dfrac{\psi_0(\pi+n-2)\psi_0(\pi+n-1)}{\psi_1(\pi+n-2)\psi_1(\pi+n-1)}\varphi_n(\pi)\right\}\\
\qquad\qquad\qquad\qquad\qquad\qquad \varrho^{n-2}\left\{\varphi_{n-2}(\pi)-\dfrac{\psi_0(\pi+n-2)}{\psi_1(\pi+n-2)}\varphi_{n-1}(\pi)+\dfrac{\psi_0(\pi+n-2)\psi_0(\pi+n-1)}{\psi_1(\pi+n-2)\psi_1(\pi+n-1)}\varphi_n(\pi)\right\}+\&c.
\end{array}$$

where the symbolical quotient is

$$\varrho^{n-1}\cdot\frac{\varphi_n(\pi)}{\psi_1(\pi+n-1)}+\varrho^{n-2}\left\{\frac{\varphi_{n-1}(\pi)}{\psi_1(\pi+n-2)}-\frac{\psi_0(\pi+n-1)}{\psi_1(\pi+n-2)\psi_1(\pi+n-1)}\varphi_n(\pi)\right\}$$
$$+\varrho^{n-3}\left\{\frac{\varphi_{n-2}\pi}{\psi_1(\pi+n-3)}-\frac{\psi_0(\pi+n-2)}{\psi_1(\pi+n-3)\psi_1(\pi+n-2)}\varphi_{n-1}(\pi)+\frac{\psi_0(\pi+n-2)\psi_0(\pi+n-1)}{\psi_1(\pi+n-3)\psi_1(\pi+n-2)\psi_1(\pi+n-1)}\varphi_n(\pi)\right\}+\&c.$$

The required condition is found by equating the remainder to zero: whence we have

$$\varphi_0(\pi)-\frac{\psi_0(\pi)}{\psi_1(\pi)}\varphi_1(\pi)+\frac{\psi_0(\pi)\psi_0(\pi+1)}{\psi_1(\pi)\psi_1(\pi+1)}\varphi_2(\pi)-\frac{\psi_0(\pi)\psi_0(\pi+1)\psi_0(\pi+2)}{\psi_1(\pi)\psi_1(\pi+1)\psi_1(\pi+2)}\varphi_3(\pi)+\&c.$$
$$\pm\frac{\psi_0(\pi)\psi_0(\pi+1)\psi_0(\pi+2)\ldots\psi_0(\pi+n-1)}{\psi_1(\pi)\psi_1(\pi+1)\psi_1(\pi+2)\ldots\psi_1(\pi+n-1)}\varphi_n(\pi)=0.$$

In the next investigation we shall suppose the symbolical function arranged in powers of (π) instead of powers of (ϱ). To determine the condition that $\psi_1(\varrho)\pi+\psi_0(\varrho)$ may be an internal factor of the symbolical function

$$\varphi_3(\varrho)\pi^3+\varphi_2(\varrho)\pi^2+\varphi_1(\varrho)\pi+\varphi_0(\varrho).$$

We easily see that

$$\pi\varphi(\varrho)=\varphi(\varrho)\pi+\varrho\frac{d}{d\varrho}\varphi(\varrho)$$

$$\pi^2\varphi(\varrho)=\varphi(\varrho)\pi^2+2\varrho\frac{d}{d\varrho}\varphi(\varrho)\pi+\left(\varrho\frac{d}{d\varrho}\right)^2\varphi(\varrho).$$

Hence we shall have

$$\varphi^2(\varrho)\pi^2\{\psi_1\varrho.\pi\}^{-1}=\varphi_2(\varrho)\pi^2.\frac{1}{\psi_1\varrho}=\frac{\varphi_2\varrho}{\psi_1\varrho}\pi^2+2\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1\varrho}.\pi+\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)^2\frac{1}{\psi_1(\varrho)};$$

$$\therefore\ \varphi_2(\varrho).\pi^2=\left\{\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\pi^2+2\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1\varrho}\pi+\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)^2.\frac{1}{\psi_1\varrho}\right\}\{\psi_1(\varrho).\pi+\psi_0(\varrho)-\psi_0(\varrho)\}$$

$$=\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\pi^2\{\psi_1(\varrho).\pi+\psi_0(\varrho)\}+2\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1\varrho}\pi\psi_1(\varrho)\pi$$

$$+\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)^2\frac{1}{\psi_1\varrho}.\psi_1(\varrho)\pi-\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\pi^2\psi_0(\varrho)$$

$$=\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\pi^2\{\psi_1(\varrho)\pi+\psi_0(\varrho)\}+2\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1(\varrho)}\{\psi_1(\varrho)\pi+\varrho\frac{d}{d\varrho}\psi_1(\varrho)\}\pi$$

$$+\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)^2.\frac{1}{\psi_1(\varrho)}.\psi_1(\varrho).\pi$$

$$-\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\left\{\psi_0(\varrho).\pi^2+2\left(\varrho\frac{d}{d\varrho}\right)\psi_0(\varrho).\pi+\left(\varrho\frac{d}{d\varrho}\right)^2\psi_0(\varrho)\right\};$$

$$\therefore\ \varphi_2(\varrho).\pi^2+\varphi_2(\varrho)\pi^2+\varphi_1(\varrho).\pi+\varphi_0(\varrho)$$

$$=\frac{\varphi_2\varrho}{\psi_1\varrho}\pi^2\{\psi_1)(\varrho)\pi+\psi_0(\varrho)\}$$

$$+\left\{\varphi_2(\varrho)+2\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1(\varrho)}.\psi_1(\varrho)-\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\psi_0(\varrho)\right\}\pi^2$$

$$+\left\{\varphi_1(\varrho)+2\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1(\varrho)}\varrho\frac{d}{d\varrho}\psi_1(\varrho)+\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)^2\frac{1}{\psi_1(\varrho)}.\psi_1(\varrho)-2\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\left(\varrho\frac{d}{d\varrho}\right)\psi_0(\varrho)\right\}\pi$$

$$+\varphi_0(\varrho)-\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\left(\varrho\frac{d}{d\varrho}\right)^2\psi_0(\varrho);$$

where we may put

$$\theta_2(\varrho)=\varphi_2(\varrho)+2\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1(\varrho)}\psi_1(\varrho)-\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\psi_0(\varrho)$$

$$\theta_1\varrho\ =\varphi_1(\varrho)+2\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1(\varrho)}\left(\varrho\frac{d}{d\varrho}\right)\psi_1(\varrho)+\varphi_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)^2\frac{1}{\psi_1(\varrho)}.\psi_1(\varrho)-2\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\left(\varrho\frac{d}{d\varrho}\right)\psi_0(\varrho)$$

$$\theta_0\varrho\ =\varphi_0(\varrho)-\frac{\varphi_2(\varrho)}{\psi_1(\varrho)}\left(\varrho\frac{d}{d\varrho}\right)^2\psi_0(\varrho)$$

$$\theta_2(\varrho)\pi^2\{\psi_1(\varrho).\pi\}^{-1}=\theta_2(\varrho).\pi.\frac{1}{\psi_1(\varrho)}=\frac{\theta_2(\varrho)}{\psi_1(\varrho)}\pi+\theta_2(\varrho).\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1(\varrho)};$$

$$\theta_2(\varrho)\pi^2=\frac{\theta_2(\varrho)}{\psi_1(\varrho)}\pi\{\psi_1(\varrho)\pi+\psi_0(\varrho)\}$$

$$-\frac{\theta_2(\varrho)}{\psi_1(\varrho)}\left\{\psi_0(\varrho).\pi+\varrho\frac{d}{d\varrho}\psi_0(\varrho)\right\}+\theta_2(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1(\varrho)}.\psi_1(\varrho)\pi$$

$$=\frac{\theta_2\varrho}{\psi_1\varrho}\pi\{\psi_1(\varrho)\pi+\psi_0(\varrho)\}-\theta_2(\varrho)\left\{\frac{\psi_0(\varrho)}{\psi_1(\varrho)}-\varrho\frac{d}{d\varrho}\left(\frac{1}{\psi_1\varrho}\right)\psi_1(\varrho)\right\}\pi-\frac{\theta_2\varrho}{\psi_1\varrho}\varrho\frac{d}{d\varrho}\psi_0(\varrho).$$

Then

$$\theta_2(\varrho)\pi^2+\theta_1(\varrho)\pi+\theta_0(\varrho)=\frac{\theta_2(\varrho)}{\psi_1(\varrho)}\pi\{\psi_1(\varrho)\pi+\psi_0(\varrho)\}$$
$$+\left\{\theta_1(\varrho)-\frac{\theta_2\varrho\psi_0\varrho}{\psi_1\varrho}+\theta_2\varrho\,\varrho\frac{d}{d\varrho}\cdot\frac{1}{\psi_1\varrho}\cdot\psi_1\varrho\right\}\pi$$
$$+\left\{\theta_0(\varrho)-\frac{\theta_2(\varrho)}{\psi_1(\varrho)}\varrho\frac{d}{d\varrho}\psi_0\varrho\right\}.$$

Put, for the sake of simplicity,

$$\omega_1(\varrho)=\theta_1(\varrho)-\frac{\theta_2(\varrho)\psi_0\varrho}{\psi_1\varrho}+\theta_2\varrho\cdot\varrho\frac{d}{d\varrho}\cdot\frac{1}{\psi_1\varrho}\cdot\psi_1\varrho$$

$$\omega_0(\varrho)=\theta_0\varrho-\frac{\theta_2\varrho}{\psi_1\varrho}\varrho\frac{d}{d\varrho}\psi_0(\varrho)$$

$$\omega_1(\varrho).\pi.\{\psi_1\varrho\pi\}^{-1}=\frac{\omega_1\varrho}{\psi_1\varrho},$$

$$\therefore\ \omega_1(\varrho)\pi=\frac{\omega_1(\varrho)}{\psi_1(\varrho)}\{\psi_1\varrho.\pi+\psi_0\varrho\}-\frac{\omega_1(\varrho)}{\psi_1\varrho}\psi_0(\varrho),$$

$$\omega_1(\varrho)\pi+\omega_0(\varrho)=\frac{\omega_1\varrho}{\psi_1\varrho}(\psi_1\varrho.\pi+\psi_0\varrho)+\omega_0(\varrho)-\frac{\omega_1(\varrho)}{\psi_1(\varrho)}\psi_0(\varrho).$$

Hence the condition that $\psi_1(\varrho)\pi+\psi_0\varrho$ may be an internal factor of

$$\varphi_3(\varrho)\pi^3+\varphi_1(\varrho)\pi^2+\varphi_1(\varrho)\pi+\varphi_0(\varrho)$$

is equivalent to the equation

$$\omega_1(\varrho)\psi_0(\varrho)-\omega_0(\varrho)\psi_1(\varrho)=0.$$

Hence, substituting for $\omega_1(\varrho)$ and $\omega_0(\varrho)$ their values, we have

$$\theta_1(\varrho)\psi_0(\varrho)-\frac{\theta_2\varrho}{\psi_1\varrho}(\psi_0(\varrho))^2+\theta_2(\varrho)\psi_0(\varrho)\varrho\frac{d}{d\varrho}\frac{1}{\psi_1\varrho}\cdot\psi_1\varrho-\theta_0(\varrho)\psi_1(\varrho)+\theta_2(\varrho)\varrho\frac{d}{d\varrho}\psi_0(\varrho)=0;$$

or, again substituting for $\theta_0(\varrho)$, $\theta_1(\varrho)$, $\theta_2(\varrho)$, we have

$$\left\{\varphi_3(\varrho)+2\varphi_3(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1\varrho}\cdot\psi_1\varrho-\frac{\varphi_3(\varrho)}{\psi_1(\varrho)}\psi_0(\varrho)\right\}\left\{\varrho\frac{d}{d\varrho}\psi_0\varrho+\psi_0(\varrho)\varrho\frac{d}{d\varrho}\cdot\frac{1}{\psi_1\varrho}\psi_1(\varrho)-\frac{(\psi_0\varrho)^2}{\psi_1\varrho}\right\}$$
$$+\psi_0\varrho\left\{\varphi_1(\varrho)+2\varphi_3(\varrho)\left(\varrho\frac{d}{d\varrho}\right)\frac{1}{\psi_1\varrho}\left(\varrho\frac{d}{d\varrho}\right)\psi_1\varrho+\varphi_3(\varrho)\left(\varrho\frac{d}{d\varrho}\right)^2\frac{1}{\psi_1\varrho}\cdot\psi_1\varrho-2\frac{\varphi_3\varrho}{\psi_1\varrho}\left(\varrho\frac{d}{d\varrho}\right)\psi_0\varrho\right\}$$
$$-\psi_1\varrho\left\{\varphi_0(\varrho)-\frac{\varphi_3\varrho}{\psi_1(\varrho)}\left(\varrho\frac{d}{d\varrho}\right)^2\psi_0(\varrho)\right\}=0.$$

Had we wished to ascertain the condition that $\psi_1(\varrho)\pi+\psi_0(\varrho)$ may be an internal factor of

$$\varphi_4(\varrho)\pi^4+\varphi_3(\varrho)\pi^3+\varphi_2(\varrho)\pi^2+\varphi_1(\varrho)\pi+\varphi_0(\varrho),$$

we must have calculated the value of $\pi^3\varphi(\varrho)$. It is evident that for every increase in the degree of the highest power of (π), the labour of the investigation becomes immensely greater, and the result far more complicated. It is, however, of considerable utility in the integration of differential equations, and we shall refer to it again at the close of this paper.

SECTION II. *On some General Theorems.*

I shall now give some theorems in general differentiation and expansion.

Since

$$\left(\frac{1}{\varrho^2}\cdot\pi\right)^n=\left(\frac{1}{\varrho^2}\cdot\pi\right)\left(\frac{1}{\varrho^2}\pi\right)\left(\frac{1}{\varrho^2}\cdot\pi\right)\ldots$$

to n factors, we have

$$\left(\frac{1}{\varrho^2}\cdot\pi\right)^n=\frac{1}{\varrho^{2n}}\pi(\pi-2)(\pi-4)\ldots.(\pi-2n+2),$$

$$\therefore\ \left(\frac{1}{\varrho^2}\cdot\pi\right)^n\cdot\frac{1}{\varrho}=\frac{1}{\varrho^{2n+1}}(\pi-1)(\pi-3)\ldots(\pi-2n+1);$$

whence we easily see that

$$\pi(\pi-1)(\pi-2)(\pi-3)\ldots(\pi-2n+1)=\varrho^{2n+1}\left(\frac{1}{\varrho^2}\pi\right)^n\varrho^{2n-1}\left(\frac{1}{\varrho^2}\pi\right)^n,$$

$$\frac{1}{\varrho^{2n}}\pi(\pi-1)(\pi-2)(\pi-3)\ldots(\pi-2n+1)=\varrho\left(\frac{1}{\varrho^2}\pi\right)^n\varrho^{2n-1}\left(\frac{1}{\varrho^2}\pi\right)^n,$$

$$\therefore\ \left(\frac{1}{\varrho}\cdot\pi\right)^{2n}=\varrho\left(\frac{1}{\varrho^2}\cdot\pi\right)^n\varrho^{2n-1}\left(\frac{1}{\varrho^2}\cdot\pi\right)^n;$$

whence we shall have

$$\frac{d^{2n}u}{dx^{2n}}=x\left(\frac{1}{x}\cdot\frac{d}{dx}\right)^n x^{2n-1}\left(\frac{1}{x}\cdot\frac{d}{dx}\right)^n u.$$

If we equate the coefficients of z^r in $(1+z)^{2n}=(1+z)^n(z+1)^n$, we have

$$\frac{2n(2n-1)(2n-2)\ldots(2n-r+1)}{1.2.3\ldots r}=\frac{n(n-1)(n-2)\ldots(n-r+1)}{1.2.3\ldots r}$$

$$+\frac{n(n-1)(n-2)\ldots(n-r+2)}{1.2.3\ldots r-1}\cdot n+\frac{n(n-1)\ldots(n-r+3)}{1.2.3\ldots r-2}\cdot\frac{n(n-1)}{1.2}+\ldots;$$

$$\therefore\ 2\pi(2\pi-1)(2\pi-2)\ldots.(2\pi-r+1)$$

$$=\pi(\pi-1)\ldots(\pi-r+1)+r\pi(\pi-1)(\pi-2)\ldots(\pi-r+2)\pi$$

$$+r\frac{r-1}{2}\pi(\pi-1)(\pi-2)\ldots(\pi-r+3).\pi(\pi-1).$$

Hence, since

$$\left(\frac{1}{\varrho^{\frac{1}{2}}}\pi\right)^r=\frac{1}{\varrho^{\frac{r}{2}}}\pi\left(\pi-\frac{1}{2}\right)(\pi-1)\ldots.\left(\pi-\frac{r-1}{2}\right),$$

we have

$$2^r\varrho^{\frac{r}{2}}\left(\frac{1}{\varrho^{\frac{1}{2}}}\pi\right)^r=\varrho^r\left(\frac{1}{\varrho}\pi\right)^r+r\varrho^{r-1}\left(\frac{1}{\varrho}\pi\right)^{r-1}\varrho\left(\frac{1}{\varrho}\pi\right)$$

$$+r\frac{r-1}{2}\varrho^{r-2}\left(\frac{1}{\varrho}\pi\right)^{r-2}\varrho^2\left(\frac{1}{\varrho}\pi\right)^2+\&c.;$$

whence we find

$$x^r\left(\frac{d}{d.x^2}\right)^r u=x^r\frac{d^r u}{dx^r}+rx^{r-1}\frac{d^{r-1}}{dx^{r-1}}x\frac{du}{dx}+r\frac{r-1}{2}x^{r-2}\frac{d^{r-2}}{dx^{r-2}}x^2\frac{d^2u}{dx^2}+\&c.*$$

* It has been pointed out to me that this theorem might be more shortly proved by applying VANDERMONDE's theorem to the equation $(2D)^r=(D+D)^r$. I have retained the demonstration in the text merely for the sake of the method.

I now come to the theorems respecting expansion, which I mentioned in the beginning of the paper as analogous to the binomial and multinomial theorems in ordinary algebra.

To expand $(\varrho^2+\varrho\theta(\pi))^n$ in powers of π, where $\theta(\pi)$ is a function of (π), and (n) is a positive integer.

Let us assume

$$(\varrho^2+\varrho\theta(\pi))^n=\varphi_n^{(0)}(\varrho)+\varphi_n^{(1)}(\varrho).\pi+\varphi_n^{(2)}(\varrho)\pi^2+\&c.,$$

where

$$\varphi_n^{(0)}(\varrho)=\varrho^{2n}+A_n^{(1)}\varrho^{2n-1}+A_n^{(2)}\varrho^{2n-2}+\&c.$$

$$\varphi_n^{(1)}\varrho \quad =B_n^{(0)}\varrho^{2n-1}+B_n^{(1)}\varrho^{2n-2}+B_n^{(2)}\ \varrho^{2n-3}+\&c.;$$

$$\therefore\ \varphi_{n+1}^{(0)}\varrho =\varrho^{2n+2}+\ A_{n+1}^{(1)}\varrho^{2n+1}+\qquad A_{n+1}^{(2)}\varrho^{2n}+\&c.$$

$$=\varrho^{2n+2}+\ A_n^{(1)}\varrho^{2n+1}+\qquad A_n^{(2)}\varrho^{2n}+\&c.$$

$$+\ \theta(2n)\varrho^{2n+1}+A_n^{(1)}\theta(2n-1)\varrho^{2n}+\&c.;$$

$$\therefore\ A_{n+1}^{(1)}=A_n^{(1)}+\theta(2n),\ A_{n+1}^{(2)}=A_n^{(2)}+A_n^{(1)}\theta(2n-1);$$

or

$$A_n^{(1)}=\Sigma\theta(2n),\ A_n^{(2)}=\Sigma(\theta(2n-1)\Sigma\theta(2n)).$$

Similarly,

$$A_n^{(3)}=\Sigma\{\theta(2n-2)\Sigma(\theta(2n-1)\Sigma\theta(2n))\}\ldots\&c.$$

Again, we shall have

$$\varphi_{n+1}^{(1)}(\varrho)=B_{n+1}^{(0)}\ \varrho^{2n+1}+\qquad B_{n+1}^{(1)}\varrho^{2n}+\qquad B_{n+1}^{(2)}\varrho^{2n-1}+\&c.$$

$$=\theta'(2n)\varrho^{2n+1}+A_n^{(1)}\theta'(2n-1)\varrho^{2n}+A_n^{(2)}\theta'(2n-2)\varrho^{2n-1}+..$$

$$+B_n^{(0)}\ \varrho^{2n+1}+\qquad B_n^{(1)}\varrho^{2n}+\qquad B_n^{(2)}\varrho^{2n-1}+..$$

$$+B_n^{(0)}\theta(2n-1)\ \varrho^{2n}+B_n^{(1)}\theta(2n-2)\ \varrho^{2n-1}+\&c.$$

Consequently

$$B_{n+1}^{(0)}=B_n^{(0)}+\theta'(2n);\qquad \therefore\ B_{(n)}^{0}=\Sigma\theta'(2n)$$

$$B_{n+1}^{(1)}=B_n^{(1)}+B_n^{(0)}\theta(2n-1)+A_n^{(1)}\theta'(2n-1);$$

$$\therefore B_n^{(1)}=\Sigma(\theta(2n-1)\Sigma\theta'(2n))+\Sigma\theta'(2n-1)\Sigma\theta(2n)).$$

Hence we shall have

$$(\varrho^2+\varrho\theta(\pi))^n=\varrho^{2n}+\Sigma\theta(2n).\varrho^{2n-1}$$

$$+\Sigma\theta(2n-1)\Sigma\theta(2n)\varrho^{2n-2}+\Sigma\theta(2n-2)\Sigma\theta(2n-1)\Sigma\theta(2n)\varrho^{2n-3}+\&c.$$

$$+\{\Sigma\theta'(2n)\varrho^{2n-1}+(\Sigma\theta(2n-1)\Sigma\theta'(2n)+\Sigma\theta'(2n-1)\Sigma\theta(2n))\varrho^{2n-2}+\&c.\}\pi+\&c.$$

When $\theta(\pi)$ is a rational and entire function of (π),

$$\Sigma\theta(2n),\quad \Sigma(\theta(2n-1)\Sigma(2n))\ \&c.;\ \text{and}\ \Sigma\theta'(2n)\,\&c.$$

can always be obtained in finite terms, as manifestly ought to be the case.

In like manner we shall have

$$\left(\varrho+\frac{1}{\varrho}\theta(\pi)\right)^n=\varrho^n+\Sigma\theta(n)\varrho^{n-2}$$

$$+\Sigma(\theta(n-2)\Sigma\theta(n))\varrho^{n-4}+\Sigma\{\theta(n-4)\Sigma(\theta(n-2)\Sigma\theta(n))\}\varrho^{n-6}+\&c.$$
$$+\{\Sigma\theta'(n)\varrho^{n-3}+(\Sigma\theta'(n-2)\Sigma\theta(n)+\Sigma\theta(n-2)\Sigma\theta'(n))\varrho^{n-4}+\&c.\}\pi+\&c.;$$

and also

$$\left(\varrho^2+\frac{1}{\varrho}\theta(\pi)\right)^n=\varrho^{2n}+\Sigma\theta(2n)\varrho^{2n-3}$$
$$+\Sigma(\theta(2n-3)\Sigma\theta(2n))\varrho^{2n-6}+\Sigma\{\theta(2n-6)\Sigma(\theta(2n-3)\Sigma\theta(2n))\}\varrho^{2n-9}+\&c.$$
$$+\{\Sigma\theta'(2n)\varrho^{2n-3}+(\Sigma\theta'(2n-3)\Sigma\theta(2n)+\Sigma\theta(2n-3)\Sigma\theta'(2n))\varrho^{2n-6}+\&c.\}\pi+\&c.$$

If we put $\theta(\pi)=\pi^3$, it is obvious that the three last theorems will give us the expansions of

$$\left(x^3+x^3\frac{d}{dx}+x^3\frac{d^2}{dx^2}\right)^n,\quad \left(x+\frac{d}{dx}+x\frac{d^2}{dx^2}\right)^n,\quad \text{and of}\quad \left(x^3+\frac{d}{dx}+x\frac{d^2}{dx^2}\right)^n$$

in terms of $x\frac{d}{dx}$.

The same methods of course will apply to all binomials included under the form $(\varrho^n+\varrho^p\theta(\pi))^n$. I have found that there is no difficulty in calculating the forms of the coefficients, beyond the labour expended in performing the finite integrations.

To determine that part of the expansion of $(\varrho^n+\varrho^{n-1}\theta_1(\pi)+\varrho^{n-2}\theta_2(\pi)+\varrho^{n-3}\theta_3(\pi)+\&c.)^n$ which is independent of π.

Let us assume

$$(\varrho^n+\varrho^{n-1}\theta_1(\pi)+\varrho^{n-2}\theta_2(\pi)+\varrho^{n-3}\theta_3(\pi)+\ldots)^n=\varphi_n^{(0)}(\varrho)+\varphi_n^{(1)}(\varrho).\pi+\varphi_n^{(2)}(\varrho).\pi^2+\ldots,$$

where

$$\varphi_n^{(0)}(\varrho)=\varrho^{2n}+A_n^{(1)}\varrho^{2n-1}+A_n^{(2)}\varrho^{2n-2}+A_n^{(3)}\varrho^{2n-3}+\ldots$$

Then we shall have

$$\begin{aligned}\varphi_{n+1}^{(0)}(\varrho)&=\varrho^{2n+2}+A_{n+1}^{(1)}\varrho^{2n+2-1}+A_{n+1}^{(2)}\varrho^{2n+2-2}+A_{n+1}^{(3)}\varrho^{2n+2-3}+\ldots\\&=\varrho^{2n+2}+A_n^{(1)}\varrho^{2n+2-1}+A_n^{(2)}\varrho^{2n+2-2}+A_n^{(3)}\varrho^{2n+2-3}+\ldots\\&\quad+\theta_1(\alpha n)\varrho^{2n+2-1}+A_n^{(1)}\theta_1(\alpha n-1)\varrho^{2n+2-3}+A_n^{(2)}\theta_1(\alpha n-2)\varrho^{2n+2-3}+\ldots\\&\quad+\theta_2(\alpha n)\varrho^{2n+2-3}+A_n^{(1)}\theta_2(\alpha n-1)\varrho^{2n+2-3}+\ldots\\&\quad+\theta_3(\alpha n)\varrho^{2n+2-3}+\ldots\end{aligned}$$

$$\therefore A_{n+1}^{(1)}=A_n^{(1)}+\theta_1(\alpha n)$$
$$A_{n+1}^{(2)}=A_n^{(2)}+A_n^{(1)}\theta_1(\alpha n-1)+\theta_2(\alpha n)$$
$$A_{n+1}^{(3)}=A_n^{(3)}+A_n^{(2)}\theta_1(\alpha n-2)+A_n^{(1)}\theta_2(\alpha n-1)+\theta_3(\alpha n)$$
$$\&c.\ =\ \&c.;$$

$$\therefore A_n^{(1)}=\Sigma\theta_1(\alpha n)$$
$$A_n^{(2)}=\Sigma\theta_1(\alpha n-1)\Sigma\theta_1(\alpha n)+\Sigma\theta_2(\alpha n)$$
$$A_n^{(3)}=\Sigma\theta_1(\alpha n-2)\Sigma\theta_1(\alpha n-1)\Sigma\theta_1(\alpha n)$$
$$+\Sigma\theta_1(\alpha n-2)\Sigma\theta_2(\alpha n)+\Sigma\theta_2(\alpha n-1)\Sigma\theta_1(\alpha n)+\Sigma\theta_3(\alpha n);$$

and consequently the part of

$$(\varrho^n+\varrho^{n-1}\theta_1(\pi)+\varrho^{n-2}\theta_2(\pi)+\ldots)^n,$$

which is independent of (π), is

$$\varrho^{an}+\Sigma\vartheta_1(\alpha n)\varrho^{an-1}+(\Sigma\vartheta_1(\alpha n-1)\Sigma\vartheta_1(\alpha n)+\Sigma\vartheta_2(\alpha n))\varrho^{an-2}$$
$$+(\Sigma\vartheta_1(\alpha n-2)\Sigma\vartheta_1(\alpha n-1)\Sigma\vartheta_1(\alpha n)+\Sigma\vartheta_1(\alpha n-2)\Sigma\vartheta_2(\alpha n)$$
$$+\Sigma\vartheta_2(\alpha n-1)\Sigma\vartheta_1(\alpha n)+\Sigma\vartheta_2(\alpha n))\varrho^{an-3}+\ldots$$

SECTION III. *On the Solution of Linear Differential Equations with Variable Coefficients.*

The general linear differential equation

$$X_r\frac{d^ru}{dx^r}+X_{r-1}\frac{d^{r-1}u}{dx_{r-1}}+X_{r-2}\frac{d^{r-2}u}{dx^{r-2}}+\&c.=X,$$

where X_r, X_{r-1} are rational and entire functions of (x), may, as Professor BOOLE has shown, be always expressed in the symbolical form

$$\varrho^n\phi_n(\pi)u+\varrho^{n-1}\phi_{n-1}(\pi)u+\ldots\varrho\phi_1(\pi)u+\phi_0(\pi)u=X,$$

where

$$\varrho=x, \text{ and } \pi=x\frac{d}{dx},$$

and $\phi_n(\pi)$, $\phi_{n-1}(\pi)$, &c. are rational and entire functions of (π).

Suppose that by using the methods explained in this paper, we are able to reduce this equation to the form

$$(\varrho\psi_1^{(n)}(\pi)+\psi_0^{(n)}(\pi))(\varrho\psi_1^{(n-1)}(\pi)+\psi_0^{(n-1)}(\pi))\ldots(\varrho\psi_1(\pi)+\psi_0(\pi))u=X\text{*}.$$

Assume

$$\begin{aligned}
&\varrho\psi_1^{(n)}\ (\pi)u_{n-1}+\psi_0^{(n)}\ (\pi)u_{n-1}=X\\
&\varrho\psi_1^{(n-1)}(\pi)u_{n-2}+\psi_0^{(n-1)}(\pi)u_{n-2}=u_{n-1}\\
&\varrho\psi_1^{(n-2)}(\pi)u_{n-3}+\psi_0^{(n-2)}(\pi)u_{n-3}=u_{n-2}\\
&\qquad\&c.\qquad\qquad\qquad=\&c.\\
&\varrho\psi_1(\pi)\quad u\quad+\psi_0\quad(\pi)u\quad=u_1.
\end{aligned}$$

We thus reduce the proposed differential equation to forms already treated of by Professor BOOLE.

We may much simplify the process already explained for treating the symbolical quantity $\varrho^n\phi_n(\pi)+\&c.+\phi_0(\pi)$, by remarking that $\psi_1(\pi)$ must be sought among the divisors of $\phi_n(\pi)$, $\psi_0(\pi)$ among the divisors of $\phi_0(\pi)$; and we shall make use of this principle in the following application of the preceding theory to the solution of differential equations.

We shall denominate the equation deduced in the former part of this memoir,

$$\phi_0(\pi)-\frac{\psi_0(\pi)}{\psi_1(\pi-1)}\phi_1(\pi-1)+\frac{\psi_0(\pi)\psi_0(\pi-1)}{\psi_1(\pi-1)\psi_1(\pi-2)}\phi_2(\pi-2)-\&c.=0,$$

the criterion of the factor $\varrho\psi_1(\pi)+\psi_0(\pi)$.

* It may be proper to remind the reader that $\psi_1^{(n)}(\pi)$, $\psi^{(n-1)}(\pi)$, &c. have no reference to the functions derived from $\psi_1\pi$ by differentiation.

To integrate the differential equation,

$$x^3(x+1)^3\frac{d^3u}{dx^3}+3x(x+1)^2\frac{d^2u}{dx^2}+(x^3+4x^2+3x)\frac{du}{dx}-(2x-3)u=X.$$

The symbolical form of this equation is

$$\varrho^3\pi^3u+\varrho^2(3\pi^2+\pi-1)u+3\varrho(\pi^2+1)u+\pi(\pi^2-1)u=Xx.$$

The divisor of π^3 is π only, the divisors of $\pi(\pi^2-1)$ are $\pi-1$, π, $\pi+1$; hence putting $\psi_1(\pi)=\pi$, $\psi_0\pi=\pi-1$, we find the criterion of the symbolical quantity $\varrho\psi_1(\pi)+\psi_0(\pi)$ to become

$$\pi(\pi^2-1)-3\{(\pi-1)^2+1\}+\{3(\pi-2)^2+(\pi-2)-1\}-(\pi-3)^2=0,$$

an identical equation.

Hence $\varrho\pi+(\pi-1)$ is an internal factor of

$$\varrho^3\pi^3+\varrho^2(3\pi^2+\pi-1)+3\varrho(\pi^2+1)+\pi(\pi^2-1);$$

and the equation may be written, effecting the internal division,

$$\{\varrho^2(\pi-1)^2+\varrho(\pi+1)(2\pi-3)+\pi(\pi+1)\}(\varrho\pi+(\pi-1))u=Xx;$$

or if $\varrho\pi+(\pi-1)u=u_1$,

$$\{\varrho^2(\pi-1)^2+\varrho(\pi+1)(2\pi-3)+\pi(\pi+1)\}u_1=Xx.$$

The only divisor of $(\pi-1)^2$ is $\pi-1$, the divisors of $\pi(\pi+1)$ are π and $\pi+1$; and by trial it is found that the divisor $\varrho(\pi-1)+(\pi+1)$ satisfies the criterion, and is therefore an internal factor. Hence, effecting the internal division,

$$(\varrho(\pi-2)+\pi)(\varrho(\pi-1)+(\pi+1))u_1=Xx,$$

and the differential equation becomes

$$(\varrho(\pi-2)+\pi)(\varrho(\pi-1)+(\pi+1))(\varrho\pi+(\pi-1))u=Xx,$$

or

$$\left\{(x^2+x)\frac{d}{dx}-2x\right\}\left\{(x^2+x)\frac{d}{dx}-(x-1)\right\}\left\{(x^2+x)\frac{d}{dx}-1\right\}u=Xx.$$

Hence, performing the inverse calculations, we find for the complete integral;

$$u=\frac{x}{x+1}\int\frac{dx(x+1)^2}{x^3}\int\frac{dx}{x+1}\int\frac{Xdx}{(x+1)^3},$$

the three arbitrary constants being included under the signs of integration.

In case this method does not succeed, we may sometimes resolve the symbolical function into factors by assuming $u=(\pi+\xi)v$ and proceeding as before, determining (α) from the criterion, as will be shown in the following examples:—

To integrate the differential equation

$$x^2(x+1)^3\frac{d^3u}{dx^3}+x(4x^2+11x^2+10x+3)\frac{du}{dx}+2x^3+10x^2+5x-3=X.$$

The symbolical form of the equation is

$$\varrho^3(\pi^3+3\pi+2)+\varrho^2(3\pi^2+8\pi+10)+\varrho(3\pi^2+7\pi+5)+\pi^2+2\pi-3=X.$$

Let $u=(\pi+\xi)v$, and the equation becomes

$$\varrho^3(\pi+1)(\pi+2)(\pi+\xi)v+\varrho^2(3\pi^3+8\pi+10)(\pi+\xi)v$$
$$+\varrho(3\pi^3+7\pi+5)(\pi+\xi)v+(\pi-1)(\pi+3)(\pi+\xi)v=X.$$

Let $\psi_1(\pi)=\pi+2$, $\psi_2(\pi)=\pi+\xi$, then the criterion of $\varrho(\pi+2)+(\pi+\xi)$ becomes

$$(\pi^3+2\pi-3)(\pi+\xi)-\frac{\pi+\xi}{\pi+1}(3\pi^3+\pi+1)(\pi+\xi-1)$$
$$+\frac{(\pi+\xi)(\pi+\xi-1)}{(\pi+1)\pi}(3\pi^3-4\pi+6)(\pi+\xi-2)$$
$$-\frac{(\pi+\xi)(\pi+\xi-1)(\pi+\xi-2)}{(\pi+1)\pi(\pi-1)}(\pi^3-3\pi+2)(\pi+\xi-3)=0.$$

Put $\pi=0$ to determine ξ, and we have $\xi=0$ as one value of ξ, which on trial is found to satisfy the proposed.

Hence $\varrho(\pi+2)+\pi$ is an internal factor of the symbolical function

$$\varrho^3\pi(\pi+1)(\pi+2)+\varrho^2(3\pi^3+8\pi^2+10\pi)$$
$$+\varrho(3\pi^3+7\pi^2+5\pi)+\pi(\pi-1)(\pi+3).$$

Wherefore, effecting the internal division, the equation becomes

$$(\varrho^3\pi+\varrho(2\pi+3)+\pi+3)(\pi-1)(\varrho(\pi+2)+\pi)v=X,$$

whence performing the inverse calculations, we have

$$v=\frac{1}{(x+1)^3}\int dx(x+1)\int\frac{dx(x+1)^3}{x^5}\int\frac{dx.x^3.X}{(x+1)^5};$$

$$\therefore\quad u=x\frac{d}{dx}\left\{\frac{1}{(x+1)^3}\int dx(x+1)\int\frac{dx(x+1)^3}{x^5}\int\frac{dx.x^3X}{(x+1)^5}\right\},$$

where the arbitrary constants must be reduced to two.

Next consider the differential equation

$$(x^4+2x^3+x^2)\frac{d^2u}{dx^2}-6(x^3+x)\frac{du}{dx}+6(x+2)u=X;$$

the symbolical form of this equation is

$$\varrho^3\pi(\pi-1)u+2\varrho(\pi-1)(\pi-3)u+(\pi-3)(\pi-4)u=X.$$

Let $u=(\pi+\xi)v$, and the equation becomes

$$\varrho^3\pi(\pi-1)(\pi+\xi)v+2\varrho(\pi-1)(\pi-3)(\pi+\xi)v+(\pi-3)(\pi-4)(\pi+\xi)v=X.$$

Let $\psi_1(\pi)=\pi-1$, $\psi_2(\pi)=\pi-3$, and the criterion becomes

$$(\pi-3)(\pi-4)(\pi+\xi)-\frac{\pi-3}{\pi-2}\{2(\pi-2)(\pi-4))(\pi+\xi-1)$$
$$+\frac{(\pi-3)(\pi-4)}{(\pi-2)(\pi-3)}\{(\pi-2)(\pi-3)\}(\pi+\xi-2)=0.$$

Putting $\pi=0$ in this equation, we have $\xi=0$, and this value renders the above equation identical,

$$\therefore\quad \varrho(\pi-1)+(\pi-3)$$

is an internal factor of the symbolical function

$$\varrho^3\pi^3(\pi-1)+2\varrho\pi(\pi-1)(\pi-3)+\pi(\pi-3)(\pi-4);$$

wherefore, effecting the internal division, the equation becomes

$$\{\varrho(\pi-1)^3+\pi(\pi-4)\}\{\varrho(\pi-1)+(\pi-3)\}u=X.$$

This equation may be written

$$\left\{\frac{(\pi-2)(\pi-3)(\pi-4)}{\pi-1}\right\}(\varrho+1)\left\{\frac{\pi(\pi-1)}{(\pi-2)(\pi-3)}\right\}(\varrho(\pi-1)+(\pi-3))=X,$$

in which the inverse calculations are all practicable.

As a final example we take the differential equation

$$(x^5+4x^4+5x^3+2x^2)\frac{d^2u}{dx^2}+(2x^4+3x^3+5x^2-6x)\frac{du}{dx}+(x+1)^2u=X.$$

The symbolical form of this equation is

$$\varrho^3\pi(\pi+1)u+\varrho^2(4\pi^3-\pi+1)u+\varrho(5\pi^3-5\pi+2)u+(\pi-1)(2\pi-1)u=X.$$

If we put $u=\pi v$, the equation becomes

$$(\varrho+1)(\pi-1)(\varrho(\pi-1)+\pi)(\varrho\pi+(2\pi-1))v=X,$$

in which the inverse calculations necessary for the solution of the equation are all practicable.

In cases where the assumption $u=(\pi+\xi)v$ does not lead to the solution of the equation, we may assume $u=(\pi+\xi_1)(\pi+\xi_2)v$, and proceed as before.

We may also treat linear differential equations by ascertaining the condition that $\psi_1(\varrho)\pi+\psi_0(\varrho)$ may be an internal factor of this symbolical expression,

$$\varphi_n(\varrho).\pi^n+\varphi_{n-1}(\varrho)\pi^{n-1}+\text{\&c.}+\varphi_1(\varrho)\pi+\varphi_0(\varrho).$$

I have shown how this is to be effected when $n=2$ or 3.

For higher degrees the investigation would be very laborious. In all cases in which the second member of the differential equation is zero, this internal factor, supposing it to exist, would conduct us to a particular integral.

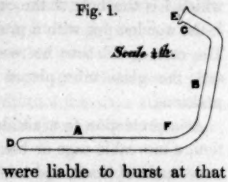
VI. *On the Properties of Liquid Carbonic Acid.* By G. Gore, *Esq.* *Communicated by Professor* Tyndall, *F.R.S.*

Received January 17,—Read January 24, 1861.

The following experiments were undertaken with a view of adding to the scanty stock of information which at present exists respecting the properties of liquid carbonic acid.

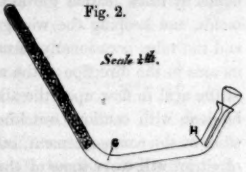
To ascertain the action of the liquid acid upon solid substances, the following method was in nearly all cases adopted. A piece of flint-glass tubing, 12 inches long, $\frac{3}{8}$ths of an inch external diameter, and $\frac{7}{32}$nds of an inch internal diameter, was bent to the annexed figure; the part A being about $6\frac{1}{2}$ inches, the part B about 4 inches, and the part C about $1\frac{1}{2}$ inch long. In pieces of glass tubing the bore is rarely of one uniform diameter, and in these experiments the smaller diameter was always employed for the end C, otherwise the tubes were liable to burst at that part. The end D was closed by fusion, and the end E was formed with a flange and left open. It is essential that the ends and bent parts of the tubes be well annealed immediately upon their formation.

Fig. 1.



The lower limb A of the tube was filled to the point F with moderately small fragments of sesquicarbonate of ammonia, and the last fragments fixed firmly in their position by pushing a slender rod of gutta percha into the tube forcibly against them. If the carbonate was employed in a state of powder or very small fragments, the tubes frequently became choked by sulphate of ammonia, and the generation of carbonic acid was soon completely arrested. The tube being now placed in the position of figure 2, was filled from G to H with pure sulphuric acid by means of a small glass funnel.

Fig. 2.



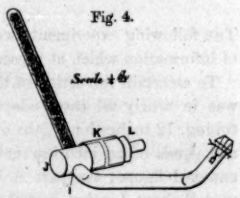
A taper plug $\frac{3}{4}$ths of an inch long was made by softening the end of a solid rod of gutta percha, $\frac{7}{32}$nds of an inch thick, in boiling water, and allowing it to cool. Two notches were cut transversely to each other across the larger end of the plug to receive binding wires, and the smaller end of the plug was coated with melted paraffin to protect the gutta percha from contact with the liquid carbonic acid. A small glass cup, of the form and dimensions of figure 3, was made of thin glass tubing, and fixed (by previously heating the small and solid end) in a small hole previously made in the centre of the small end of the plug.

Fig. 3.

A small quantity or fragment of the solid substance to be subjected to the action of the liquid acid was placed securely in this tube, the sides of the plug welted with a saturated solution of paraffin in chloroform, and the stopper strongly forced while slippery into the mouth of the tube, taking care not to jerk or force any of the sulphuric acid into contact with the alkaline carbonate. The plug was then fastened down tightly by means of the two transverse binding wires, each of which was composed of a twisted strand of four "No. 30" copper wires. It is important that a plug of the exact diameter be carefully selected.

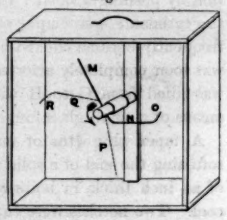
For convenience of manipulation, the tube was placed in the hole of a transversely perforated cork; the cork fitted rather loosely in a short piece of brass tube; the tube fixed upon a wooden peg, as shown in figure 4, in which I is the hole, J the cork, K the brass tube, and L the wooden peg with a prolongation of less diameter. The cork could thus be readily turned upon its axis, and the glass tube placed and retained in various positions.

Fig. 4.



As a protection from accidents by explosion, and for further convenience of manipulation, a moveable cage or box was constructed for each tube; its sides, top, and bottom were made of wood, and its front and back were of fine iron wire-gauze; its dimensions were 12 inches high, 12 inches wide, and 5 inches from front to back; the pieces of gauze were nailed to the wood on one of their vertical edges only, so that they served the purpose of doors. A small vertical strip or piece of wood was fixed at the back part of the box, with a hole at its middle part to receive tightly the small projecting part of the horizontal peg L, fig. 4. An inspection of fig. 5 will make more clear the whole arrangement.

Fig. 5.



The tube, charged with its acid and carbonate, is placed in the frame in the position shown by the lines M, N, O in fig. 5; and the operator having previously protected his hands by thick leather gloves, his eyes by a pair of spectacles, and keeping the wire-gauze door between his face and the tube, occasionally turns the supporting cork upon its axis in the direction of the arrow, so as to cause a little of the acid to flow upon the alkaline carbonate; this must be done with cautious watching and in very small quantities at the commencement, otherwise the bubbles of gas which ascend through the oil of vitriol will carry some of the latter acid into contact with the contents of the little glass cup. The process requires much watching; and if at any time the chemical action is allowed to progress too rapidly, the generating tube is liable to burst in consequence of the heat evolved.

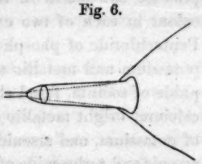
The tube is thus occasionally turned until after the lapse of several hours it has

acquired the permanent position indicated by the dotted lines P, Q, R in fig. 5, when a piece of cotton-wool should be placed upon the stoppered end and saturated with ether, so as to distil off the liberated carbonic acid, and the application of ether be repeated at intervals until nearly the whole of the carbonate is decomposed and the stoppered end of the tube is full of liquid carbonic acid. If the experiment has succeeded well, a length of the tube, equal to 1 or $1\frac{1}{4}$ inch, will, in cold weather, be filled with the liquid acid. By occasionally (once a day or less) applying the ether to the stoppered end of the tube, that part may be kept full, or partly full, of liquid acid for a long time; I have kept it in this manner during several months.

The most frequent cause of failure is clogging of the tube with sulphate of ammonia; it is rarely that a leakage occurs at the stopper; and the most frequent causes of explosion are, too rapid generation of the gas, and increased temperature of the atmosphere. It is highly advisable never to examine a charged tube without the wire-gauze intervening to intercept fragments of glass, and to use a large moveable screen of glass to protect the eyes from projected oil of vitriol. Nearly all the explosions which occur take place during the process of generating carbonic acid, or within a few days afterwards. The proportion of tubes fractured or burst at different stages of the process is about one-third.

In some experiments, where electric sparks were passed through the liquid acid, two longitudinal cuts, $\frac{1}{10}$th of an inch deep, were made by a knife on opposite sides of the gutta-percha plug, extending from its smaller nearly to its larger end, before inserting it in the tube; and two fine platinum wires inserted into the cuts and secured very carefully by means of a heated penknife; the inner end of the plug was then coated with melted paraffin, the plug wetted with paraffin solution, and inserted in the usual manner. The wires extended nearly $\frac{1}{8}$ an inch within the tube, and were imbedded for about $\frac{1}{4}$ of an inch next to the plug in paraffin, the remaining part served for the electrodes. (See fig. 6.) After inserting the plug and securing it by the binding wires, the whole of the outer end of the plug was freely coated with melted paraffin, to insulate more perfectly the electrodes from each other.

Fig. 6.



In a first experiment, with the electrodes $\frac{1}{12}$th of an inch asunder, and a sensitive galvanometer in the circuit, very feeble and variable conduction occurred with 30 Smee's batteries; but this was found to arise from absorption of a trace of sulphuric acid or saline liquid which had adhered to that end of the generating tube. In a second experiment, the electrodes being $\frac{1}{10}$th of an inch apart, and the liquid acid below 32° Fahr., not the slightest conduction occurred with 40 Smee's batteries; and sparks from a Ruhmkorff's coil which passed through $\frac{3}{8}$ths of an inch of cold air would not pass through the liquid acid. In a third experiment, with electrodes about $\frac{1}{10}$th of an inch apart, sparks from the coil, which were passing freely through $\frac{5}{32}$nds of an inch of cold air, occasionally passed through the cold

liquid acid and exhibited a pale blue colour. And in a fourth experiment, the tube being charged with sulphuric acid diluted with an equal bulk of water, and with an anode of wood-charcoal $\frac{1}{8}$th of an inch distant from a cathode of platinum wire, and 40 SMEE's batteries, faint conduction occurred, probably in consequence of the presence of a trace of water from the dilute acid; the deflection of the galvanometer needles was 21°, but the conduction entirely ceased in twelve hours, and then, upon connecting the electrodes with the secondary wire of the coil, no sparks passed between them.

These experiments show that liquid carbonic acid is a strong insulator of electricity, and that when prepared with concentrated oil of vitriol, and with the precautions stated, it is free from water, sulphuric acid, and sulphate of ammonia. As further proofs of the freedom of the liquid acid from oil of vitriol and water, it may be mentioned,—1st, that dry extract of litmus exhibited no signs of redness by immersion in the liquid acid; 2ndly, a small fragment of glacial phosphoric acid did not appear at all liquefied, nor lost the sharpness of its edges after being immersed several weeks; and 3rdly, anhydrous sulphate of copper did not become at all blue in the liquid acid.

The following are the results obtained with various solid substances immersed in the liquid acid; some were immersed during several months, many during several weeks, and a few only during several days. The temperature of the liquid acid was generally a little below that of the external atmosphere. Wood-charcoal remained undissolved and unaltered. Anhydrous boracic acid in powder slightly dissolved. White phosphorus slightly dissolved. Glacial phosphoric acid, green solid biphosphide of hydrogen, and ordinary yellow sulphur, undissolved and unchanged. (Bisulphide of carbon absorbs *gaseous* carbonic acid.) Sulphide of phosphorus, and selenium, undissolved and unaltered. Iodine, biniodide of phosphorus, and iodide of sulphur, all dissolved in small quantities (iodine the most freely), and formed red or reddish solutions. Anhydrous hydrochloric acid (prepared by first half-filling the limb A of the tube with sesquicarbonate of ammonia, and then filling the remaining half with hydrochlorate of ammonia) did not produce two strata in the condensed liquid, but imparted to the liquid a brownish colour in each of two experiments, probably by acting upon the gutta-percha stopper. Pentachloride of phosphorus dissolved slowly and formed a colourless liquid. Metallic potassium and metallic sodium slowly acquired bulky white coatings of alkali. Phosphide of sodium, fused chloride of sodium, phosphide of calcium, anhydrous chloride of calcium, bright metallic aluminium, crystals of silicium, anhydrous silica, silico-fluoride of potassium, and arsenic acid, all remained undissolved and unaltered. Terbromide of arsenic and terbromide of antimony, each dissolved slightly. Hydrated crystals of monosulphate of iron became dehydrated and fell to pieces as a white powder, and did not dissolve. Anhydrous sulphate of copper remained white and undissolved; the hydrated salt became white. Protochloride of mercury and nitrate of silver remained undissolved and unaltered.

Cyanide of mercury, oxalic acid, benzoic acid, succinic acid, pyrogallic acid, gallic acid, tannic acid, tannic acid and potash, paraffin, and cocostearic acid remained undis-

solved and unaltered in appearance. Pitch softened and partly dissolved. Napthalin dissolved in small quantity. Gutta percha; the liquid acid dissolved out the dark-brown colouring matter, and left the gutta percha undissolved, and much more white. India-rubber remained black externally, but became perfectly white through the whole of its thickness; on removing it from the acid it suddenly swelled to a large size, and then gradually shrank in a few hours to its original dimensions, and afterwards (in a few days) slowly regained its original colour. Common yellow resin dissolved slightly. Gum-copal remained undissolved. Camphor dissolved rapidly and formed a clear colourless liquid. (Spirit of turpentine dissolves more than its own volume of *gaseous* carbonic acid.) Spermaceti, indigo, pyroxylin, and solid extract of litmus remained undissolved and unaltered. Gamboge dissolved in minute quantity and formed a slightly-yellow liquid.

These experiments show that liquid carbonic acid is a chemically inert body, and is also a very feeble solvent of substances in general, and is not deoxidized by any of the ordinary deoxidizing agents except the alkali-metals.

The way to discharge the tubes of their contents is to support them over a gutta-percha vessel within the safety-cage, or behind a double screen of glass and wire-gauze, and, protected by gloves, cut off the binding-wires with a pair of nippers; then, if the stoppers are not blown out, pour boiling water upon them. Sometimes the explosion occurs immediately upon cutting the wires, but in most cases it requires the application of the hot water; this is a most convincing proof of the tightness of the stoppers, as the pressure of liquid carbonic acid (according to different authorities) varies from 500 to 1100 pounds per square inch, according to the temperature of the atmosphere. The tubes nearly always break by the violence of the recoil. In many instances a safer plan was adopted; the wires were not cut, but a current of steam was directed upon the stoppers until they were expelled; the discharge was then less sudden, and the tubes were less frequently broken. It would probably be a still further improvement if the lower end of the tube was closed by a stopper in a similar manner to the upper end; the contents of the tube might then be discharged at that end, and the substances operated upon would then be exposed to less risk of being lost, and of being brought into contact with the acid and saline matters by the discharge.

In an endeavour in one experiment to liberate the enclosed acid *gradually*, a small hole was made lengthwise through the stopper before inserting it; a piece of glass tubing, half an inch long and possessing a very fine bore, barely visible to the unassisted eye, was fixed in the hole at the small end of the stopper, and a small metal plug fixed in the outer end of the hole; and after generating the liquid acid the metal plug was withdrawn; nearly the whole of the liquid carbonic acid escaped through the fine aperture of the glass in about four seconds, and left the remainder in a solid state in the tube. In two instances, tubes with cracks in them half an inch in length, withstood the full pressure of the acid during several days.

VII. *On Magnetic Storms and Earth-Currents.*

By CHARLES V. WALKER, *Esq.*, *F.R.S.*, *F.R.A.S.*

Received January 31,—Read February 14, 1861.

DISTURBANCES of the needles of the electric telegraph were noticed very soon after the completion of the first working lines. They were at once seen to be due to causes exterior to the apparatus itself.

On Saturday, May 8, 1847, I find the following entry in my diary:—"At Tonbridge. Through instrument needles stood at 'wait' from 5.6 to 5.10, and hung when turned to Dover; all right when turned to London. From 6.4 to 6.19 they hung to 'go on' on the down side. The Maidstone needles also hung."

On Friday, September 24:—"Singular atmospheric day. All over the line, and in all the groups, needles were strongly affected between noon and midnight. Signals could hardly be made. Bells rang. The deflections varied. Sometimes the up-side deflections differed from the down. Memorandum for Tonbridge.—12.50 turned down, needles at E.N.; 1 P.M., vertical; 1.5, at E.N.; 1.11, +H; 1.14, vertical; 1.15, E.N. hard up; 1.17, vertical; 1.18, E.N.; 1.23, vertical. During some parts of day through-bells rang. At night evidently an extensive aurora; but being full harvest moon and clear, not very conspicuous."

On Saturday, October 23:—"Needles hung. Wind S.W. gale; driving rain; low heavy clouds; continued unfair; signals much retarded. This continued more or less during day and night, of which I have a series of notes made at Tonbridge. The needles were occasionally affected during Sunday; and at night there was a splendid aurora visible, in spite of the full moon. The sudden transitions from right to left were remarkable."

On Monday, October 25:—"The needles affected a little this morning. At 10.1 A.M. the needles went over suddenly to W.; and in the next minute changed as suddenly to R."

I copy these notes just as they stand recorded. They are expressed in the technical language of telegraphy. I shall have no occasion to refer back to them for illustration, as we have observations of recent date, carried on for long periods and under regulation, so that it is not necessary to occupy time in putting the above into common language, further than to say that when the telegraph needles are deflected to the right hand they make E.N. or R, or "wait;" and when deflected to the left, they make +H or W, or "go on;" and that a positive current passing in the telegraph wires from Dover to London causes a right-hand deflection, and one from London to Dover a left hand.

The above are specimens of disturbances that made themselves, at that period, very conspicuous. On the one hand, they were inconvenient to the good working of the telegraph; on the other, they opened out a new field of inquiry. I therefore issued the following circular to all the telegraph stations under my control, viz. those on the South-Eastern Railway:—

"General Order, No. 54. October 25, 1847. To all Telegraph Clerks.

"The needles are occasionally deflected by atmospheric causes. Whenever this occurs make accurate notes of the time, the direction, and all changes; continue these notes as long as the phenomena endure; and notice the different effects on the different instruments and on the bells. Send the notes to this office on the following morning. Those on night duty at such times are to look out for the aurora borealis, and if possible to describe it.

(Signed) "CHARLES V. WALKER,
"*Superintendent of Telegraphs.*

"Send all the notes you made on Saturday and yesterday immediately."

The instruments on which the above observations, and all that form the subject of this communication, were made, are COOKE and WHEATSTONE's well-known double-needle and single-needle telegraphs. A magnetized needle is suspended vertically in a vertical galvanometer coil; an outer needle on the same axis as the other serves as an index; its motion is limited by a pair of ivory studs, permanently fixed about an inch apart in the metal face of the instrument.

That these disturbances were connected with the appearance of aurora borealis and magnetic storms was soon recognized, as may be gathered from the extracts that have been given, as well as from the following taken from my 'Electric Telegraph Manipulation,' published March 1, 1850:—

"85. *Magnetic storms.*—Did any doubt remain of the electrical character of the *Aurora Borealis*, it would be removed by the phenomena presented by the needles of the telegraph, and often by the bells, during the prevalence of this meteor. At such times the needles move just as if a good working current were pursuing its ordinary course along the wires: they are deflected this way or that, at times with a quick motion and changing rapidly from side to side many times in a few seconds, and at other times moving more slowly and remaining deflected for many minutes with greater or less intensity, their motions being inconstant and uncertain. These phenomena have occurred less frequently on the part of the line between Reigate and Dover, which runs nearly E. and W., and on the part between London and Reigate, which is nearly N. and S. When, however, they do make their appearance on the telegraphs in those parts, we are prepared to expect auroral manifestations when night arrives; and we are rarely disappointed. The deflections, in their variations, appear to coincide with the various phases of the aurora. On the branch line running from Ashford to Ramsgate,

these deflections have been a much more common occurrence, even when the other parts of the line were unaffected, and when no auroral phenomena were noticed."—P. 66.

From the record of 'The Daily Observations of Magnetometers at the Royal Observatory, Greenwich, in the year 1847,' I make extracts, as far as they apply to the notes of disturbances quoted above.

May 8. *Declination magnet.*—"Between 4^h and 6^h the change was considerable for the time of day."

Vertical-force magnet.—"Between 0^h and $1^h\ 50^m$ and between 6^h and 8^h considerable changes occurred."

Sept. 24. *Declination magnet.*—"From 0^h to 16^h . . . . the changes were very considerable."

Horizontal-force magnet.—"The changes were very frequent and of large amount."

Vertical-force magnet.—"Between 0^h and 10^h . . . . considerable changes occurred."

Oct. 23. *Declination magnet.*—"The changes were frequent and of large amount."

Horizontal-force magnet.—"The changes were frequent and of large amount."

Vertical-force magnet.—"Between 0^h and 12^h the changes were frequent and of large amount."

October 24.—"October $23^d\ 22^h$ (*i. e.* 10 A.M. October 24). Remarkable changes having taken place in the positions of the declination and horizontal-force magnets, extra observations were commenced" [they were continued till $23^h\ 57^m\ 30^s$, *i. e.* till $11^h\ 57\frac{1}{2}^m$ A.M. October 25].

Oct. 25. *Declination magnet.*—"The changes were frequent and of large amount."

Horizontal-force magnet.—"The changes were frequent and of large amount."

Vertical-force magnet.—"The changes were frequent and of large amount."

In the 'Extraordinary Meteorological Observations' at the same place for the same year, an aurora is noted on September 24 from $9.39\frac{1}{2}$ to $10.40\frac{1}{2}$ P.M.

Seven pages are also occupied with an ample description of the magnificent aurora of Sunday, October 24. An account of this aurora, with a series of coloured engravings, was also published at Cambridge by Mr. JOHN MORGAN and Mr. T. BARBER.

The period to which I have been referring was, as is well known, a period of great magnetic disturbance. The extent to which these disturbances were manifested in the form of current electricity traversing our telegraph wires, may be gathered from the following further extract from 'Electric Telegraph Manipulation:'—

"87. *Movable studs.*—So much were we inconvenienced during 1848, that I was compelled to provide a remedy. On the face of the instruments are seen a pair of small studs, which are of ivory, and which give a limit to the deflection of the needle; and if by any extraneous cause, and without any act of ours, the needle should be driven hard up against one of the studs, our attempt to deflect it in that direction by an ordinary signal would not be manifest; for it is there already. But if under such circumstances

the studs were removed, and the needle allowed to go as far as the natural force could deflect it, we could *increase* that deflection by sending our usual currents in the proper direction; so that if the studs are made to follow the needle into its new position, and are so placed that the needle shall be half-way between them, we can still get each kind of deflection," &c.—P. 67.

I then go on to describe how I mounted the studs and made them movable. Immediate steps were taken to apply this adjustment to the existing instruments. Matters looked so serious at the time that further steps were taken. I made the coils of the galvanometers movable round their centre; and, when the needles were disturbed, moved the coils after them, as when using a sine-galvanometer, until the needle was midway between the studs and in the centre of the coil, although of course no longer vertical.

We were soon, however, as much surprised by the disappearance of these disturbances, as we had originally been by their appearance. Our arrangements for adjusting the needles were scarcely carried out when the necessity for the alterations began to disappear. Again quoting the words which conveyed our then impressions:—"86. It is most remarkable that during the year 1849 these deflections were rare and trivial, whereas in 1847 and 1848 they occurred many times on the main line, and were of almost daily occurrence on the Ramsgate branch, and to such an extent that the instruments required unusual precautions to enable them to give signals."—*Electric Telegraph Manipulation*, p. 67.

This state of things still continued to March 1, 1850, when the remarks just quoted were published; and a considerable period subsequently elapsed before any change for the worse (speaking technically) occurred; and this will account, in great measure, for the question having so long lain dormant. We did not then know, as we now do, that these disturbances have a cycle of about eleven years from the maximum period of activity to the next maximum, and that they go with the magnetic disturbances. It seemed at the time as if we had passed out of an abnormal state, which had left no evidence behind of its probable return at some distant period.

The "unusual precautions" referred to in the last extract, over and above the adjustments applied to the instruments themselves, were:—detaching the earth-wire at the respective termini of the Ramsgate branch line of telegraphs; converting the double needle into a single needle communication; employing the wire released from one needle as the return wire for the circuit of the needle retained. When the telegraph was thus cut off from all communication with the earth the disturbances ceased.

In the 'Philosophical Transactions' for the year 1849, is a paper by W. H. Barlow, Esq., "On the Spontaneous Electrical Currents observed in the Wires of the Electric Telegraph," which was read on May 25, 1848. The author not only records the simultaneous disturbances observed by him of the magnets, and of the telegraph galvanometers on September 24 and 25, and October 23, 24, and 25, 1847, but he describes also an original and very interesting set of experiments made by him at Derby upon various telegraph routes centring in that station. He regarded the stations in their absolute bearing

each on the other, and without reference to the route pursued by the telegraph wire itself in its course from station to station; and deduced from his observations that "the direction in which the currents travel ..... will be between S. 28° W. and S. 75° W.; and apparently strongest when the earth-connexions are N.E. and S.W." The great interest attached to the fact that the direction of the current depends on the relative positions of the earth-connexions, and not on the direction of the wire itself, induced him to make further observations, which confirmed that view. They were made on May 1, 2, 3, and 8, 1848. It was further ascertained that disturbances had been noticed on the short line of telegraph that was led entirely under ground from the Strand to Nine Elms. Taking this in connexion with the influence of the relative position of the earth-connexions, he continues, "The most probable explanation appears to be that the currents are terrestrial, of which a portion is conveyed along the wire, and rendered visible by the multiplying action of the coil of the galvanometer." I refer particularly to this communication, because the two facts with which we have to deal—that currents are found travelling in the earth, and that a portion of such currents comes under our notice in the form of a derived current—are at the basis of all further inquiries into their nature.

I have mentioned that the disturbances ceased to attract our notice after the close of the year 1848. Feeble deflections, it is true, were occasionally observed, but they were practically disregarded, as they were not of sufficient magnitude to interfere with telegraphic operations. Our attention began again to be called forcibly to them about the close of the year 1856; and instructions were again issued to take notes and make a weekly return to me. The result is that I have a large accumulation of observations, commencing in April 1857 and continuing to the present date. It is not my intention to discuss these returns, nor to extract from them the dates of great disturbances. The fact that earth-currents, disturbed magnetometers, and auroræ are part of the same phenomenon, is too well established to require cases in point.

The most remarkable magnetic storm on record, at least since lines of telegraph have existed, is that which occurred in the autumn of 1859, commencing on August 27, and continuing until September 6. All the concurrent phenomena were manifested in a very exalted degree, and attracted the attention of observers in both hemispheres. Disturbances were conspicuous in Australia, as they were in Europe and America. Auroral light was seen in latitudes as low as 20°, 18°, and 13° 18′. A series of five articles will be found in 'The American Journal of Science and Arts' (vol. xxviii. pp. 385 to 408; vol. xxix. pp. 92 to 97, 249 to 266, 386 to 399; and vol. xxx. pp. 79 to 89, being the numbers issued in November 1859, and in January, March, May, and July 1860). To the last of these the name of Professor Elias Loomis is attached; and the other four*, I believe, passed through his hands.

The ninety-three pages referred to are enriched with a large collection of reports from

* Since the above was written, the November Number (1860) of Silliman's Journal has appeared, with a sixth article of twenty-two pages (pp. 3 to 25); and the July Number (1861) with a seventh of fourteen pages (pp. 1 to 14).

various quarters of the globe, of auroral manifestations, disturbed magnetometers, and earth-currents in telegraph wires, of which latter more than one case is mentioned in which the ordinary telegrams were transmitted by aid of the electricity thus presented to the wires.

In his 'Archives des Sciences Physiques,' are two articles by Professor A. De la Rive on this same disturbance (tom. vi. Nouvelle période, pp. 49 to 59, and 275 to 288, being in the Numbers 21 and 23, issued respectively on September 20 and November 20, 1859). M. De la Rive has availed himself of the phenomena recorded on these days in further illustration of his electrical theory of the Aurora, which will be found in detail in the third volume of his 'Treatise on Electricity,' p. 283, English Translation.

In looking over the records collected by Professor Loomis, and those which had been supplied to M. De la Rive, it was plain that we, who have the direction of systems of electric telegraph, had, to a certain extent, failed in our duty. For myself, I had, it is true, received week by week certain returns from a few places in the district under my charge; but they had neither been discussed nor published; so that when M. De la Rive stated in reference to certain Paris observations, "Malheureusement le sens des courants transmis par les fils télégraphiques n'a pas pu être indiqué exactement" (*op. cit.* p. 57), I took the matter up more actively, the more so as he was under a misapprehension (to which I shall refer presently) for lack of information.

The net of telegraph wires upon which my observations have been made, occupies the south-eastern portion of England. The wires are spread over the counties of Kent, Surrey, Sussex, and Berkshire. In a general way, the district may be regarded as bounded on the N. by the river Thames, and on the E. and S. by the British Channel, the other southern counties of England being on the western side. A map of this portion of England is given (Plate II.). The district includes a large number of telegraph stations and other groups of telegraphs, in addition to those which are set out on the map. I have selected various telegraph terminal stations, and taken them in pairs, not giving, as they are not necessary to our purpose, any of the intermediate stations; and have regarded the two stations of each pair as connected by a direct line. It will be evident at a glance that I am in possession of lines in various azimuths. There are eighteen selected lines in all, and each is in a different azimuth. A few more could have been found; but as they occupy only intermediate places, and connect stations where observations cannot conveniently be made, they are rejected. It will be seen that the railway routes between any two stations (and they are also the routes of the telegraph wires) are often devious, while the lines which are to form the basis of this inquiry are direct. In approaching the subject, I take it that a current or drift or flood of electricity is passing at a given time and in a given direction through the mass of the earth. Our telegraph wires penetrate the earth at their respective terminations, probing or sounding, as it were, into this then pervading stream of electricity. They penetrate by the aid of the gas-pipes and water-pipes in towns, by pumps and wells in country places, occasionally by a plate of copper sunk deep in the wet soil, and very frequently by the metals themselves of the railway,

the latter method having been more available since the custom of *fishing* or connecting the consecutive rails together with a plate of iron has prevailed. The electricity collected from the earth by these probes or earth-plates appears in our wires as a *derived current*. It enters a wire at the station nearest to the point of the horizon *from* which the current is flowing, and leaves it at the station nearest to the point of the horizon *toward* which it is flowing. In Table XI. the direct distances between the respective terminal stations are given in the fourth column, and the telegraph distances in the fifth. A few cases occur in which the difference is considerable, which is when the railway route is circuitous. I may state here that the reason why the Margate—Ashford telegraph route is nearly double that of the direct route, is because the wires make a loop to Deal, and thence *viâ* Ramsgate to Margate. The effect of this extra length of wire being between the respective earth-plates is, of course, from the increase of resistance, to diminish the value of the derived current. The Margate—Ashford is, as we shall see hereafter, our most active circuit; it would show values still higher were the connecting wire in a direct line.

I regretted to find that the returns which reached me of the telegraph disturbances of August—September 1859 were unusually meagre. The fact appears to have been that the disturbance was of such magnitude and of so long continuance, and this at the busy season when the telegraph is more than usually required, that our clerks were at their wit's end to clear off the telegrams (which accumulated in their hands) by other less affected but less direct routes. At a time when observations would have been very highly acceptable, they were too much occupied with their ordinary duties to make notes of the deflection of the needles and the changes. And I may here advert to the circumstances with which we are surrounded, and the conditions under which our observations are made. The wires and telegraph instruments are erected for commercial purposes; they are, as a rule, very fully occupied. The clerks or employés in charge of the instruments have their various duties to discharge, and have not much time at their command. I am on this account the more indebted to them for the interest they have evinced in these observations, and the unusual diligence with which some of them have made notes of what they have seen. And I may especially refer to J. Dyke at Ashford, D. Malpas at Ramsgate and T. Pulley at Ramsgate, and then at Canterbury. They have proved themselves able volunteers in the cause of science.

The Astronomer Royal, in his "Report to the Board of Visitors," on June 2, 1860, says "it is extremely difficult to extract from the accounts, even the careful ones, of telegraph clerks, such an idea of the phases of the currents as will make them comparable with the phases of magnetic storms." I can well enter into Mr. Airy's views; and if I plead guilty to having not furnished him with observations made on my district, to compare with the other observations that have reached him, it is because I have not heretofore had the opportunity of fairly discussing the crude facts that have accumulated in my hands. He further adds that "it may be worth considering whether it would ever be desirable to establish in two directions at right angles to each other (for instance,

along the Brighton Railway, and along the North Kent Railway) wires which would photographically register in the Royal Observatory the currents that pass in these directions, exhibiting their indications by photographic curves in close juxtaposition with the registers of the magnetic elements*." I think it would be very desirable, because it is not practicable on wires erected for other purposes, and with observers whose duties with these wires are of an urgent nature, to secure an undisturbed series of consecutive observations,—and the more so as the abnormal state comes on without forewarning. Nor would it be necessary for this purpose to incur the cost of erecting a long line of wire; for it will be seen, as we proceed, that one of the most active circuits with which we have to deal is only three miles long. I have also two stations at Ashford which are only 972 yards apart, between which derived currents of small value have more than once passed, although the galvanometers by which they were indicated are not of the most sensitive character. This was the case, 1859, November 30, 4.48 to 5.6 P.M., and 1860, August 11, 10.6 to 10.8 P.M. The most favourable direction for wires, or the best bearing for earth-plates, may be gathered from the results to which the following observations will lead.

To return to August—September 1859. The Dover clerk writes on September 2, "This morning, on opening the office, I found the needles of both instruments firmly blocked over to the left, and although the handles were firmly held over to the right to counteract the current, to my surprise I found that our battery power had not the slightest effect.... I am sorry to say there is not the slightest possibility of our working the instrument; needles continuing firmly fixed over, and which has continued for upwards of half an hour." The clerk at Ashford, who generally makes a good series of continuous observations, in this instance, for reasons already named, reports in very few words: "September 1. From 4 A.M. to 9 A.M. very strong on all instruments, first one side and then the other, which prevented any messages to or from; and deflections very strong through the day, which caused great delay to the work.

"September 2. Very strong from 4 A.M. to 10 A.M. and throughout the day. Great delay to messages in consequence."

Deal reports for each of the three days, September 1, 2, and 3, very briefly:—"Hard on, right and left the whole day."

The Ramsgate observations were more in detail, and are given in full in Table I. At this station are three telegraph instruments; the most sensitive is on the group which terminates at Ashford and Margate; the next in character is on the group termi-

* Since this paper was read, Mr. AIRY's Report to Visitors, 1861, June 1, has been printed. Referring to the suggestion in his previous Report, he says, "I conceive that this may be justly regarded as an important physical experiment; and I hope to be able shortly to lay before the Visitors some details of plan, and to ask their opinion in a more precise form." The proposition has come before me officially; I have reported upon it to the Astronomer Royal, and have furnished him with an estimate of the cost of erecting a wire from Greenwich to near Croydon, and another from Greenwich to near Dartford (*vide* Map, Plate II.). The proposition has met with the approval of the Visitors; and I have, to a certain small extent, made progress in anticipation.—C. V. W., July 1861.

nating at Ashford and Ramsgate; the third, on the short group from Ramsgate to Margate, is less delicate. I may mention that arrangements of this kind are general, the long circuits being furnished with the best instruments. This Table is a good specimen of the notes that are usually taken, and of the manner in which the returns are sent in. In the column headed "Direction," the observers insert the letter to which the telegraph needles point; I have substituted for this the direction in which a derived current of positive electricity would be travelling when that letter was made. By N., I mean a positive current passing from the station that is more northerly to the one that is more southerly, and by S. *vice versâ*. And in all cases, as well in this as in the Tables and diagrams that follow, the magnetic north is referred to. The value of the deflections observed on the needles are in this Table expressed in very general terms. The word "hard over" means 45°, or thereabouts; "horizontal," 80° or more, in fact as far over as it can well go. I have placed a copy of this Table in the hands of Professor Elias Loomis, as he had no continuous series like this in the collection of observations that he had published. It contains some of the details for which M. De la Rive inquired. This Table may be taken as correct as far as it goes. It contains many blank spaces, the observations having necessarily been discontinued from time to time. The changes of the needles from right-hand to left-hand deflections are gradual. One of my best observers says, "I have not at any time known the needles to return suddenly from their deflected position. I have frequently observed them to pass very quickly from one side to the other in a gradual manner, as though worked round by some slow-moving machinery, but never to drop suddenly. I have also observed the needles to partially right themselves, and then to be brought back again to quite as strong, and frequently much stronger deflected position." The manner in which the change from a north to a south current is brought about is very remarkable; it is evidently no drift of a "circular magnetic storm," so to speak, nor is it any kind of axial rotation. To all appearance the north current gradually fades away, and the south as gradually rises and increases in value. When a series of lines converge from various points of the horizon, the change of deflection on one instrument is accompanied by a change on all the others; and this is the case throughout the district. And it has been observed that needles of the most active groups are a little less sluggish in these changes than are the needles of the less active groups. In the midst of storms the needles very frequently have periods of entire tranquility; so, on the other hand, in the midst of calm the needles have periods of activity, sometimes of a few minutes' duration only, and then all is again still. The following are a few cases taken at random from many such:—

1859, November	13, four slight deflections during the day.
	14, two slight deflections during the day.
	18, one slight deflection from 7.35 to 8 A.M.
1860, January	15, one slight deflection from 9.10 to 9.23 P.M.
	30, one slight deflection.
February	27, one slight deflection.
April	21, one slight deflection.

We have repeatedly doubled the size of the conducting wire in a given group, and in all instances have by the change increased, and to all appearance doubled the deflection. This was to be expected with a derived current under like conditions. It was done by Mr. BARLOW, and with similar results, in 1847*.

The frequent occurrence of the words "hard over" and "horizontal" in Table I., convey a good general idea of the violence of the storm and of the times when the greatest activity prevailed. I refrain from discussing this Table, because arrangements were subsequently made by which observations of a more definite character were made, and which have furnished data of greater value. To the examination of these data we now proceed.

In order to form some general idea of the comparative value of the currents that present themselves, I selected a telegraph station (Ramsgate) where there was a good observer, and where deflections occur at all times of disturbance, even when the currents are too feeble to attract attention elsewhere; and I placed there in the telegraph circuit of the Ashford—Margate group a graduated galvanometer. Certain preliminary experiments were made with this galvanometer, in order to have some values of its deflections with which to make comparison. The results are given in Table II. I first took six cells of a battery of amalgamated zinc and platinized graphite, charged with 1 sulph. acid +10 water, and obtained the deflections with one or more cells when the galvanometer and battery were alone in the circuit. The mean results are given in the fourth column.

The galvanometer was then permanently connected at Ramsgate in the Margate—Ashford circuit, the telegraph length of which is 51½ miles; and ordinary currents were sent through the circuit from cells of the common telegraph battery, the number of cells in use being varied. The results are given in the Table; three cells gave a deflection of 5°, and forty-eight a deflection of 62°; intermediate numbers intermediate amounts. It was further noted that good telegraph signals produced a deflection of 60°; middling, one of 54°; and weak, one of 40°. Having thus a tolerable standard for reference, we could observe to better purpose.

Favourable opportunities occurred very soon after these arrangements were made, and of which we were able to avail ourselves, namely, on August 8, 9, 10, 11, and 12, and on September 7, 1860. The results are contained in Tables III. to VIII. inclusive. These Tables do not contain the whole of the observations made on the respective days; there were other detached or interrupted observations before the commencement or after the completion of those tabulated. I have selected a portion of each day during which there was the least possible break of continuity in observing. For instance, on August 8, out of $6^h\ 44^m$, only 37^m are blank; on August 12, only 21^m out of $10^h\ 31^m$, and so on (see Table IX. *b*). The "time" column is accurate. All the observing stations receive time signals direct from the Royal Observatory, Greenwich, at least once a day. The "duration" column is subdivided into two parts, marked N. and S., which contain the time in minutes during which a positive current was flowing from the N. to the S.

* Philosophical Transactions for 1849, p. 63.

station, or from the S. to the N. station respectively. The "values" are given in degrees of the galvanometer already described; those under the Margate—Ashford heading are read off direct on the galvanometer; those under the Margate—Ramsgate heading are approximate values on the same galvanometer, obtained by observation on another instrument, which had previously been compared with the standard galvanometer. The majority of the observations contained in the six Tables were made at Ramsgate; in some cases, where an interruption occurred at that station, the blank has been supplied from the notes made at other stations; in such cases the approximate value has been estimated and a query (?) placed beside it. The series would be complete without the Margate—Ramsgate values; but I have collected and tabulated these for the special purpose of pointing out the very large amount of action, the great value to which a derived current will attain on so short a circuit as one of three miles only when well placed. It is very striking to read such values as 76°, 71°, 70°, 73°, 86° under such circumstances. They equal the effect of four or five cells in short circuit; they exceed the effect from a good telegraph battery of forty-eight cells in an ordinary circuit. They greatly exceed what are considered good telegraph signals; and hence we are not surprised to find that the ordinary battery current, as stated in the report from Dover already quoted, has not power enough in many instances to neutralize them. It is evident from these figures that wires of moderate length will suffice, especially when galvanometers of a more highly sensitive character than those before us are employed, for all the purposes of a magnetic observatory.

I have on each day of observation calculated the mean value of the N. and the S. currents in time and in degrees of deflection. The most complete series of observations are those made on September 7. They embrace a period of $9^h\ 28^m$, with blank periods amounting to only $40\frac{1}{2}$ minutes. It is very instructive to go through such a Table as this, and notice the frequent transitions, not merely from N. to S. currents, but from a N. current of high value to a S. current also of high value, and this in the course of a very few minutes. For instance, between $7^h\ 19\frac{1}{2}^m$ and $7^h\ 29^m$, that is to say in the course of only ten minutes, four high and alternate values are registered, 64°, 44°, 34°, and 38°. Many other like cases may be selected from the Tables before us.

These six groups of selected observations are analysed in Table IX. The total number of currents recorded on the six days are arranged according to their values in time. The number of currents on each day that had a duration of $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ min. are first given; then those of various lengths between one and five minutes; and finally those for each interval of five minutes, beginning with those from six to ten minutes, and terminating with those between 61 and 120 minutes, which is the limit. The results of each day are given under the respective columns in the division of the Table marked *a*; and the sum for each time-value is given in the last column. Only 19 out of 389 currents, or 1 in 20, had a duration of less than a minute. The proportion between those currents which exceeded, and those which did not exceed five minutes in duration, was as 117:272, or as 1:2·32. Of those which exceeded twenty minutes and those which fell between six minutes and twenty minutes, the proportion was 26:91, or

1 : 3·5. Three currents are recorded as having continued for more than one hour; sixteen as having continued more than half an hour. The 1-minute currents are most in number, namely, eighty, or nearly one-fifth of the whole; the next in number are the 2-minute currents, seventy-five. Then follow the 3-minute and the 4-minute, the ½-minute and the 5-minute.

The division *b* of the Table shows at a glance the total period of observation on each day, the total number of minutes during which N. currents were collected, and those during which S. currents were collected, and the sum of the whole. On August 8 the S. currents exceeded the N. by 49 min.; on August 9 the N. exceeded the S. by 1·5 min.; on August 10 the S. exceeded the N. by 28 min.; August 11, N. exceeded S. by 57 min.; August 12, S. exceeded N. by 24 min.; September 7, N. exceeded S. by 21·5 min. During the whole series the S. currents exceeded the N. by 21 minutes. The division *c* of Table IX. shows the mean duration of the N. and S. currents on each day, and the mean of the whole series. On some days the mean duration of the N. currents exceeded that of the S. currents; and on other days the reverse was the case. But taking the mean of all the observations and all the days, as given in the last column, the values of each are nearly the same. We have 9·51 min. as the mean for the N. currents and 9·42 for the S.

In Table X. the total number of currents are arranged according to their values in degrees; that is, according to their action on the galvanometer. In division *a* of the Table, the first column contains the galvanometric degrees, in divisions of 5° each, 1° to 5° being the first in the list, and 81° to 85° being the last. The column under each date is subdivided, the N. currents being entered under the first division, and the S. under the second. The sums of N. and of S. currents for each value are then given; and these are added to give the total of currents for each value. It will be seen, by referring back to Table II., that a deflection of 5° is equal to that produced by three cells of an ordinary telegraph battery in a circuit of 51¼ miles. One cell will give about 2°. The greatest number of currents are found with a value varying between 15° and 20°; they are 91. The next in number, 84, range between 5° and 10°; then follow 56 ranging between 10° and 15°. The proportion of currents which exceed 40° to those which do not, is as 59 : 383, or as 1 : 6·49. The similar proportion of N. currents is 32 : 193, or 1 : 6·03; of S. currents 27 : 190, or 1 : 7·03. Two N. as well as two S. currents were obtained ranging from 71° to 75°, and one N. current exceeding 80°; it was 82°. There is no very marked difference between the numbers of N. and S. for the different values on each day; sometimes the N., at other times the S. are in the ascendent. Above 45°, 26 N. currents are recorded against 22 S.

The *b* division of Table X. gives the mean value of the N. and the S. currents for each day, and the general mean of the whole. The N. on some days and the S. on other days have the higher mean value. The general mean gives 28°·01 for the N. and 26°·87 for the S. currents. Taking the mean of all the currents, the values are 9·46 minutes of time (Table IX. *c*), and 27°·44 of deflection (Table X. *b*).

The Tables we have now been discussing may be taken as good specimens of the

general character of earth-currents; and of these I should be somewhat disposed to think that the observations made on September 7 are the most characteristic. Be this as it may, it comes out, from what we have recorded, that there is little difference of behaviour between the two kinds of currents. The S. currents, for instance, differ but little either in degree or in duration from the N. currents.

It is not my purpose in this communication to set forth any theory as to the origin of the currents in question; neither is it my object to discuss the auroral theories. But there is a certain feature in M. De la Rive's theory which fails in the [redacted]ence of the facts before us, and which requires notice here from the confidence with which it is put forth. On the faith of a few observations made on the Berne—Zurich telegraph line between 8^h and 9^h A.M. on September 2, 1859, this philosopher arrives at the conclusion that the N. currents have a longer duration and are more powerful than the S. currents, and that the latter are merely due to the secondary polarities acquired by the earth-plates when they have transmitted a derived N. current. He calls the N. the direct current; and he says, "Nous pouvons donc conclure de là, d'abord que le courant qui à la fois est le plus fort et dure le plus longtemps, est bien le courant direct perçu par le fil télégraphique, dont une des extrémités plonge à Zurich dans le sol, et l'autre à Berne, et qu'il chemine bien du nord au midi, c'est à dire du pôle nord à l'équateur; nous sommes également conduits à regarder le courant inverse de moindre intensité et de moindre durée, comme provenant des polarités sécondaires qu'acquièrent les deux lames de cuivre plongées dans le sol, quand elles ont transmis dans un circuit fermé, pendant quelques instants, une dérivation du courant terrestre. Ces deux conclusions .... sont, comme on le comprend, très précieuses pour la théorie, et elles confirment pleinement celle que j'ai donnée dans ma première notice." Further on he adds, "En effet il n'y a véritablement dans le sol, lors de l'apparition de l'aurore, que des courants dirigés du nord au sud; ces courants sont seulement d'une intensité variable d'un instant à l'autre. Quant aux courants inverses plus faibles, et d'une durée moindre, qu'indiquent les appareils, ils ne sont que l'effet de la manière dont on perçoit les premiers; les plaques de cuivre qui terminent les fils télégraphiques, et qui, plongées dans le sol servent de sondes pour dériver une portion des courants terrestres dirigés du nord au sud, se polarisent bien vite, et donnent ainsi naissance à des courants inverses par l'effet des polarités sécondaires qu'elles ontacquises*."

But from the figures before us it would be hard to say that either the N. or the S. current exceeds the other, either in value or in duration. Take for instance the high values, those exceeding 60°: if we have two N. currents between 61° and 65°, we have also two S.; the same is the case between 66° and 70°, and 71° and 75°. And with respect to duration, the N. currents on some days, and the S. currents on others, have a total excess in time; and if, on the one hand, the mean duration of each S. current is 0·09 minute less than that of the N. currents, the total flow of S. currents is 21 minutes greater. Were it otherwise, it would still be impossible to admit a polarization of elec-

* Arch. des Sc. Phys., Nouvelle période, tom. vi. pp. 281, 282, 286. 1859, Nov. 20.

trodes that would continue active, and highly active too, not for minutes merely, but often for quarter and half hours. We have no approach to such polarization in the ordinary use of the telegraph. Besides, it could with equal propriety be attributed to the S. currents as to the N.; for all our experience thus far leads to the conclusion that no one thing can be said of the one class of current that cannot with equal truth and equal force be said of the other.

Had results such as those before us been made more public heretofore, the learned Professor of Geneva could not but have modified his views; for with the proved existence of so large a quantity of S. current, he could scarcely have presented the auroral theory in its present form, because the N. currents entering at the polar regions and flowing southward is the essential feature of his theory.

While investigating the value and duration of earth-currents, our attention has heretofore been almost confined to a solitary telegraph group, namely, to the Margate—Ashford line; and the currents of positive electricity found moving from Margate, the more northerly station, to Ashford, have in a general way been termed N. currents, and *vice versâ*. It is obvious, however, that the electric flood, in its course through the mass of the earth, might vary greatly in azimuth and still give the same apparent result. Fig. 2, Plate III. will explain this. If the line marked 13 represents the bearing of the Margate—Ashford group in respect to the magnetic meridian N.S., Margate being at the circumference of the circle, an earth-current from any azimuth, not exceeding 90° to the north or south, right or left of the line 13, would enter the telegraph wire at Margate and leave it at Ashford. The shaded portion of the circle shows this limit in either direction. The azimuth of the line in question is 72° E. of N.; so that the limit in one direction is 18° W. of N., and in the other 18° E. of S.; and therefore what we have in general terms called a N. current, might be the result of an actual S.S.E. current, just as it might equally be of a N.N.W. current. The real direction of the currents in the earth cannot be determined from observations made on a single telegraph group; neither can it be determined by simultaneous observations made on different telegraph groups, unless there be some among the groups that have widely different azimuths.

The Map (Plate II.) that accompanies this communication shows the district of England over which these observations extend. I have omitted all intermediate stations, and have inserted, with here and there an exception, only the name of the telegraph stations that are concerned in this investigation. The magnetic meridian makes an angle of $21\frac{1}{4}°$ W. with a vertical passing through the Map. It is necessary to bear this in mind when referring from Tables XI. and XII. to the Map. The fine lines drawn on the Map represent the railway routes, and at the same time the routes along which the telegraph wires are led. Direct lines drawn from station to station are used with the magnetic meridian in obtaining the bearings given in Table XI., and the values shown in the first column of distances in the same Table.

I have selected eighteen pairs of telegraph terminal stations, each one differing from the other in azimuth, and have referred them to the magnetic in preference to the astrono-

mical meridian, in order the more readily hereafter to compare the determined direction of earth-currents with the behaviour of the declination-needle during their existence. The mean westerly magnetic declinations at Greenwich have been—

For 1857	21°	34′	30″
For 1858	21	29	30
For 1859	21	23	30

I have therefore taken $21\frac{1}{4}°$ as an approximate value for 1860 sufficiently near for the present purpose*. Whitstable and Canterbury are as nearly as may be on this meridian, the former being to the north of the latter. Table XI. contains these eighteen groups arranged in order, and in three divisions: 1st, those on the magnetic meridian; 2nd, those bearing East of it; 3rd, those bearing West of it. The numbers in the left-hand column apply to the stations in order; and in using them for reference, 1—2, or more briefly 1, means *from* Red Hill *to* Brighton; 2—1, or more briefly 2, means *from* Brighton *to* Red Hill; and so of the rest. The odd numbers are all made to fall to the north of a line drawn at right angles to the magnetic meridian; and the even numbers to the south of the same line. The figures in the column headed "Bearings" are approximate, fractions of degrees being neglected. They extend from the 82nd degree West of North to the 80th degree East, embracing in all 162°, or only 18° short of the half of a great circle,—giving, therefore, a very wide field for observation. The Table also contains the distances measured in direct line from point to point, and also the distances along the route pursued by the telegraph wire which connects each two points respectively.

Fig. 1, Plate III. is a graphic representation of Table XI. The numbers 1—2, 3—4, &c. correspond with those in the Table; and the lines are laid down at the angles gathered from the Map and given in the Table.

From time to time I have secured simultaneous observations from more groups than one, frequently from several groups. It is not necessary to encumber this communication with specimens of all combinations. I have selected four, beginning at fig. 2 (already referred to, and which is the most simple but least instructive case), and concluding with fig. 5 (which is the most full and conclusive).

Fig. 2 represents the most common of all cases, a current collected by the Margate—Ashford wire No. 13. In this wire, as well as in its near neighbour No. 15, the Ramsgate—Ashford wire, a current may always be found, if detected elsewhere. The effects are similar and simultaneous on all the instruments of this circuitous telegraph group, namely, at Margate, Ramsgate, Deal, and Ashford.

Fig. 3 represents a case almost equally common. No. 13, as before, is the Margate—Ashford group, and No. 17 the Margate—Ramsgate. By setting-off 90° from each line in the direction of the other line, we define the limits within which an earth-current

* Mr. AIRY's Report, published since this paper was read, gives it 21° 14′ 20″.—C. V. W.

must flow in order to be collected in the two wires in question, and in the direction given, as follows:—

No. 13 . . . 90°−72°=18° W. of N.; the northern limit.
No. 17 . . . 90− 2=88 E. of N.; the southern limit.
106; the total range.

This range is shown by the shaded part of fig. 3; and is 74° less than that of fig. 2.

In times of greater activity (and these are very frequent) we have the case shown in fig. 4. Besides No. 13, which, as I have said, is always present, we have, as before, on the one hand No. 17, the Margate—Ramsgate group, and on the other No. 26, the Dover—London group. The azimuths here are far wider apart. By treating them as before, setting off 90° from either toward the other, we have

No. 26 . . . 90°−44°=46° E. of N.; the northern limit.
No. 17 . . . 90− 2=88 E. of N.; the southern limit.
42; the total range.

We get here within a comparatively narrow range, and begin to discover the approximate parts of the horizon whence the currents come. The shaded parts of fig. 4 show this graphically. I have not thought it necessary to give these figures in duplicate in order to show the cases in which all the currents are reversed; it will be readily gathered that, *mutatis mutandis*, the results would be shown by a shaded part in the opposite quadrant.

Table XII. contains, in sections 1 and 2, a selection of a few out of the multitude of simultaneous observations that have been made, and which give the results shown in fig. 4. The years 1857, 1859, and 1860 have contributed samples. I might extend this Table at pleasure. It will be remarked that not only have we here the direction in which the derived currents were moving in the telegraph lines No. 17 and No. 26, or their converse, Nos. 18 and 25; but we have also the evidence that currents were moving in a similar direction along many distinct lines of telegraph, the bearings of which with the magnetic meridian are intermediate between the two extremes given. For example: by comparing fig. 4 with fig. 1, it will be seen that all these other groups, taken from Table XI. and entered in Table XII., fall between the two radii marked 17 and 26; they are in order N. 5, 13, 15, 32 and 28; so that we have here a large series of observations taken over a considerable area of country, each confirming the accuracy of the other, and all conspiring to prove that the point of the horizon from which the earth-currents came in 1857, as well as in 1860, was situated somewhere between 46° and 88° E. of the magnetic north. I have in the several columns entered the values of the currents in the words of the observers. They are sufficiently characteristic.

On December 17, 1857 (which, by the by, was at the period of the earthquake that committed so much devastation in the kingdom of Naples, and of which a Report was presented to the Royal Society by Mr. R. Mallet, on May 24, 1860*), the earth-

* Proceedings of the Royal Society, vol. x. p. 486.

current must have been very strong, seeing that the action was so great in both of the extreme groups, although their bearings with each other made an angle of no less than 138°, that the galvanometer needles in each case were described as being "hard over." In the later entries, which were of observations made subsequently to the erection of the graduated galvanometer at Ramsgate, the values are entered in degrees. If a query (?) occurs, it implies that the observation was made and the direction given, but the value was not noted. When blanks occur in the Table, it means that no observation was made on the group against which the blank appears, at the time in question. In all cases where simultaneous observations have been made, the direction of currents in intermediate groups is in strict conformity with the results obtained from the two extremes. And when the direction changes, as in Table XII. section 2, for one extreme, it changes also for the other, and for all the intermediate groups that are the subject of observation. The remarks, therefore, which have been made on section 1 are, *mutatis mutandis*, equally applicable to section 2. Simultaneous observations have now and then reached me from the Minster—Deal group, No. 19. I have entered cases, but pass them over here. They take 4° from the range, reducing it to 38° in the instances given.

The case shown in fig. 5 is the most complete; it has not attracted attention so frequently as the last. The telegraph group No. 23, London—Tonbridge, is less sensitive, is more liable to interruption and not so convenient for observations, and it is more distant from the centre of action. The observations on this group have a special interest of their own. There are two lines of telegraph between Tonbridge and London; one is $40\frac{1}{2}$ miles in length, and goes *via* Red Hill; the other is $57\frac{1}{4}$ miles in length, and takes a circuitous route by Paddock Wood, Maidstone, Strood, Gravesend, and Woolwich. The currents in these two groups in all instances coincide, and have reference to the bearings London—Tonbridge, and not to the route. This is the more striking because one wire, as may be seen by the Map, comes to Tonbridge from the eastward and the other from the westward, and the currents, as far as the wires are concerned, arrive from directly opposite points of the compass, and to all outward appearance are opposed to each other.

The bearing of No. 23, the London—Tonbridge group, is 13° W. of N. By treating these bearings as before, we have

No. 26 . . . $90^\circ - 44^\circ = 46^\circ$ E. of N.; the northern limit.
No. 23 . . . $90 - 13 = 77$ E. of N.; the southern limit.
31; the total range.

The 3rd and 4th sections of Table XII. contain cases in point, each, as in sections 1 and 2, being the converse of the other. All the observations in these sections confirm those in the other two, with the advantage of having 11° more of azimuth, and reducing the range within which the true direction is to be sought, to 31°. By referring to fig. 1, it will be seen that between Nos. 23 and 25 there is a blank space which amounts to 31°. Had we possessed telegraph lines having azimuths that might fall within the

vacant space, we should no doubt have been able to present results from lines situate either beyond No. 26 on the one hand, or beyond No. 23 on the other, and have contracted still more the range, now standing at 31°.

In the absence of these observations, we are still justified in concluding that neither No. 26 on the one hand, nor No. 23 on the other, are the boundary lines, because, when we look at the eight cases before us, four have *strong* currents in both limiting lines, and one has *very strong*; and if we turn back to sections 1 and 2 of Table XII., fourteen out of the eighteen cases have strong or very strong currents in the London—Dover boundary line.

Subject to correction by future observations, I have assumed that 10° may reasonably be allowed in each direction, making 20° in all to be deducted from 31°, leaving a limit of only 11° within which to seek for the true direction. For the place of this limiting arc, we have

$$90^\circ - 44 + 10 = 56^\circ \text{ E. of N.; the northern limit.}$$
$$90 - (13 + 10) = 67 \text{ E. of N.; the southern limit.}$$
$$11 \text{; the total range.}$$

Its place is shown by the more deeply shaded part of fig. 5. From finding the London—Dover line (No. 25–26) the more active, I should be disposed to infer the true direction to be nearer the Dover—London than the London—Tonbridge line; and to divide the 11° unequally. Making the proportion as 7° to 4°, we have

$$\left.\begin{aligned} 56^\circ + 7^\circ &= 63^\circ \text{ E. of Mag. N.} \\ 67 - 4 &= 63 \text{ E. of Mag. N.} \end{aligned}\right\} \text{true direction.}$$

This direction is shown by the arrow R R′ in fig. 5, Plate III. And by deducting from this the magnetic declination of $21\frac{1}{4}°$, we have $63° - 21\frac{1}{4}° = 41\frac{3}{4}°$ E. of N. as the true geographical bearing of the point whence these earth-currents flow when in one direction, and $41\frac{3}{4}°$ W. of S. when in the other direction. And there is no evidence that they exist in other azimuths. We are not in a condition to detect secular changes, if any, as there may be. They would possibly be small; and our range in respect to such probable changes is large. For three years at least their direction has not greatly varied, at least so far as the south-eastern counties of England are concerned. The greater may contain the less; but I would pause before inferring the greater from the less, and contending that what is true of the small dot of the earth which has been the subject of my observations, may be equally true of the rest of Europe, to go no further, or even of the rest of this little island.

The contents of the reports referred to above, of the disturbances of Aug.–Sept. 1859, are too concise to contain materials from which the probable direction of the earth-currents elsewhere may be determined with accuracy. But the little information they contain helps for the most part to confirm the conclusions to which we have arrived,

and to indicate that the N.E. and S.W. general direction of the currents is not confined to the spot under consideration, but is more general.

M. De la Rive, quoting a letter of M. Bergon's from the 'Comptes Rendus de l'Académie des Sciences de Paris' (1859, Sept. 5), says, "Les lignes les plus influencées ont été celles de Bordeaux, Toulouse et Marseille.... La ligne de Strasbourg, si on la compare aux lignes de même longueur, paraît avoir subi les moindres atteintes" (*op. cit.* p. 53).

The following Table gives the bearing geographically of these places in respect of Paris, and the angular distance of this bearing from the line arrived at above, whose bearing, as we have seen, is determined to be about $41\frac{3}{4}^\circ$ E. of N.

Bordeaux . .	58° S. of W. . .	$9\frac{3}{4}^\circ$ angular bearing.
Toulouse . .	80 S. of W. . .	$31\frac{3}{4}$ angular bearing.
Marseilles . .	75 S. of E. . .	$56\frac{3}{4}$ angular bearing.
Strasbourg . .	10 S. of E. . .	$58\frac{1}{4}$ angular bearing.

The Bordeaux line, making an angle of only $9\frac{3}{4}^\circ$ with the assumed direction of the earth-currents, would necessarily be more affected, as it was reported to have been, than the Strasbourg line, making as it does an angle of $58\frac{1}{4}^\circ$.

M. De la Rive also states (*op. cit.* 280): "Dès 7 heures de matin (Sept. 2), M. Hipp, informé de l'impossibilité de télégraphier, se transporta au bureau de Berne, ... il constata l'existence de courants énergiques dans les fils. Ces courants étaient à peu près constants; celui de Zurich fasait dévier l'aiguille de la boussole de 45°, celui de Lucerne de 33°, celui de Lausanne par Fribourg de 38°, celui de Olten de 38°."

The angular distances, as before, of these places and Berne, in respect to the $41\frac{3}{4}^\circ$ E. of N. line, are in order.

Olten . . .	54° N. of E. . .	$5\frac{3}{4}^\circ$ angular bearing . .	38°
Lausanne . .	34 S. of W. . .	$14\frac{1}{4}$ angular bearing . .	38
Zurich . .	29 N. of E. . .	$19\frac{1}{4}$ angular bearing . .	45
Lucerne . .	13 N. of E. . .	$35\frac{1}{4}$ angular bearing . .	33

Zurich is here out of order in the value of its current; but it is further from Berne; the others are in due order; but in the absence of precise information as to the direction and simultaneity of the observations, accurate deductions could not be expected; and therefore too much reliance must not be placed on the results that come out. The same may be said of the following, which is the only other comparative case cited, and in which the observations were made during an interval of an hour and a half, and may not have been quite simultaneous. M. De la Rive says, "Ainsi à Bâle, ... on avait entre $4\frac{1}{2}$ et 6 heures du matin un courant sur la ligne de Paris de 75°, sur celle de Saint-Gall de 40°, sur celle de la Chaux de Fonds de 50°, sur celle de Strasbourg de 34°" (*op. cit.* p. 282). The order in which these places fall, their bearings and angular distances, are as under:—

Chaux de Fonds . .	40° S. of N. . .	8¼° angular bearing . .	50°
Strasbourg	87 N. of E. . .	38¾° angular bearing . .	34
St. Gall	7 S. of E. . .	55¼° angular bearing . .	40
Paris	24 N. of W. . .	72¼° angular bearing . .	75

Here also, in the absence of the direction of the derived currents and of other necessary data, bearing in mind also the very unequal lengths of the lines on which the observations were made, we cannot trace out why Paris is last instead of first in order. Its distance is greatest.

We must not attempt to pursue this part of the inquiry further, for lack of data, as far as the Continent is concerned; and the same may be said of the Transatlantic observations. Here and there among the latter I find a solitary line of greatest activity given, but there are no other sufficient data with which to compare it; so that no reasonable conclusions can be arrived at.

Returning to England, we have traced the azimuth of the earth-currents at the present time in the south-eastern counties to be a few degrees north of N.E.

The same general direction prevailed as far back as the year 1847, and in another district of England. A set of observations were made by Mr. Barlow at Derby, on lines of telegraph radiating in various directions from that town. The conclusion to which he arrived then was that the direction in which the currents travel "will be between S. 28° W. and S. 75° W., and apparently strongest when the earth-connexions are about N.E. and S.W." On treating this as I have done the results to which I had myself arrived, we have it thus:—

S. 28 W. or 28° E. of geog. N.; the northern limit.
S. 75 W. or 75 E. of geog. N.; the southern limit.
47; range.

And we can get the N.E. bearing by dividing the range in the proportion of 17 : 30,

$$\left.\begin{aligned} 28^\circ + 17^\circ &= 45^\circ \text{ or N.E.} \\ 75 - 30 &= 45 \text{ or N.E.} \end{aligned}\right\} \text{the direction given.}$$

In the S.E. direction, which is at right angles with the inferred direction, he found that "the motion of the needles becomes undefined." The bearings of the groups nearest to S.E. which fell under his observation were S. 38° E. and S. 50° E.

I have more than once called attention to the greater activity of the Margate—Ashford line; and have also pointed out the remarkable activity manifested by the short Ramsgate—Margate line. There are other lines, differing but little in bearing from these, and equally or better favoured as to length, which have never been known to approach it in activity. Take, for instance, the Tonbridge—Hastings line, which differs in bearing from the Margate—Ramsgate by only 8° (see Table XI.). Currents are not very common on

this line; when, however, they are observed, they always confirm the law as to *direction*, but always fall far below the Ramsgate—Margate in value. I have had only a solitary instance to quote in all four sections of Table XII., and this was of doubtful (?) value. Yet the length in one case is only three miles, and in the other twenty-six miles, in which latter, therefore, a greater rather than a less derived current might have been expected, and the more so as its galvanometers happen to be better.

Then, again, on comparing the respective values of the currents collected by the Margate—Ashford and the Ashford—Hastings wires, we find the same apparent contradictions. The former is 9° and the latter 11° from the determined direction, the angular distance differing by only 2°; yet the currents in the latter are invariably marked as "slight" and "middling," when those in the former are "strong" and "very strong" (see Table XII.). There is no material difference in mileage.

Again, the bearing of the London—Dover line is the *same* as that of the Reading—Red Hill. Currents, some even of high value, are frequent in the former, but have been rarely noticed, and then only in very small amount in the latter.

These apparent anomalies lead to the conclusion that the amount of current travelling through the substance of the earth during a magnetic storm is not the same in all parts of a large district; and this must needs be the case. The more favourably the conducting materials may be disposed in the geological strata of a district, the greater the value of the current-drift along the same may be expected to be. Where the materials are of inferior conducting power and ill arranged, there will the current that is present have a lower value. In fact these currents travel, as might be anticipated, in the conducting mass of the earth, just as they travel in all other conductors, and adjust and distribute themselves according to the known laws of resistance.

In Sturgeon's 'Annals of Electricity,' vol. i. p. 124, is a paper by Mr. Henwood "On the Electric Currents observed in some Metalliferous Veins," a discovery which the author rightly attributes to Mr. Fox*. In the 'Annual Reports' of the Royal Polytechnic Society of Cornwall for 1836, 1841, and 1842, are reports "On Mineral Veins," by Robert Were Fox, and "On the Electricity of Mineral Veins," by Robert Hunt and Professor Phillips. It is more than probable that the electric currents which were found by these gentlemen to be traversing the metallic veins in the mines of Cornwall, were in many instances portions of great floods of electricity, drifting along the district in which the mines were placed, forming in fact a portion of larger disturbances attending on magnetic storms. The derived currents which were collected by their galvanometers gave the same contradictory results as to direction that come out from any of our own cases, looked at individually; but in the papers before me I have no sufficient data from which to make groups of observations, in order to arrive at probable directions, nor do I gather whether changes of direction were noted. My impression is that I have heard of the value of currents being different, collected at different times at the same places. It would have been instructive, had it been possible, to compare some of the

* Philosophical Transactions, 1830, p. 399.

original notes made with the published magnetic observations of the Royal Observatory. I should scarcely think it possible when we collected electric currents of value so high in August and September 1859, by our probes or sounds thrust slightly into the earth, that the conducting metallic veins, large in bulk though more deeply beneath the surface, took no part in the general work of conduction.

It will be seen that when the direction of the current changes in one length of telegraph wire, it changes also in all the others then under observation; so that, should observations upon earth-currents at any future time form part of the work of magnetic observatories, it will by no means be essential to have an absolute N.E. line of wire for determining general direction and changes. It may in many cases be inconvenient or impracticable to select this direction,—as with the Royal Observatory, at this moment. In order to obtain maximum results such direction will of course be preferable. The derived currents collected from the earth are higher in value in proportion as the conducting wire is larger. It will therefore be better for comparative observations of values to select some wire most commonly met with,—as, for instance, No. 8 galvanized iron for suspension, being a wire $\frac{11}{64}$ inch in diameter, or, for buried wire, No. 16 copper, a wire $\frac{1}{16}$ inch in diameter.

On the Map, Plate II., I have laid down the line of direction through Greenwich of a wire coinciding in azimuth with the resultant direction determined herein for the earth-currents. I apprehend that a wire extended in this direction from the Royal Observatory to the River Thames, a distance of a mile or thereabouts, with a sensitive galvanometer, or possibly a WEBER'S dynamometer in circuit at the observatory, would enable the Astronomer Royal to study the general direction of earth-currents in juxtaposition with the movements of the magnetometers, and to obtain photographic registry of them side by side with those already recorded of the magnetic variations. For a further extension of the system to observations upon the actual azimuth, and the periodical or secular changes in such azimuth, a second wire in a direction at right angles to the other would be essential. Longer wires would necessarily be better.

It is not my purpose to enter into the complex problem of the magnetism proper of the earth, and of its variations. I am not competent to deal with these questions. Happily for science, they are well cared for in the hands of our Treasurer, the highly talented General SABINE. But having thus far succeeded in determining that there are actual currents of electricity, large in amount, travelling in now known directions through the mass of the earth, I have been naturally led to look at the magnetometers at observatories simply as magnets, and to inquire whether their behaviour, during the prevalence of these active states of the earth, was in accordance or not with the known reactions of current electricity and magnetism. My inquiries at present are rather tentative than complete. My means of observation are less perfect than is the systematic organization of an observatory, especially when further aided as it is by the introduction of photography. It may be also that the very small masses of metal of which my needles are formed are more readily moved, that is more promptly moved than are the larger

masses of the declination magnetometer, or the horizontal-force magnetometer. I may or may not get changes in direction, when the transitions are quick, that are not recorded in the observatories. On the other hand, we on our part may omit to notice many changes. The best mode of making the comparison appeared to be to select some periods of what appeared the best and most continuous series of earth-current observations, and set them out in curves side by side with the curves registered at Greenwich or at Kew. My choice was limited to the series from August 8 to August 12, and that of September 7, 1860, which have been before referred to, and given in detail in Tables III. to VIII.

Various photograms of the Greenwich observations are before me, prepared under the eye of Mr. GLAISHER, and with which I have been kindly furnished by the Astronomer Royal. General SABINE also promptly supplied me with tracings of the Kew photograms, made by Mr. BALFOUR STEWART.

Plate IV., figs. 6, 7 and 8, shows results of this comparison. I have selected for illustration such parts of the photograms and Tables as admitted of ready comparison. Fig. 6 comprises the interval between 2.55 P.M. and 6.45 P.M. of August 8, 1860, civil time. The ordinary reaction between a magnet in the position of N. S., fig. 5, Plate III., and an electric current moving in the direction of the arrow-headed resultant R R′ in the same figure, is that the north end of the magnet moves to the right if the current is passing beneath it in the direction from R to R′, to the left if in the reverse direction; that is, a northerly current *increases* the declination of the magnet; a southerly *decreases* it. The upper curve in fig. 6 shows the value in degrees of the deflections of the galvanometer needle taken from Table III. The distance between the horizontal lines is 10°; the distance between the vertical lines is five minutes of time. The northern currents N. are set off below the zero line, the southern above, in order to correspond with the curves on the photograms. The lines are left broken where observations are wanting.

The lower curve is taken from the Greenwich photogram of the variations of the declination magnet, but is expanded so as to correspond in time with the galvanometer curve. It was found more practicable to expand the Greenwich than to contract the galvanometer curve. It is expanded in the proportion of 1 : 4·5. A portion of the declination scale, extending from 21° W. to 21° 45′ W. is attached. From 3.15 to 3.57 the photogram was defective. The general correspondence between the two curves is apparent. This is particularly the case at 4; 4.30; 4.45 to 5.15; 5.15 to 5.25; 5.30; and thence in the general range to the end of the curve.

Fig. 7 contains the interval between 2 P.M. and 5.50 P.M., August 9, 1860. The curve of the declination magnet in this case is taken from a tracing of the Kew photogram. It is expanded in the proportion of 1 : 4. I had not the scale before me; the omission, however, is of no moment, for we are merely comparing general directions, not values. These curves do not correspond so fully as those of fig. 6; but in the more active portions the resemblance is sufficiently obvious.

Fig. 8 is prepared from observations made on September 7, 1860. My observations on that day were close and frequent, and a very large number were recorded. As it was not easy to trace the curve complete with a five-minute period between the verticals, I have therefore limited them to $2\frac{1}{2}$ minutes; so that the Greenwich curve is expanded in the proportion of 1 : 9. There is a considerable resemblance between the two curves in this case also. It was a little difficult to read off the Greenwich photograms on this magnified scale; the inflections I have traced out seem a little in arrear of those of the galvanometer. I am unfortunately almost without night observations, and am thus unable to make comparisons with some more manageable and conspicuous portions of the photograms. Other portions I have been constrained to pass over when the movements of the magnets have been continuous, and the photogram has been so accurate that a wide white band with jagged projections was recorded.

My morning observations on September 7 were many and good. I would fain have compared them with the Greenwich photogram; but it was unfortunately defective for those hours; and, by a strange coincidence, on turning to the Kew tracing, the photogram there was defective at the same time.

I could scarcely expect to find a rigid correspondence between the two classes of results. The causes of magnetic disturbance are evidently of a mixed character, and remain to be determined. But I think the comparisons I have made between the movement of the magnet and the direction of the current, which, if not wholly, was, as I believe, in large part concerned in causing it to move, are sufficiently encouraging. Results of a more definite character would of a surety follow from a system of well-concerted observation, made under more favourable circumstances than we can expect to enjoy.

My attention was naturally directed also to the behaviour of the horizontal-force magnet during the times when the earth is thus active with electric currents. The position of the horizontal-force magnetometer is given by W.E. in fig. 5, Plate III.,—the magnet being at right angles to the magnetic meridian, or to the declination magnet N.S., its marked or north end being to the west, or in the position W. The tendency of the horizontal-force magnetometer under the influence of earth-currents is to take the position of the dotted line A B, at right angles to the resultant line R R′. The declinometer has a tendency to the same position under similar influence; so that, if no other causes than earth-currents were in operation, the motion of the horizontal-force magnetometer would be *towards* the north when that of the declination magnet was *from* the north, and *vice versâ*. And regarding the magnets merely as magnets, and without reference to the constrained suspension of the one as compared with the more free suspension of the other, the reaction of the earth-currents on the horizontal-force magnet might be expected to be greater than upon the declinometer, because of the less angle it makes with the resultant R R′. It might therefore be expected, as far as these sources of disturbance are concerned, that an increase of horizontal force would coincide with an increase in declination. This could hardly be expected to come out in every case, on account of the other and recondite causes of disturbance that are ever present.

On referring to such of the Greenwich photograms as are before me, I have selected those portions of the curves that show extreme departures from the mean position, and where the magnets are in tolerably steady motion. The cases which are in conformity with this view are more frequent than those which are not in accordance with it. The following are some cases in which the maxima and minima, either or both, in any given day, coincide in point of time, both being large:—

		Declination.	Horizontal force.
1858. April 9.	4th to 5th hour.	Minimum very large.	Minimum very large.
	h m	° ′	
1859. August 27.	19 50 . . .	Minimum 20 51 . .	Minimum 0·086
	23 25 . . .	Maximum 21 32 . .	Maximum 0·093
1859. August 31.	16 15 . . .	Maximum out of range.	Maximum out of range.
	19 35 . . .	Minimum 20 32 . .	Minimum 0·077
1859. Sept. 1.	2 0 . . .	Maximum 21 43 . .	Maximum 0·091
1859. Sept. 4.	2 15 . . .	Maximum 21 42 . .	Maximum 0·094

On the 3rd of this September the magnets were very active; and from the third to the sixth hour there was a very bold and remarkable increasing curve for both instruments; and from about the sixth to the tenth hour an equally conspicuous decreasing curve. For the present, these examples must be accepted as general illustrations, and as suggestions to point out the direction in which the further pursuit of these inquiries may be most profitably carried on.

One or two questions have occurred to me while discussing these observations, which I had proposed to solve on the first day of disturbance that presented itself. From September 7, 1860, to January 20, 1861, the day on which I am writing, that is, for the unusually long period of five months, the earth has been almost inactive. Not a single storm-day has occurred: two or three solitary currents, small in value and brief in duration, have occasionally, though but rarely, been collected; but with these rare exceptions, it has been a period of perfect calm.

Since writing the last sentence, the returns for the week ending January 26 have reached me, from which I perceive that the earth was again showing signs of a relapse into an active state. Currents made their appearance in tolerable numbers from January 22 to January 26 inclusive, especially on January 24; on which day I notice that from 6.30 to 6.37 the Margate—Ashford needles were horizontal for a north current, and from 6.37 to 6.52 were horizontal for a south current. Other high values occurred throughout these days. On January 26 the observer at Ramsgate noticed "that the deflections, instead of moving steadily as usual, kept continually oscillating, more particularly at 1.35 P.M., when they incessantly went from right to left, making somewhat sudden movements for twelve minutes*."

* The earth has been further inactive to the date when this sheet is passing through the press, July 27, 1861.

I have made no reference in this communication to feeble currents, which are possibly at all times to be collected from the earth, and in which a periodicity has been traced, but have strictly confined myself to the larger disturbances, the concomitants of "Magnetic Storms." Nor have I made any attempt to trace the origin of these earth-currents, or to offer any theoretical views thereon. I find current-electricity in the earth in a very marked degree at certain times; I simply take it as I find it, and endeavour to arrange the facts in some degree of order, so as to throw a little more light than we have heretofore possessed upon these interesting phenomena. I have touched very lightly upon terrestrial magnetism, and have given no suggestion as to the probable causes of disturbance, save in the one case that necessarily arises out of the present inquiry. Other influences than those exerted by electric currents upon magnets may or may not be in play; but one thing is very certain, that at least a large portion of the motion presented by the magnetometers on storm-days is connected with the then prevalence of earth-currents; and doubtless some portion of all the more regular and less violent disturbances may be more or less due to the same causes. At any rate, although we are considerably in the dark as to the forms of force in operation to make up the *whole* of the causes concerned in magnetic disturbance, we are yet quite certain that the current-form of force is at least in *part* concerned. But we can collect this force, and measure it, and deal with it independently. We can receive the results and record them photographically, as foreshadowed by the Astronomer Royal, side by side with those presented by the magnetometers. And doubtless should such combined results come at any future day under discussion, and the more so should they pass into the hands of General SABINE, he would devise a method of eliminating the values due to these known causes, that is, due to earth-currents absolutely collected, and would by so doing render the values thus corrected more manageable, and might get one step nearer towards penetrating into the more recondite causes of the earth's magnetism and its variations. This will not be accomplished until Mr. AIRY'S suggestion is brought to bear, and the duties of a magnetic observatory shall be extended to the observation of earth-currents. I hope, ere long, that some attempts of this kind may be made. Preliminary observations are necessary before endeavours are made to organize a system that shall admit of general application. An electrical survey of the mass of the earth promises to be rich in results akin to those presented by the magnetic survey. They are evidently twin phenomena. The magnetic survey requires three instruments and their adjuncts, and a considerable amount of delicate manipulation. An electrical survey would require, as far as one can yet see, a single instrument only, after the resultant line for the place had been determined, and no larger amount, if so large, of manipulative skill.

TABLE I.—Magnetic Storm: 1859, August 29 to September 2.

Action of Earth-Currents upon the Telegraph Instruments at Ramsgate.

Date.	Time.	Telegraph lins.	Direction.	Value.
August 29	7.10 to 7.25 A.M.	Ashford and Margate.	S.	Strong.
	7.36 7.45	Ashford and Margate.	S.	Hard over.
	7.46 7.49	Ashford and Ramsgate.	N.	Strong.
	7.50 8.0	Ashford and Margate.	S.	Hard over.
	9.45 10.0	"	N.	Hard over.
	10.20 10.27	"	N.	Strong.
	10.27 10.28	"	S.	Hard over.
	10.28 10.36	Ashford and Margate.	N.	Strong.
	10.37 10.40	Ashford and Ramsgate.	S.	Hard over.
	10.40 10.45	Ashford and Margate.	N.	"
	10.45 10.49	"	S.	"
	10.50 10.53	"	N.	Hard over.
	10.53 11.0	"	S.	Horizontal.
	11.2 11.25	"	N.	Horizontal.
	11.26 11.40 A.M.	"	N.	Hard over.
	11.45 12.20 P.M.	"	N.	Hard over.
	12.30 P.M. 12.45	"	N.	Strong.
	12.48 1.3	"	N.	"
	1.5 1.40	"	S.	Strong.
	2.40 2.53	"	N.	Very strong.
	3.40 3.50	"	N.	Very strong.
	3.52 4.5	Ashford and Margate.	S.	Horizontal.
	3.52 4.5	Ashford and Ramsgate.	S.	Very strong
	4.15 4.50	Ashford and Margate.	N.	"
	5.0 5.20	Ashford and Margate.	N.	"
	5.0 5.20	Ashford and Ramsgate.	N.	"
	5.25 5.48	Ashford and Margate.	N.	"
	6.10 6.23	"	S.	"
	6.50 7.20	"	S.	Very strong.
August 29	7.53 8.10	"	S.	Slight.
September 1	11.20 A.M. 11.26 A.M.	"	S.	Strong.
September 1	11.28 11.35	"	N.	Slight.
September 2	7.10 A.M. 7.42 A.M.	Ashford and Margate.	N.	Horizontal.
	7.10 7.50	Ramsgate and Margate.	N.	"
	7.10 7.42	Ashford and Ramsgate.	N.	Horizontal.
	7.43 7.48	Ashford and Margate.	S.	Strong.
	7.43 7.48	Ashford and Ramsgate.	S.	Strong.
	7.49 7.51	Ashford and Margate.	N.	Hard over.
	7.49 7.51	Ashford and Ramsgate.	N.	"
	7.51 7.56	Ashford and Ramsgate.	S.	"
	7.51 7.56	Ashford and Margate.	S.	"
	7.56 8.0	Ashford and Margate.	N.	"
	7.56 8.0	Ashford and Ramsgate.	N.	"
	7.56 8.0	Ramsgate and Margate.	N.	Hard over.
	8.0 8.7	Ashford and Margate.	S.	Strong.
	8.0 8.7	Ashford and Ramsgate.	S.	"
	8.0 8.7	Margate and Ramsgate.	S.	"
	8.8 8.12	Ashford and Margate.	S.	"
	8.8 8.12	Ashford and Ramsgate.	S.	"
	8.8 8.17	Ramsgate and Margate.	S.	Strong.
	8.12 8.17	Ashford and Ramsgate.	S.	Hard over.
	8.12 8.17	Ashford and Margate.	S.	"
	8.20 8.30	Ashford and Ramsgate.	N.	"
	8.20 8.30	Ashford and Margate.	N.	"
	8.31 8.46	Ashford and Margate.	S.	"
	8.31 8.40	Ashford and Ramsgate.	S.	"
	8.41 8.46	Ashford and Ramsgate.	N.	"
	8.41 8.46	Ashford and Margate.	N.	"
	8.47 8.54	Ashford and Margate.	S.	"
	8.47 8.54	Ashford and Ramsgate.	S.	Hard over.
	8.54 9.0	Ashford and Ramsgate.	N.	Strong.
September 2	8.54 to 9.0 A.M.	Ashford and Margate.	N.	Strong.

TABLE I. (continued).

Date.	Time.	Telegraph line.	Direction.	Value.
September 2	9.22 to 9.25 A.M.	Ashford and Margate.	N.	Strong.
	9.26 9.28	"	S.	"
	9.29 9.40	"	N.	"
	9.40 9.52	"	S.	"
	9.55 10.32	Ashford and Margate.	N.	"
	9.55 10.32	Ashford and Ramsgate.	N.	"
	10.35 10.38	Ashford and Margate.	S.	"
	10.38 10.40	"	N.	"
	10.41 10.46	"	S.	"
	10.55 11.0	"	S.	"
	11.2 11.15	Ashford and Margate.	N.	"
	11.2 11.15	Ashford and Ramsgate.	N.	"
	11.16 11.27	Ashford and Margate.	S.	"
	11.20 11.32	Ashford and Ramsgate.	S.	"
	11.38 11.40	Ashford and Margate.	N.	"
	11.40 11.45	"	S.	"
	11.45 11.49	Ashford and Margate.	N.	"
	11.45 11.49	Ashford and Ramsgate.	N.	"
	11.45 11.50	Margate and Ramsgate.	N.	"
	11.50 11.51	Margate and Ashford.	S.	"
	11.50 11.51	Ramsgate and Ashford.	S.	"
	11.50 11.51	Ramsgate and Margate.	S.	"
	11.52 11.54	Ramsgate and Ashford.	N.	"
	11.52 11.54	Ramsgate and Margate.	N.	"
	11.52 to 11.54 A.M.	Ramsgate and Ashford.	N.	"
	11.59 to 12.3 P.M.	Ramsgate and Ashford.	N.	"
	11.59 12.3	Ashford and Margate.	N.	"
	11.59 12.3	Ramsgate and Margate.	N.	Strong.
	12.4 12.14	Ashford and Margate.	S.	Horizontal.
	12.4 12.14	Ashford and Ramsgate.	S.	Strong.
	12.15 12.30	Ashford and Margate.	N.	Horizontal.
	12.15 12.30	Ashford and Ramsgate.	N.	Strong.
	12.30 12.35	Ashford and Margate.	N.	"
	12.30 12.35	Ashford and Ramsgate.	N.	"
	12.30 12.35	Margate and Ramsgate.	N.	"
	12.36 12.57	Margate and Ashford.	N.	"
	12.36 12.57	Margate and Ramsgate.	N.	"
	12.36 12.57	Ashford and Ramsgate.	N.	"
	12.57 1.18	Ashford and Margate.	S.	"
	12.57 1.18	Ashford and Ramsgate.	S.	"
	12.57 1.18	Margate and Ramsgate.	S.	"
	1.20 1.44	Ashford and Margate.	N.	"
	1.20 1.44	Ashford and Ramsgate.	N.	"
	1.20 1.44	Margate and Ramsgate.	N.	"
	1.44 1.47	Ashford and Ramsgate.	S.	"
	1.44 1.47	Ashford and Margate.	S.	"
	1.47 1.54	Ashford and Margate.	N.	"
	1.47 1.54	Ashford and Ramsgate.	N.	"
	2.0 2.15	Ashford and Margate.	N.	"
	2.0 2.15	Ashford and Ramsgate.	N.	"
	2.15 2.18	Ashford and Margate.	S.	"
	2.15 2.18	Ashford and Ramsgate.	S.	Strong.
	2.21 2.31	Ashford and Margate.	S.	Horizontal.
	2.21 2.37	Ashford and Ramsgate.	S.	Horizontal.
	2.38 2.52	Margate and Ramsgate.	N.	Strong.
	2.38 2.52	Ashford and Ramsgate.	N.	"
	2.38 2.52	Ashford and Margate.	N.	"
	2.52 2.55	Ramsgate and Margate.	S.	"
	2.52 2.55	Ramsgate and Ashford.	S.	"
	2.52 2.55	Margate and Ashford.	S.	"
	2.55 3.2	Margate and Ramsgate.	N.	"
	2.55 3.2	Ashford and Ramsgate.	N.	"
September 2	2.55 to 3.2 P.M.	Ashford and Margate.	N.	Strong.

Table II.—Galvanometer Tests.

1. In short circuit.

Battery: Zinc and Platinized Graphite, 4 in. × 2 in.

	Right.	Left.	Mean.
	°	°	°
1 cell	37	37	37
2 cells	56	58	57
3 cells	64	68	66
4 cells	68	73	70½
5 cells	71	77	74
6 cells	73	79	76

2. In Margate—Ashford Telegraph Circuit, 51½ miles.

Battery: Zinc and Copper in sand, 3 in. × 3 in.

	°
3 cells	5
6 cells	13
12 cells	26
18 cells	32
24 cells	44
30 cells	48
36 cells	54
42 cells	59
48 cells	62

Good telegraph signals produce			60
Middling	„	„	54
Weak	„	„	40

TABLE III.—Direction, Duration, Changes and Values of Earth-Currents.

Date.	Time.	General direction from N. or S.					
		Duration.		Values. Margate To Ashford (27½ miles).		To Ramsgate (3 miles).	
		N.	S.	N.	S.	N.	S.
	h m h m	min.	min.	°	°	°	°
August 8, 1860.	2.55 to 3.5 P.M.	.....	10	.....	10	.....	8
	3.5 3.20	.....	.....	20	.....	18	
	3.20 3.23	18	.....	70	.....	76	
	3.23 3.24	.....	1	.....	20	.....	12
	3.24 3.25	.....	.....	23	.....	18	
	3.25 3.27	3	.....	40	.....	25	
	3.27 3.28	.....	1	.....	28	.....	29
	3.28 3.53	25	.....	82	.....	71	
	3.53 3.54	.....	1	.....	23	.....	15
	3.54 3.55	1	.....	45	.....	27	
	3.55 3.56	.....	1	.....	64	.....	34
	3.56 3.58	2	.....	48	.....	31	
	3.58 4.11	.....	13	.....	62	.....	55
	4.11 4.23	11	.....	74	.....	70	
	4.24 4.30	.....	6	.....	20	.....	10
	4.30 4.31	1	.....	10	.....	7	
	4.31 4.34	.....	3	.....	26	.....	27
	4.34 4.36	2	.....	12	.....	10	
	4.38 5.13	.....	.....	.....	60	.....	63
	5.13 5.17	.....	39	.....	40	.....	24
	5.17 5.19	2	.....	29	.....	23	
	5.19 5.20	.....	1	.....	40	.....	24
	5.20 5.23	3	.....	40	.....	24	
	5.23 5.24	.....	1	.....	12	.....	9
	5.24 5.25	1	.....	13	.....	8	
	5.25 5.29	.....	.....	.....	60	.....	73
	5.29 5.44	.....	19	.....	73	.....	86
	5.44 5.48	4	.....	40	.....	34	
	5.49 5.53	.....	4	.....	23	.....	10
	5.53 5.59	.....	.....	35	.....	21	
	6.0 6.12	18	.....	38	.....	25	
	6.12 6.13	.....	1	.....	20		
	6.13 6.14	1	.....	25	.....	12	
	6.14 6.16	.....	2	.....	16	.....	6
	6.16 6.17	1	.....	18	.....	10	
	6.17 6.18	.....	.....	.....	10	.....	6
	6.19 6.21	.....	4	.....	35	.....	14
	6.21 6.25	4	.....	42	.....	32	
	6.25 6.26	.....	.....	.....	48	.....	35
	6.27 6.40	.....	15	.....	60	.....	43
	6.45 6.49	4	.....	10	.....	9	
	7.15 7.25	.....	10	.....	40	.....	18
	7.25 7.33	8	.....	20	.....	15	
	7.33 7.40	.....	7	.....	39	.....	27
	7.40 7.55	15	.....	20	.....	23	
	7.55 8.10	.....	15	.....	40	.....	43
	8.10 8.20	10	.....	50	.....	40	
	8.20 8.28	.....	8	.....	38	.....	27
	8.28 8.45	17	.....	20	.....	12	
	8.45 8.55	.....	10	.....	10	.....	7
	8.55 9.3	8	.....	20	.....	12	
August 8, 1860.	9.3 to 9.39 P.M.	.....	36	.....	39	.....	27
	Sum.....	159	208				
	Means.....	7·22	9·04	33·76	35·40		

TABLE IV.—Direction, Duration, Changes and Values of Earth-Currents.

Date.	Time.		General direction from N. or S.					
			Duration.		Values.			
					Margate			
					To Ashford (27¼ miles).		To Ramsgate (3 miles).	
			N.	S.	N.	S.	N.	S.
	h m	h m	min.	min.	°	°	°	°
August 9, 1860.	10.4	to 10.10 A.M.	6	.....	3			
	10.15	10.17	.....	2	.....	20		
	10.17	10.20	3	.....	14	.....	7	
	10.37	10.47	.....	10	.....	23	.....	12
	10.47	10.49	2	.....	10	.....	9	
	10.49	10.50	.....	.....	15			
	10.50	10.54	.....	.....	50			
	10.55	10.56	.....	7	.....	20	.....	12
	10.56	10.58	2	.....	12			
	10.58	10.59	.....	1	.....	16		
	11.0	11.0½	5	.....	13	.....	7	
	11.½	11.1½	.....	1	.....	40	.....	19
	11.2	11.2½	0·25	.....	20			
	11.2½	11.4	.....	1·75	.....	54	.....	43
	11.4	11.5	1	.....	30	.....	6	
	11.5	11.7	.....	2	.....	50	.....	52
	11.7	11.8	1	.....	10	.....	7	
	11.8	11.10	.....	2	.....	20	.....	15
	11.10	11.11	1	.....	20	.....	23	
	11.11	11.13	.....	2	.....	30	.....	25
	11.13	11.20	.....	.....	26	.....	19	
	11.20	11.21	8	.....	10			
	11.21	11.21½	.....	0·5	.....	10	.....	8
	11.22	11.26	4	.....	40	.....	24	
	11.26	11.27	.....	.....	.....	12	.....	9
	11.28	11.35	.....	9	.....	20	.....	18
	11.35	11.36	1	.....	10	.....	8	
	11.36	11.37	.....	1	.....	3		
	11.37	11.37½	0·5	.....	10			
	11.38	11.38½	.....	0·25	.....	4		
	11.38½	11.40	1·75	.....	30	.....	25	
	11.41	11.43	.....	2	.....	12		
	11.43	11.46	3	.....	18	.....	8	
	11.46	11.48	.....	2	.....	10		
	11.48	11.50	2	.....	14			
	11.50	11.55 A.M.	.....	5	.....	20	.....	18
	12.0	12.3 P.M.	3	.....	40	.....	40	
	12.3	12.5	.....	2	.....	50	.....	31
	12.5	12.10	.....	.....	10	.....	7	
	12.10	12.12	7	.....	14	.....	9	
	12.12	12.15	.....	3	.....	20		
	12.15	12.16	1	.....	10			
	12.16	12.18	.....	2	.....	20		
	12.18	12.19	1	.....	10			
	12.19	12.21	.....	2	.....	34	.....	31
	12.21	12.22	1	.....	12	.....	7	
	12.22	12.24	.....	2	.....	20	.....	9
	12.24	12.27	3	.....	14	.....	18	
	12.27	12.28	.....	1	.....	4	.....	4
	12.28	12.30	2	.....	10	.....	10	
	12.30	12.32	.....	2	.....	22	.....	12
	12.33	12.35	2	.....	10	.....	11	
	12.39	12.41	.....	2	.....	20	.....	9
	12.42	12.44	2	.....	10	.....	9	
August 9, 1860.	12.44	to 12.48 P.M	.....	4	.....	6	.....	6

TABLE IV. (continued.)

Date.	Time.	General direction from N. or S.					
		Duration.		Values. Margate To Ashford (27¼ miles).		To Ramsgate (3 miles).	
		N.	S.	N.	S.	N.	S.
	h m h m	min.	min.	°	°	°	°
August 9, 1860.	12.48 to 12.50 P.M.	2	.....	7	.....	8	
	12.50 1.10	.....	20	.....	14	.....	9
	1.10 1.31	.....	.....	36	.....	15	
	1.32 2.0	50	.....	36	.....	18	
	2.0 2.5	.....	5	.....	10		
	2.7 2.10	.....	3	.....	18		
	2.10 2.11	1	.....	20	.....	10	
	2.11 2.12	.....	1	.....	22	.....	10
	2.12 2.14	2	.....	.....	.....	12	
	2.16 2.19	.....	.....	30	.....	18	
	2.19 2.23	.....	.....	40	.....	31	
	2.23 2.29	13	.....	30	.....	24	
	2.29 2.29½	.....	0·5	.....	16	.....	7
	2.29½ 2.34	.....	.....	48	.....	35	
	2.34½ 2.50	.....	.....	54	.....	38	
	2.50½ 3.2	32·5	.....	38	.....	9	
	3.3 3.6	.....	3	.....	25	.....	12
	3.8 3.20	12	.....	50	.....	27	
	3.21 3.24	.....	.....	.....	12	.....	6
	3.25 3.33	.....	12	.....	36	.....	15
	3.36 3.55	.....	19	.....	44	.....	12
	3.57 3.59	2	.....	12	.....	6	
	4.0 4.2	2	.....	30	.....	15	
	4.13 4.15	2	.....	13	.....	6	
	4.16 4.18	.....	2	.....	20	.....	8
	4.19 4.23	4	.....	20	.....	10	
	4.25 4.31	6	.....	60	.....	34	
	4.32 4.38	.....	6	.....	28	.....	12
	4.39 4.40	1	.....	10	.....	6	
	4.41 4.43	.....	2	.....	20	.....	10
	4.44 4.46	.....	2	.....	10	.....	6
	4.46 4.47	1	.....	8			
	4.49 4.53	.....	4	.....	20	.....	13
	4.55 4.57	.....	.....	30	.....	15	
	4.57 5.7	12	.....	40	.....	18	
	5.8 5.12	4	.....	42	.....	24	
	5.13 5.15	2	.....	38			
	5.16 5.25	9	.....	16	.....	10	
	5.25 5.43	.....	18	.....	24	.....	10
	5.45 5.49	.....	4	.....	18	.....	6
	6.0 6.1	.....	1	.....	10	.....	6
	6.3 6.8	.....	.....	42	.....	24	
	6.8½ 6.26	23	.....	60	.....	38	
	6.26 6.28	.....	2	.....	14	.....	8
	6.30 6.32	.....	2	.....	12	.....	8
	6.36 6.46	.....	10	.....	26	.....	12
	6.50 7.2	.....	12	.....	40	.....	21
	7.4 7.7	.....	3	.....	30	.....	15
	7.8 7.27	.....	19	.....	28	.....	12
	7.28 7.42	14	.....	40	.....	24	
	7.45 8.10	.....	25	.....	45	.....	32
August 9, 1860.	8.12 to 8.20 P.M.	.....	8	.....	18	.....	15
	Sum	253½	252				
	Means	5·63	5·14	24·45	21·96		

TABLE V.—Direction, Duration, Changes and Values of Earth-Currents.

Date.	Time.	General direction from N. or S.					
		Duration.		Values.			
				Margate			
				To Ashford (27½ miles).		To Ramsgate (3 miles).	
		N.	S.	N.	S.	N.	S.
	h m h m	min.	min.	°	°	°	°
August 10, 1860.	12.13 to 12.17 P.M.	.....	4	.....	19		
	12.21 12.25	.....	4	.....	24		
	12.40 12.42	2	.....	30	.....	15	
	12.43 12.50	.....	7	.....	10	.....	6
	1.1 1.3	.....	2	.....	20	.....	15
	1.4 1.5	1	.....	10	.....	6	
	1.6 1.9	3	.....	22	.....	12	
	1.9 1.20	.....	11	.....	34	.....	12
	1.20 1.24	4	.....	38	.....	21	
	1.24 1.32	.....	8	.....	36	.....	12
	1.33 1.38	5	.....	20	.....	10	
	1.38 1.42	.....	4	.....	10	.....	6
	1.43 1.49	6	.....	12	.....	6	
	2.2 2.11	.....	.....	8	.....	8	
	2.12 2.49	47	.....	12	.....	6	
	2.57 3.10	.....	13	.....	12	.....	6
	3.27 3.33	.....	6	.....	10		
	3.34 3.45	.....	11	.....	18		
	4.8 4.12	4	.....	40	.....	24	
	4.16 4.21	.....	5	.....	20	.....	10
	4.30 4.32	.....	2	.....	17	.....	4
	4.40 4.43	3	.....	50	.....	38	
	4.45 4.47	2	.....	58	.....	32	
	4.55 4.57	2	.....	50	.....	35	
	4.59 5.15	16	.....	70	.....	53	
August 10, 1860.	5.18 to 6.14 P.M.	.....	56	.....	60	.....	38
	Sum	95	133				
	Means	7·91	9·46	32·30	22·30		

TABLE VI.—Direction, Duration, Changes and Values of Earth-Currents.

Date.	Time.	General direction from N. or S.					
		Duration.		Values.			
				Margate. To Ashford (27½ miles).		To Ramsgate (3 miles).	
		N.	S.	N.	S.	N.	S.
	h m h m	min.	min.	°	°	°	°
August 11, 1860.	7.50 to 8.0 A.M.	.....	10	.....	40	.....	32
	8.0 8.2	2	.....	24	.....	12	
	8.4 8.7	.....	3	.....	26	.....	10
	8.7 8.20	13	.....	20	.....	8	
	8.26 8.33	.....	7	.....	18		
	8.39 8.47	.....	8	.....	10		
	8.47 8.48	1	.....	30 (?)			
	8.48 8.49	.....	1	.....	40 (?)		
	8.49 8.50	1	.....	30 (?)			
	8.50 8.55	.....	1	.....	10		
	8.55 8.55½	0·5	.....	3			
	8.55½ 9.5	.....	9·5	.....	54	.....	35
	9.15 9.20	.....	.....	20	.....	15	
	9.20 9.22	7	.....	10			
	9.23 9.30	.....	7	.....	15	.....	6
	9.30 9.40	10	.....	10 (?)			
	9.40 10.12	.....	32	.....	20		
	10.12 10.14	2	.....	20	.....	6	
	10.14 10.24	.....	.....	.....	18	.....	15
	10.24 10.30	.....	16	.....	30	.....	10
	10.36 10.50	.....	.....	18 (?)			
	10.50 10.54	.....	.....	38	.....	32	
	10.54 11.0	24	.....	18 (?)			
	11.0 11.10	.....	10	.....	34		
	11.10 11.13	3	.....	12			
	11.14 11.36	.....	.....	20	.....	8	
	11.36 11.38	.....	.....	30	.....	10	
	11.38 11.43	29	.....	20	.....	15	
	11.43 11.45	.....	2	.....	20	.....	15
	11.45 11.55 A.M.	10	.....	18 (?)			
	11.55 12.18 P.M.	.....	23	.....	20	.....	15
	2.31 2.34	.....	3	.....	18	.....	15
	2.36 2.40	.....	.....	15	.....	10	
	2.40 2.55	19	.....	30 (?)			
	2.57 2.59	2	.....	30 (?)			
	2.59 3.5	.....	.....	.....	60	.....	15
	3.5 3.17	.....	18	.....	16	.....	8
	3.17 3.19	2	.....	46	.....	27	
	3.27 4.10	43	.....	16	.....	10	
	4.23 4.28	5	.....	22	.....	12	
	4.32 4.50	18	... .	36	.....	15	
	4.50 4.51	.....	1	.....	30 (?)		
	4.51 5.26	35	.....	36	.....	29	
August 11, 1860.	5.27 5.45 P.M.	.....	18	.....	10 (?)		
	Sum	226½	169½				
	Means	11·92	9·97	22·88	25·73		

TABLE VII.—Direction, Duration, Changes and Values of Earth-Currents.

Date.	Time.		General direction from N. or S.					
			Duration.		Values.			
					Margate			
					To Ashford (27¼ miles).		To Ramsgate (3 miles)	
			N.	S.	N.	S.	N.	S.
	h m	h m	min.	min.	°	°		
August 12, 1860.	9.30 to	9.52 A.M.	.....	22	.....	68	.....	?
	9.52	10.2	10	.....	36	.....	?	
	10.2	10.10	.....	12	.....	74	.....	?
	10.10	10.21	11	.....	48	.....	?	
	10.21	10.22	.....	1	.....	30	.....	?
	10.22	10.56	34	.....	74	.....	?	
	10.56	11.12	.....	16	.....	30	.....	?
	11.12	11.15	3	.....	32	.....	?	
	11.15	11.16	.....	1	.....	28	.....	?
	11.16	11.17	1	.....	44	.....	?	
	11.17	11.26	.....	9	.....	68	.....	?
	11.26	11.40	14	.....	30	.....	?	
	11.40	11.50 A.M.	.....	10	.....	15 (?)		
	11.51	12.10 P.M.	19	.....	10 (?)			
	12.10	12.15	.....	5	.....	35 (?)		
	12.15	12.34	29	.....	46	.....	?	
	12.34	12.45	.....	11	.....	50 (?)		
	12.46	12.55	9	.....	35			
	12.55	1.20	.....	25	.....	38	.....	(?)
	1.20	1.50	30	.....	20	.....	(?)	
	1.50	1.52	.....	2	.....	30 (?)		
	1.52	2.10	.....	.....	20			
	2.10	2.39	.....	.....	30			
	2.39	3.3	71	.....	50			
	3.4	3.6	.....	2	.....	30 (?)		
	3.7	3.10	3	.....	40			
	3.10	3.16	.....	6	.....	18 (?)		
	3.16	3.20	4	.....	8 (?)			
	3.20	3.45	.....	.....	.....	48	.....	(?)
	3.45	4.30	.....	70	.....	38		
	4.30	5.25	55	.....	50			
	5.36	5.45	.....	.....	.....	15 (?)		
	5.45	6.30	.....	.....	.....	38	.....	?
	6.30	7.35	.....	119	.....	15 (?)		
August 12, 1860.	7.55	8.1 P.M.	.....	6	.....	40		
	Sum.....		293	317				
	Means		20·92	19·81	35·81	37·26		

TABLE VIII.—Direction, Duration, Changes and Values of Earth-Currents.

Date.	Time.		General direction from N. or S.					
			Duration.		Values. Margate To Ashford (27¼ miles).		To Ramsgate (3 miles).	
			N.	S.	N.	S.	N.	S.
	h m	h m	min.	min.	°	°	°	°
Sept. 7, 1860 ...	7.12 to	7.18 A.M.	.....	6	.....	44		
	7.18	7.19	1	.....	8			
	7.19	7.19½	.....	0·5	.....	6	.....	8
	7.19½	7.22	1·5	.....	64	.....	40	
	7.22	7.24	.....	2	.....	44	.....	48
	7.24	7.28	4	.....	34	.....	16	
	7.28	7.29	.....	.....	.....	38	.....	57
	7.29½	7.30	.....	.....	.....	16	.....	34
	7.30½	7.33	.....	.....	.....	58	.....	61
	7.33	7.40	.....	12	.....	8		
	7.40	7.42	2	.....	30			
	7.42	7.44	.....	2	.....	20	.....	15
	7.44	7.45	1	.....	22	.....	21	
	7.45	7.46	.....	.....	.....	30	.....	12
	7.46½	7.47	.....	2	.....	38	.....	21
	7.47	7.50	.....	.....	40	.....	34	
	7.50	7.52	5	.....	18	.....	10	
	7.52	7.53	.....	1	.....	14	.....	8
	7.53	7.57	4	.....	30	.....	34	
	7.57	7.58	.....	1	.....	8		
	7.58	8.0	2	.....	20	.....	34	
	8.1	8.2	.....	1	.....	20	.....	10
	8.2	8.3	1	.....	10	.....	6	
	8.3	8.5	.....	.....	.....	20	.....	10
	8.6	8.6½	.....	3·5	.....	10	.....	6
	8.6½	8.7	0·5	.....	6	.....	10	
	8.7	8.8	.....	.....	.....	20	.....	15
	8.8½	8.18	.....	11	.....	38	.....	24
	8.18	8.19	1	.....	16	.....	8	
	8.19	8.20	.....	.....	.....	12	.....	8
	8.21	8.24	.....	5	.....	18	.....	10
	8.24	8.25	1	.....	24	.....	8	
	8.25	8.27	.....	2	.....	12		
	8.27	8.27½	0·5	.....	6			
	8.27½	8.28	.....	1	.....	8		
	8.28	8.29	.....	.....	12			
	8.29	8.30	2	.....	12			
	8.30	8.32	.....	2	.....	12	.....	18
	8.32	8.32½	0·5	.....	9			
	8.32½	8.33	.....	0·5	.....	5		
	8.33	8.35	2	.....	12			
	8.35	8.36	.....	1	.....	3		
	8.36	8.37	1	.....	4			
	8.37	8.38	.....	1	.....	5		
	8.38	8.42	4	.....	36	.....	15	
	8.42	8.46	.....	4	.....	26	.....	15
	8.46	8.48	2	.....	6	.....	6	
	8.48	8.49						
	8.49	8.51	.....	3	.....	8		
	8.51	9.1	10	.....	20	.....	8	
	9.1	9.2	.....	1	.....	10	.....	9
Sept. 7, 1860 ...	9.2 to	9.5 A.M.	3					

TABLE VIII. (continued).

Date.	Time.		General direction from N. or S.					
			Duration.		Values.			
					Margate			
					To Ashford (27¼ miles).		To Ramsgate (3 miles).	
			N.	S.	N.	S.	N.	S.
	h m	h m	min.	min.	°	°	°	°
Sept. 7, 1860 ...	9.5	9.6 A.M.	.....	1	.....	10	.....	9
	9.6	9.7	1	.....	5			
	9.7	9.10	.....	3	.....	11	.....	10
	9.10	9.13	.....	.....	11			
	9.13	9.14	4	.....	5			
	9.14	9.14½	.....	0·5	.....	4	.....	5
	9.14½	9.15	0·5	.....	5			
	9.15	9.15½	.....	.....	.....	18		
	9.16	9.18	.....	3	.....	10	.....	9
	9.19	9.20	1	.....	5			
	9.20	9.21	.....	1	.....	4	.....	5
	9.21	9.22	2	.....	6			
	9.22	9.25	.....	3	.....	50	.....	45(?)
	9.25	9.28	3	.....	40	.....	43(?)	
	9.28	9.30	.....	2	.....	18	.....	18
	9.30	9.36	6	.....	30	.....	15	
	9.36	9.37	.....	1	.....	25	.....	18
	9.37	9.40	3	.....	24	.....	15	
	9.40	9.43	.....	3	.....	27	.....	25
	9.43	9.43½	0·5	.....	29	.....	12	
	9.43½	9.46	.....	.....	.....	28	.....	13
	9.46	9.55	.....	.....	.....	14	.....	10
	9.55	9.56	.....	12·5	.....	20	.....	23
	9.56	9.58	2	.....	12	.....	10	
	9.58	9.59	.....	1	.....	24	.....	28
	9.59	10.0	1	.....	?			
	10.0	10.6	.....	6	.....	43(?)	.....	18
	10.6	10 8	.....	.....	10	.....	8	
	10.8	10.10	4	.....	45			
	10.10	10.14	.....	4	.....	24	.....	15
	10.14	10.15	1	.....	9			
	10.15	10.19	.....	4	.....	25		
	10.19	10.20	1	.....	10			
	10.20	10.21	.....	6	.....	.....	5	
	10.21	10.22	1	.....	7			
	10.22	10.23	.....	1	.....	13	.....	12
	10.23	10.30	.....	.....	17	.....	9	
	10.30	10.32	.....	.....	6			
	10.32	10.34	.....	.....	9			
	10.34	10.50	27	.....	17			
	10.50	10.52	.....	2	.....	8		
	10.52	10.52½	.....	.....	6			
	10.52½	10.54	4	.....	9			
	10.54	10.55	.....	1	.....	4		
	10.55	10.58	.....	.....	17	.....	8	
	10.58	11.0	5	.....	19			
	11.0	11.4	.....	4	.....	34	.....	12
	11.4	11.7	3	.....	12	.....	24	
	11.7	11.8	.....	.....	.....	8		
	11.8	11.10	.....	3	.....	28	.....	15
	11.10	11.12	2	.....	10			
Sept. 7, 1860 ...	11.12	to 11.14 A.M.	.....	2	.....	27		

TABLE VIII. (continued).

Date.	Time.		General direction from N. or S.					
			Duration.		Values.			
					Margate To Ashford (27½ miles).		To Ramsgate (3 miles).	
			N.	S.	N.	S.	N.	S.
	h m	h m	min.	min.	°	°	°	°
Sept. 7, 1860 ...	11.14	to 11.15 A.M.	.....	.....	36	.....	24	
	11.15	11.18	.....	.....	30	.....	24	
	11.18	11.20	6	.....	12	.....	21	
	11.20	11.22	.....	2	.....	18	.....	10
	11.22	11.23	1	.....	50	.....	54	
	11.23	11.25	.....	2.5	.....	25		
	11.25	11.25½	.....	.....	.....	18	.....	12
	11.25½	11.26	0·5	.....	34			
	11.26	11.28	.....	2	.....	60 (?)		
	11.28	11.30	2	.....	38	.....	7	
	11.30	11.33	.....	3	.....	18	.....	12
	11.33	11.36	3	.....	20	.....	12	
	11.36	11.39	.....	3	.....	10	.....	10
	11.39	11.44	5	.....	23	.....	23	
	11.44	11.48	.....	4	.....	26	.....	24
	11.48	11.53	5	.....	8	.....	6	
	11.53	11.57	.....	4	.....	14	.....	13
	11.57	11.59 A.M.	2	.....	17			
	11.59	12.2 P.M.	.....	3	.....	13	.....	12
	12.3	12.7	4	.....	12			
	12.7	12.10	.....	3	.....	10	.....	7
	12.13	12.14	.....	1	.....	10	.....	7
	12.14	12.19	5	.....	25			
	12.19	12.20	.....	1	.....	17		
	12.20	12.20½	0·5	.....	10			
	12.21	12.21½	.....	0·5	.....	10		
	12.22	12.22½	0·5	.....	25			
	12.22½	12.25	.....	2·5	.....	34	.....	29
	12.25	12.28	3	.....	50	.....	27	
	12.28	12.29	.....	1	.....	50	.....	27
	12.29	12.31	2	.....	27			
	12.31	12.34	.....	3	.....	17	.....	12
	12.34	12.35	1	.....	9			
	12.35	12.37	.....	2	.....	24	.....	23
	12.44	12.50	.....	.....	17	.....	12	
	12.50	12.58	.....	.....	64			
	12.58	1.0	.....	.....	17			
	1.0	1.5	15	.....	30	.....	27	
	1.5	1.9	.....	4	.....	35		
	1.9	1.13	4	.....	20	.....	5	
	1.13	1.14	.....	1	.....	12	.....	10
	1.15	1.20	5	.....	12	.....	6	
	1.20	1.22	.....	2				
	1.22	1.25	.....	.....	17			
	1.25	1.40	18	.....	25	.....	27	
	1.40	1.44	.....	.....	(?)			
	1.44	1.47	.....	7	(?)			
	1.53	1.54	.....	1	(?)			
	1.59	2.0	.....	1	(?)			
	2.0	2.3	3	.....	8	.....	8	
	2.3	2.7	.....	4	.....	18	.....	12
Sept. 7, 1860 ...	2.7	2.10 P.M.	3	.....	12	.....	10	

TABLE VIII. (continued).

Date.	Time.		General direction from N. or S.					
			Duration.		Values.			
					Margate			
					To Ashford (27½ miles).		To Ramsgate (3 miles).	
			N.	S.	N.	S.	N.	S.
	h m	h m	min.	min.	°	°	°	°
Sept. 7, 1860 ...	2.10	to 2.11 P.M.	.....	1	.....	8	.....	8
	2.11	2.15	4	.....	34	.....	24	
	2.15	2.20	.....	5	.....	24		
	2.20	2.30	10	.....	28			
	2.30	2.32	.....	2	.....	16		
	2.32	2.35	.....	.....	14			
	2.35	2.37	.....	.....	(?)			
	2.37	2.38	6	.....	16			
	2.38	2.40	.....	2	.....	12	.....	12
	2.40	2.43	3	.....	7			
	2.43	2.45	.....	2	.....	10	.....	5
	2.45	2.48	3	.....	11			
	2.48	2.49	.....	1	.....	9		
	2.49	2.50	1	.....	8			
	2.50	2.55	.....	5	.....	11		
	2.55	2.59	4	.....	10			
	2.59	3.0	.....	1	.....	12		
	3.0	3.5	5	.....	16			
	3.5	3.7	.....	2	.....	4		
	3.7	3.9	2	.....	10			
	3.9	3.10	.....	1	.....	8		
	3.10	3.17	7	.....	14	.....	10	
	3.17	3.18	.....	1	.....	8		
	3.18	3.20	2	.....	14	.....	10	
	3.21	3.25	.....	4	.....	20	.....	12
	3.25	3.28	3	.....	19			
	3.28	3.29	.....	1	.....	4		
	3.29	3.40	11	.....	38	.....	18	
	3.40	3.50	.....	10	.....	8	.....	7
	3.50	4.5	.....	.....	0			
	4.5	4.7	2	.....	10			
	4.7	4.8	.....	1	.....	5		
	4.8	4.10	2	.....	7			
Sept. 7, 1860 ...	4.10	4.40 P.M.	.....	30	.....	12	.....	12
		Sum	274½	253				
		Means	3·51	3·12	18·87	18·61		

ANALYSES OF EARTH-CURRENTS.

TABLE IX.—Values in Time.

a. Daily Numbers for each Value.

Duration.	August 8.	August 9.	August 10.	August 11.	August 12.	September 7.	Sums.
minutes.							
$\frac{1}{4}$	0	2	0	0	0	0	2
$\frac{2}{4}$	0	4	0	1	0	12	17
$\frac{3}{4}$	0	0	0	0	0	0	0
Under 1	0	6	0	1	0	12	19
1	12	16	1	5	3	43	80
1½	0	0	0	0	0	1	1
1½	0	2	0	0	0	0	2
2	4	29	5	5	2	30	75
2½	0	0	0	0	0	2	2
3	3	8	2	3	2	22	40
3½	0	0	0	0	0	1	1
4	5	6	5	0	1	19	36
5	0	2	2	1	1	10	16
1 to 5	24	63	15	14	9	128	253
6 10	9	11	4	9	6	11	50
11 15	5	6	3	1	4	5	24
16 20	4	4	1	5	2	1	17
21 25	1	2	0	2	2	0	7
26 30	0	0	0	1	2	2	5
31 35	0	1	0	2	1	0	4
36 40	2	0	0	0	0	0	2
41 45	0	0	0	1	0	0	1
46 50	0	1	1	0	0	0	2
51 55	0	0	0	0	1	0	1
56 60	0	0	1	0	0	0	1
61 to 120	0	0	0	0	3	0	3
Sums	45	94	25	36	30	159	389

b. Total duration of each Current.

	August 8.	August 9.	August 10.	August 11.	August 12.	September 7.	Sums.
	min.	min.	min.	min.	min.	min.	h m
N. Currents ...	159	253·5	95	226·5	293	274·5	21 41·5
S. Currents.....	208	252	123	169·5	317	253	22 2·5
Wanting.....	37	110·5	143	66	21	40·5	6 58
Sums	404	616	361	462	631	568	50 42

c. Mean duration of each Current.

	August 8.	August 9.	August 10.	August 11.	August 12.	September 7.	Mean.
	min.	min.	min.	min.	min.	min.	min.
N. Currents ...	7·22	5·63	7·91	11·92	20·92	3·51	9·51
S. Currents.....	9·04	5·14	9·46	9·97	19·81	3·12	9·42
Mean	8·13	5·38	8·68	10·99	20·36	3·31	9·46

TABLE X.—Values in Degrees of Deflection.

a. Daily numbers for each Value.

Deflections.	Aug. 8.		Aug. 9.		Aug. 10.		Aug. 11.		Aug. 12.		Sept. 7.		Sums.		Total.
	N.	S.	N.	S.	N.	S.	N.	S.	N.	S.	N.	S.	N.	S.	
1°— 5°	0	...	1	...	0	...	1	...	0	...	5	...	7		
"	...	0	...	3	...	0	...	0	...	0	...	9	...	12	19
6—10	2	...	14	...	2	...	2	...	2	...	26	...	48		
"	...	3	...	6	...	3	...	3	...	0	...	21	...	36	84
11—15	2	...	10	...	2	...	2	...	0	...	14	...	30		
"	...	1	...	6	...	1	...	1	...	3	...	14	...	26	56
16—20	5	...	6	...	1	...	9	...	2	...	17	...	40		
"	...	4	...	17	...	5	...	7	...	1	...	17	...	51	91
21—25	2	...	0	...	1	...	2	...	0	...	7	...	12		
"	...	2	...	5	...	1	...	0	...	0	...	7	...	15	27
26—30	1	...	7	...	1	...	5	...	2	...	8	...	24		
"	...	2	...	5	...	0	...	3	...	5	...	7	...	22	46
31—35	1	...	0	...	0	...	0	...	2	...	3	...	6		
"	...	1	...	1	...	1	...	1	...	1	...	3	...	8	14
36—40	4	...	9	...	2	...	3	...	2	...	6	...	26		
"	...	7	...	3	...	1	...	2	...	4	...	3	...	20	46
41—45	2	...	2	...	0	...	0	...	1	...	1	...	6		
"	...	0	...	2	...	0	...	0	...	0	...	3	...	5	11
46—50	3	...	3	...	2	...	1	...	4	...	2	...	15		
"	...	1	...	2	...	0	...	0	...	2	...	2	...	7	22
51—55	0	...	1	...	0	...	0	...	0	...	0	...	1		
"	...	0	...	1	...	0	...	1	...	0	...	0	...	2	3
56—60	0	...	2	...	1	...	0	...	0	...	0	...	3		
"	...	3	...	0	...	1	...	1	...	0	...	2	...	7	10
61—65	0	...	0	...	0	...	0	...	0	...	2	...	2		
"	...	2	...	0	...	0	...	0	...	0	...	0	...	2	4
66—70	1	...	0	...	1	...	0	...	0	...	0	...	2		
"	...	0	...	0	...	0	...	0	...	2	...	0	...	2	4
71—75	1	...	0	...	0	...	0	...	1	...	0	...	2		
"	...	1	...	0	...	0	...	0	...	1	...	0	...	2	4
76—80	0	...	0	...	0	...	0	...	0	...	0	...	0		
"	...	0	...	0	...	0	...	0	...	0	...	0	...	0	0
81—85	1	...	0	...	0	...	0	...	0	...	0	...	1		
"	...	0	...	0	...	0	...	0	...	0	...	0	...	0	1
	25	... 27	55	... 51	13	... 13	25	19	16	19	91	88	225	217	442

b. Mean Value of each Current.

	Aug. 8.	Aug. 9.	Aug. 10.	Aug. 11.	Aug. 12.	Sept. 7.	Mean.
N. current	33°76	24°45	32°30	22°88	35°81	18°87	28°01
S. current	35°40	21°96	22°30	25°73	37°26	18°61	26°87
Each	34°58	23°20	27°30	24°30	36°53	18°74	27°44

Table XI.—Telegraph Stations.

Bearings and Distances.

		Bearings.	Distances.	
			Direct.	Telegraph.
	I. On the Magnetic Meridian.			
N. S.	Whitstable—Canterbury	° 0	miles. 5¾	miles. 5¾
	II. East of the Magnetic Meridian.	East of North.		
1— 2	Red Hill—Brighton	° 17	28½	29¾
3— 4	London—Brighton	25	46½	50½
5— 6	Ashford—Hastings	52	23½	26½
7— 8	London—Portsmouth	62	66½	95½
9—10	Maidstone—Paddock Wood	64	8½	9¾
11—12	Red Hill—Portsmouth	70	51½	71½
13—14	Margate—Ashford	72	27½	51¼
15—16	Ramsgate—Ashford	80	27½	30
	III. West of the Magnetic Meridian.	West of North.		
17—18	Margate—Ramsgate	° 2	3	3¾
19—20	Minster—Deal	6	7½	8¾
21—22	Tonbridge—Hastings	10	26	32½
23—24	London—Tonbridge	13	26½	40½*
25—26	London—Dover	44	65	87½
	Reading—Red Hill	44	38½	46½
27—28	Ashford—Folkestone	50	15	15½
29—30	Red Hill—Tonbridge	57	19	19¾
31—32	Tonbridge—Ashford	62	26½	26½
33—34	Red Hill—Shalford	82	18	18¼

* Or 57¼ miles *vid* Paddock Wood and Maidstone.

TABLE XII.—Absolute Direction of Earth-Currents.

1. Range 42°; extending from 46° E. of Magnetic N. to 88° E. of Magnetic N. (Fig. 4, Plate III.)

		1857, Dec. 17. 1.15 to 1.25 A.M.	1860, July 1. 3.50 to 4.30 P.M.	7.5 to 7.15 P.M.	7.40 to 8.3 P.M.	1860, August 8. 2.0 to 2.45 P.M.	3.28 to 3.53 P.M.	1860, August 12. 10.10 to 10.21 A.M.	10.22 to 10.56 A.M.	1860, Sept. 7. 11.22 to 11.23 A.M.
19.	Minster to Deal	.....	.....	.....	.....	Strong.				
17.	Margate to Ramsgate	Hard over.	Middling.	Middling.	Middling.	Strong.	71°.	(?)	Very strong.	54°.
N.	Whitstable to Canterbury	.....	.....	.....	.....	Very strong.	Strong.	Very strong.	Very strong.	Strong.
5.	Ashford to Hastings	Strong.	.....	.....	Slight.	.....	.....	Strong.	Strong.	Strong.
13.	Margate to Ashford	Hard over.	Very strong.	Very strong.	Very strong.	64°	82°	45°	74°	50°
15.	Ramsgate to Ashford	Hard over.	Middling.	Middling.	Strong.	Strong.	Hard over.	Very strong.	Very strong.	Very strong.
32.	Ashford to Tonbridge	.....	.....	.....	Very strong.	.....	Slight.	Strong.	Strong.	Strong.
28.	Folkestone to Ashford	Slight.	.....	.....	Middling.	.....	Slight.	Strong.	Strong.	.....
26.	Dover to London	Hard over.	Slight.	Slight.	Very strong.	Strong.	Middling.	Strong.	Strong.	Strong.

2. Range 42°; extending from 46° W. of Magnetic S. to 88° W. of Magnetic S. (Fig. 4, Plate III.)

		1859, December 13. 1.39 to 1.50 P.M.	6.50 to 7.30 P.M.	1860, July 1. 8.10 to 9.13 P.M.	9.17 to 10.3 P.M.	1860, August 8. 4.38 to 5.13 P.M.	5.25 to 5.44 P.M.	1860, August 12. 9.30 to 9.52 A.M.	10.2 to 10.10 A.M.	1860, Sept. 7. 7.10 to 7.18 A.M.
20.	Deal to Minster	.....	.....	.....	.....	.....	Slight.	.....	.....	.....
18.	Ramsgate to Margate	Slight.	Middling.	Slight.	Slight.	63°	80°	Very strong.	Very strong.	Slight.
8.	Canterbury to Whitstable	.....	.....	.....	.....	.....	.....	Very strong.	Very strong.	
4.	Brighton to London	.....	Strong.							
6.	Hastings to Ashford	Middling.	Middling.	Slight.	Slight.	Slight.	Strong.	Strong.	Strong.	Slight.
14.	Ashford to Margate	Strong.	Very strong.	Strong.	Strong.	60°	73°	68°	74°	44°
16.	Ashford to Ramsgate	Middling.	Very strong.	Strong.	Strong.	Hard over.	Very strong.	Very strong.	Very strong.	Very strong.
31.	Tonbridge to Ashford	.....	Very slight.	Middling.	Middling.	.....	Slight.	Strong.	Strong.	
27.	Ashford to Folkestone	.....	.....	Slight.	Slight.	Slight.	Slight.	Strong.	Strong.	Slight.
25.	London to Dover	Slight.	Strong.	Strong.	Strong.	Very strong.	Very strong.	Very strong.	Strong.	Very strong.

3. Range 31°; extending from 46° E. of Magnetic N. to 77° E. of Magnetic N. (Fig. 5, Plate III.)

		1858, April 9. 3.40 to 3.58 P.M.	1859, September 2. 7.10 to 7.43 A.M.	7.56 to 8.0 A.M.	12.15 to 12.30 P.M.
23.	London to Tonbridge	Strong.	Very strong.	Strong.	Slight.
21.	Tonbridge to Hastings	.....	(?)		
17.	Margate to Ramsgate	Strong.	Very strong.	Hard over.	
N.	Whitstable to Canterbury	.....			
13.	Margate to Ashford	Very strong.	Very strong.	(?)	Very strong.
15.	Ramsgate to Ashford	Very strong.	Very strong.		Strong.
32.	Ashford to Tonbridge	.....	(?)	Strong.	(?)
28.	Folkestone to Ashford	Strong.			
26.	Dover to London	Strong.	Very strong.	Strong.	Slight.

4. Range 31°; extending from 46° W. of Magnetic S. to 77° W. of Magnetic S. (Fig. 5, Plate III.)

		1859, September 2. 7.43 to 7.48 A.M.	8.12 to 8.17 A.M.	12.4 to 12.15 P.M.	1860, March 28. 10.11 to 10.13 A.M.
24.	Tonbridge to London	Strong.	Slight.	Strong.	Very slight.
18.	Ramsgate to Margate	.....	(?)	.....	Strong.
6.	Hastings to Ashford	.....	.....	.....	Slight.
14.	Ashford to Margate	Strong.	Hard over.	Very strong.	Very strong.
16.	Ashford to Ramsgate	Strong.	Hard over.	Very strong.	Very strong.
31.	Tonbridge to Ashford	(?)	(?)	(?)	Slight.
27.	Ashford to Folkestone	.....	.....	.....	Slight.
25.	London to Dover	Strong.	Slight.	Strong.	Strong.

VIII. *On the Surface-condensation of Steam.* By J. P. Joule, *LL.D., F.R.S., President of the Literary and Philosophical Society of Manchester, &c.*

Received October 10,—Read December 13, 1860.

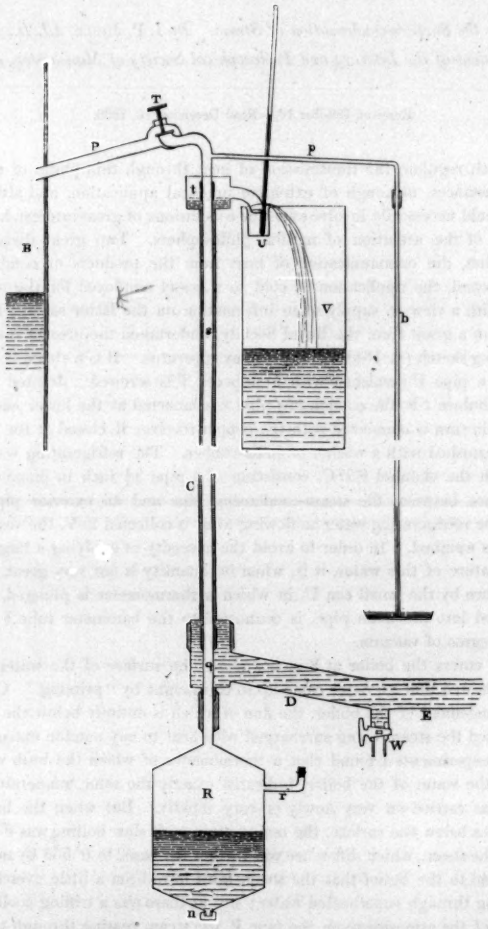
The laws which regulate the transmission of heat through thin plates of metal under various circumstances, although of extensive practical application, and although their elucidation would necessarily involve scientific conclusions of great interest, have hitherto received little of the attention of natural philosophers. Two great divisions of the inquiry are, first, the communication of heat from the products of combustion to a boiler; and second, the application of cold to a vessel employed for the condensation of steam. With a view to supply some information on the latter subject I have, with the assistance of a grant from the Royal Society, undertaken the present research.

The adjoining sketch (p. 134) will explain my apparatus. B is a steam-boiler into the side of which a pipe P furnished with a stopcock T is screwed. Jointed to this by a caoutchouc tubulure *t* is the condensing pipe *s*, connected at the lower end to a short pipe *q*, which in turn is connected with the copper receiver R, closed at the bottom by a screw-nut *n* furnished with a washer of india-rubber. The refrigerating water is transmitted through the channel E D C, consisting of a pipe $1\frac{1}{4}$ inch in diameter, and the concentric space between the steam-condensing pipe and an exterior pipe of larger diameter. The refrigerating water on flowing away is collected in V, the vessel in which it is afterwards weighed. In order to avoid the necessity of applying a large correction to the temperature of this water, it is, when its quantity is not very great, received in the first instance by the small can U, in which a thermometer is plunged. A branch pipe *p*, screwed into the main pipe, is connected to the barometer tube *b* in order to measure the degree of vacuum.

The pipe P enters the boiler at 8 inches above the surface of the water. Separate experiments showed that no water came up to this height by "priming." On the other hand, the arrangement of the boiler, the flue of which is entirely below the level of the water, prevented the steam being surcharged with heat to any notable extent.

By careful experiments I found that a thermometer of which the bulb was held six inches above the water of the boiler, indicated exactly the same temperature whether the boiling was carried on very slowly or very rapidly. But when the bulb was immersed 3 inches below the surface, the temperature with slow boiling was 0°·532 higher than that of the steam, which difference was further increased to 0°·538 by rapid boiling. This would lead to the belief that the steam must have been a little overcharged with heat by passing through superheated water; but as there was a trifling cooling effect by the influence of the atmosphere on the pipe P, the steam passing through the stopcock might be safely considered as neither superheated nor mixed with water.

Up to the stopcock T the temperature of the pipe may be considered as that of the



boiler; beyond it the temperature becomes gradually that of the condenser. A certain,

though very small, quantity of heat is thus conducted along the tube from the stopcock T as far as the india-rubber junction *t*. Any water condensed in P falls back again into the boiler; that between the stopcock and *t* falls into the receiver; so that the small quantity of conducted heat just mentioned is probably compensated by the trifling cooling effect of the atmosphere between T and the refrigerating water.

The short continuation pipe *q* exposes to the water an effective length of 3 inches, which, on account of the wideness of the channel there, could not generally have had an effect greater than that due to 2 inches in the narrower part. As, however, a length amounting to an inch and a half of the ends of the condensing tube is overlapped by the vulcanized tubing, the entire amount of condensation may, without appreciable error, be laid to the account of the condensing pipe.

The receiver R and the pipes C, P, and *p* are enveloped by a thick coating of cotton-wool and flannel, so as to prevent as far as possible the refrigerating effect of the atmosphere.

Great pains were taken to make every part of the apparatus in which the pressure is below that of the atmosphere, perfectly air-tight. It will be seen that the form of the stopcock T effectually prevents any leakage except from the high pressure side into the atmosphere, which is of no consequence. The india-rubber junctions were at first made by simply binding on the ends of the tubes short lengths of vulcanized caoutchouc; but it was soon found that enough air passed to vitiate the experiments, which were consequently rejected. The method afterwards adopted was to smear the ends of the tubes with melted vulcanized caoutchouc before the short india-rubber tubes were bound on. This plan was found to be so efficacious that air appeared to be perfectly excluded, and the vacuum wholly unimpaired, however long an experiment was carried on.

The vacuum-gauge glass tube is 0·45 of an inch in internal diameter. It is plunged into a wide dish of mercury, from the surface of which the height of the column is measured. The temperature of the mercury in the gauge was always nearly that of the barometer which registered the atmospheric pressure. During each experiment a small quantity of condensed water settled by degrees on the top of the mercury, the length of which, divided by 13·56, gave the correction to be supplied to the height of the column.

It will be observed that the pipe leading to the vacuum-gauge is inserted near the stopcock which admits the steam. It was important to ascertain whether the gauge would stand at the same level if it were connected with other parts of the vacuous space. To determine this, a pipe was attached to the receiver at *r*, and connected with a gauge placed side by side with the first gauge, and dipping into the same dish of mercury. The gauges were observed during rapid and slow condensation, at different and at varying pressures; but the height of the columns appeared to be in general exactly the same: if any difference could be observed at any time, I would say that the receiver gauge indicated the less perfect vacuum of the two; the difference, however, amounted in no case to more than $\frac{1}{30}$th of an inch.

The following is my method of experimenting. The nut *n* being unscrewed, the dish of mercury removed from under the gauge-tube, and the water being completely discharged from the tap W, the cock T is partly opened, and the steam is blown through the steam-pipe *a*, the gauge *b*, and the receiver R until they are completely freed from air. The nut *n* is then screwed on, W closed, and the water let on, the three operations being performed as simultaneously as possible. At the moment when the steam is about to cease issuing from the gauge-pipe, its end is introduced into the dish of mercury. After an interval of time, varying from half a minute to three minutes, the condensation goes on with perfect regularity, and the mercury in the vacuum-gauge remains steady. The temperature of the water flowing away and the gauge are observed every two or three minutes. The experiment is terminated by simultaneously shutting off the steam and the water, and opening the tap W to let off the water remaining in the pipe. The nut *n* is then removed, and a quantity of air having entered the receiver, the condensed water is caught by a small can (held close and containing a thermometer), which overflows into a larger vessel in which the water is immediately afterwards weighed.

The values of several small corrections which had to be applied to the observations were obtained from data derived from separate experiments. Of the thermometers employed, one was made by Fastré, in which each division is equal to 0°·225; the two others were from Kew Observatory, and have for each division the values 0°·1 and 0°·0994 respectively. A correction had generally to be applied in consequence of the non-immersion of the stems.

The cooling effect of the atmosphere on the receiver R operates partly to condense steam and partly to cool condensed water. The correction on the former account was found to be equal to the continual product of the time in minutes, the proportion of acting surface, and the difference between the temperatures of the receiver and atmosphere, divided by 77 times the difference between 640 and the temperature of the condensed water: the result had to be subtracted from the weight of condensed water. The correction on the latter account is equal to the continual product of the time, acting surface, and difference of temperature, divided by 77 times the weight of condensed water; it had to be added to the observed temperature of the condensed water.

The correction on account of the cooling of the refrigerating water on flowing through C into the vessel U, was found to be equal to the difference of temperature between the water and the atmosphere, multiplied by 0·51, and divided by the quantity of water flowing per hour. This rule applies to the case in which the external pipe C was 4 feet long and 1 inch in diameter. Corrections in the instances in which other tubes were used were made by calculation without express experiments, inasmuch as they were of very trifling amount.

The slight loss of water by evaporation, before and during the process of weighing, was allowed for in the weighing both of the refrigerating and condensed water.

The metal of the steam-pipe and receiver is necessarily at 100° at the commencement of an experiment, and therefore communicates some heat during the first few moments.

On the other hand, the small quantity of water drawn off at W at the termination of an experiment is always more or less heated. Corrections on both these accounts were easily applied.

I had at first some doubts whether the vacuum would not become gradually impaired by air coming over from the boiler; for it has been frequently asserted that water becomes perfectly free from air only after long-continued boiling. I found, however, that after boiling had taken place for only two or three minutes, the air was entirely expelled, and that even if condensation were afterwards carried on until the receiver was entirely filled with water, no change took place in the height of the gauge. Hence, by blowing off steam for ten minutes at the commencement of a day's experimenting, I effectually secured myself against any risk of the interference of air*.

The Table of experiments requires little explanation. It will be seen that column 5 contains some numbers with the negative sign. This might be expected where a small quantity of water was used, on account of its being raised in temperature during its ascent. When the water was intended to go in the same direction as the steam, it was poured in at the upper end of the outer tube, and flowed away at the lower end, the pipe E being removed. Each number in the 14th column is the average of all the observations of the pressure in the condenser after it became constant; and column 17 contains the averages of all the observations of the temperature of the refrigerating water at its overflow made at the moments of gauge-observation. Hence this column contains numbers generally a little different from those of column 7, which, being taken for the purpose of deducing the total heat of steam, are the averages of all the temperature observations of the overflow water in the several experiments.

In order to explain the principle on which the 18th column is based, I cannot do better than give textually the extract of a letter I received from Professor THOMSON, to whom at the outset I communicated my design, and who, with his usual zeal and kindness, immediately offered me very valuable suggestions.

"Steamer Venus, August 10th, 1859.

"If the resistance to equalization of temperature between the steam and water depended on *conduction* through the separating metal alone, the heating effect would take place according to the law you name. The formula would be thus found,

$$wdv = -k\frac{A dx}{a}v,$$

where w is the mass of water passing per unit of time, dv the augmentation of the

* I could not discover any alteration in the composition of the air after it had remained in the boiler some days. There appears to be no truth in the hypothesis which ascribes boiler explosions to the formation of hydrogen. The obvious cause is over-pressure; and it is not wonderful that, when multitudes of boilers are worked at a very considerable proportion of the pressure calculated to burst them when new, accidents occasionally occur. I have repeatedly insisted upon the absolute necessity of periodical testing, and have proposed a method requiring no extra apparatus or expense, which consists simply in lighting a fire under the boiler when completely filled, and so producing the proof pressure by the expansion of water by heat. I try my boiler every six weeks by this process, which appears to answer the end in view in every respect.

difference of temperatures inside and outside in a length from x to $x+dx$, v the difference itself at any point P, k the conducting power of the metal, A the area of the tube per unit length, a its thickness. By integrating, we find

$$\log\frac{V}{v}=\frac{kAx}{aw},$$

where V denotes the difference of temperatures at the entrance end. A will be the area corresponding to a mean diameter calculated by the formula $\frac{2a}{\log\frac{D}{D-2a}}$, when the outer diameter D, and the inner $D-2a$ differ so much that it will not do to use one or the other indifferently. For all practical purposes, with such tubes as are actually used, it will do to take as the mean diameter the arithmetic mean $D-a$.

"The truth, however, is that, except with a very great velocity of the water, there will be a heated film close to the metal much higher in temperature than the average temperature of the water in the same section, and the abstraction of heat will be much slower than according to the preceding formula. It is not improbable, however, that some law of variation will still hold from point to point in the direction of flow; and if so, the same formula would apply, only that for k something much smaller than the true conductivity of the metal must be substituted. Thus, supposing k to be a function of w, smaller the smaller is w and increasing to a limit (the true conductivity of the metal), your experiments might give values of k for different rates of the flow of the water by the expression

$$k=\frac{aw}{Ax}\log\frac{V}{v}.$$

It would be necessary to ascertain by experiment how nearly the geometrical law of decrease of the difference of temperatures along the tube holds, as there is no sufficient theory for convection to give any decided indication.

"As the results would probably depend but little on the thickness and quality of the metal, it would be better perhaps to take $\frac{k}{a}$ as the thing to be determined: calling it C, we have

$$C=\frac{w}{Ax}\log\frac{V}{v},\quad\text{or}\quad v=Ve^{\frac{-CAx}{w}}.$$

e being the base of the nap. log, $e^{\frac{-CA}{w}}$ is the fraction expressing the reduction of the difference per unit of length, and therefore $\left(1-\frac{CA}{w}\right)100$ is the per-centage of difference lost per unit of length. If this be called θ, we have

$$v=V(1-\theta)^x,\quad\text{or}\quad\log\frac{1}{1-\theta}=\frac{1}{x}\log\frac{V}{v},$$

where log denotes any kind of log. These are, in fact, the compound interest formulæ, and are perhaps the most convenient for numerical reductions."

The results of my experiments were quite in conformity with Professor THOMSON's view as to the smallness of the resistance to conduction through the thickness of the metal compared with the resistance at the surfaces of the tubes through the closely adhering film of fluid. I therefore sought to discover in each instance the entire conductivity by the formula

$$C=\frac{w}{a}\log\frac{V}{v},$$

where, a being the area of the tube in square feet, and w the quantity of refrigerating water transmitted per hour, C represents the number of units of heat, in lbs. of water raised 1°, which would be conducted through a surface of 1 foot area, the opposite sides of which differ from one another by 1°. The determinations of C in each instance will be found in column 18.

I generally obtained observations of the vacuum-gauge directly after the stoppage of the condensation. The results of these, reduced to the value they would have had at the precise time of the closing of the stopcock, are given in column 15 of the Table. The effect of stopping the condensation was generally a diminution of pressure, which took place rapidly at first, and afterwards slowly and with great regularity. I believe that this diminution of pressure is owing to the water collected in the receiver, which, having fallen somewhat in temperature during an experiment, governs the vacuum as soon as the fresh hot condensed water ceases to be supplied to its surface. In some few instances the mercury in the gauge was observed to *fall* immediately on the stoppage of the condensation. In these the vacuum appeared to be more perfect while the condensation was being carried on than was due to the temperature of the condensed water. It was long before I was able to form any conjecture as to the cause of this anomalous circumstance. I now think that it might have been occasioned by a stricture in the india-rubber junction which connected the gauge with the steam-tube p. It is not, however, easy to see how this can account for the sudden fall of the gauge at the moment of the stoppage of the condensation. In the Table, I have marked those results which I suspect to have been influenced by a contraction at the junction, by a note of interrogation. I may observe that the india-rubber tubulures were frequently renewed, in order to prevent the chance of a stricture, which, moreover, I always endeavoured to detect at its first approach, by observing whether the mercury descended instantaneously on the admission of the first bubble of air into the receiver when the nut was unscrewed.

Great care was always taken to keep the flow of steam and refrigerating water as constant as possible during each experiment. If this had not been done, the temperature of the water collected in the receiver during the former part of an experiment would have influenced to a certain extent the vacuum observed at the latter part. It was easy, by first condensing rapidly, and afterwards slowly by partially closing the steam-cock, to maintain for some time a vacuum much more perfect than that due to the temperature of the water in the receiver. In this case "bumping boiling" took place in the receiver, whilst the pressure gradually decreased to the value due to the new conditions.

TABLE I.

1. Description.		2. No.	3. Duration of experiment, in minutes.	4. Total pressure of steam in the boiler, in inches of mercury.	5. Head of refrigerating water above its overflow, in inches.	6. Mean temperature of refrigerating water. At its entrance (t).	7. Mean temperature of refrigerating water. At its exit.
Copper steam-tube, s, 4 feet long, exterior diameter ·75 inch, interior ·63 inch, mean area $s = ·7225$ sq. ft. Outer tube C 1·4 inch in diameter. Refrigerating water moving in a direction contrary to that of the steam. In the experiments 10–16 the receiver was in communication with the atmosphere.		1	60	48·2	0·2	5·18	20·21
		2	60	41·88	— 0·1	5·18	40·38
		3	30	46·23	1·13	5·15	19·21
		4	30	48·36	1·2	4·96	17·63
		5	45	91·47	0·47	4·78	16·13
		6	37	120·14	0·66	4·81	19·19
		7	50	114·27	0·51	4·93	15·23
		8	60	39·29	0·47	4·67	13·62
		9	52	35·68	0·54	4·7	14·17
		10	60	51·98	0·12	4·94	11·58
		11	30	45·22	0·12	5·17	31·6
		12	30	48·02	0·48	5·12	22·02
		13	20	50·31	0·97	5·39	22·21
		14	30	54·4	— 0·1	5·37	48·35
		15	20	45·6	1·35	5·12	26·1
		16	18½	50·9	1·04	5·37	29·62
The same copper steam-tube. The outer tube ·87 inch in interior diameter. Experiments No. 30, 31, 32, and 33 were made when the steam-tube had been recently cleaned by dilute sulphuric acid.	Refrigerating water moving in a direction contrary to that of the steam.	17	60	44·91	4·37	6·57	16·5
		18	60	48·8	— 1·4	6·22	81·08
		19	120	47·77	— 0·13	6·62	50·51
		20	120	45·27	0·6	6·86	25·61
		21	50	48·71	— 0·49	6·36	88·08
		22	60	48·61	1·19	6·42	52·07
		23	44	45·25	6·45	6·24	27·32
		24	19	47·1	14·07	6·04	34·48
		25	555	40·78	0·08	6·2	16·7
		26	26½	49·86	14·73	5·36	26·94
		27	27	46·68	21·45	5·22	22·67
		28	20	51·5	23·29	5·22	26·75
		29	435	46·27	— 0·63	5·22	53·73
		30	24	53·13	12·9	8·5	32·135
		31	18½	52·09	20·12	8·4	30·014
		32	30	52·09	14·15	8·46	13·518
		33	30	53·81	11·48	8·44	20·66
	Refrigerating water moving in the same direction as the steam.	34	30	48·99	48	8·655	22·55
		35	15	46·51	48	8·62	22·955
		36	12	46·51	48	8·62	27·78
		37	11¼	49·07	48	8·63	29·739
		38	10	48·09	48	8·64	29·817
The same copper steam-tube. The outer tube ·98 inch in internal diameter. Experiments 53–61 inclusive, were made when the steam-tube had been recently cleaned with dilute sulphuric acid.	Refrigerating water moving in a direction contrary to that of the steam.	39	60	43·81	12·8	6·73	38·69
		40	60	41·32	— 0·1	7·27	89·146
		41	60	42·77	37·16	7·13	22·852
		42	57	43·05	35·61	6·87	47·62
		43	60	45·78	18·33	6·95	56·784
		44	120	44·75	0·5	6·95	54·01
		45	60	45·33	9·92	6·61	35·29
		46	60	45·06	28·58	6·67	18·353
		47	60	47·16	210·2	6·51	14·317
		48	23	50·72	232	6·47	36·732
		49	17	48·52	206·3	6·84	45·282
		50	15	51·97	211·7	6·82	47·496
		51	14	51·67	237·2	6·82	47·371
		52	30	53·76	292·06	6·82	24·312
		53	60	62·4	28·74	7·09	25·343
		54	30	55·06	28·6	6·7	41·49

TABLE I.

8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.
Weight of refrigerating water, in pounds.		Weight of condensed water, in pounds.		Temperature of the condensed water.	Total heat of steam.	Barometer, minus vacuum-gauge, or pressure in the condenser, in inches of mercury.	Pressure in the condenser immediately after the conclusion of the experiment.	Temperature due to the pressure in the condenser (col. 14) per Regnault (t_2).	Temperature of the refrigerating water at its exit, at the times the vacuum was observed (t_1).	Conduction of heat, per square foot of the surface of the steam-pipe, or $\frac{w}{a}\log\left(\frac{t_2-t}{t_2-t_1}\right)$.	No.
In the experiment.	Per hour (w).	In the experiment.	Per hour.								
97·64	97·64	2·374	2·374	33°·08	651·41	1·56	1·49	34°·03	20°·074	98·13	1
98·73	98·73	5·818	5·818	55·27	652·6	5·0	4·66	56·63	40·493	158·46	2
379·45	758·9	9·467	18·934	78·87	640·83	17·992	12·53	86·37	18·956	195·68	3
399·08	798·16	8·914	17·828	75·24	640·48	15·32	11·65	82·3	17·29	191·86	4
443·26	591·01	8·410	11·213	61·58	656·49	9·094	6·074	69·79	15·82	152·24	5
389·64	631·85	9·556	15·496	69·95	654·04	13·332	.....	78·87	18·882	184·3	6
495·39	594·47	8·611	10·332	58·94	644·58	7·865	6·57	66·48	15·152	149·44	7
597·01	597·01	9·128	9·128	56·65	638·63	6·346	4·373	61·73	13·492	138·79	8
570·26	657·99	9·284	10·712	59·12	637·72	7·342	5·4	64·95	13·94	151·6	9
430·64	430·64	4·418	4·418	12·68	645·15	29·38	.....	99·49	11·42	.....	10
218·34	436·68	9·321	18·642	25·62	638·76	29·618	.....	99·72	31·165	.....	11
309·42	618·84	8·200	16·399	20·78	651·19	29·618	.....	99·72	21·72	.....	12
261·26	783·78	7·027	21·081	29·08	647·86	29·915	.....	100	21·924	.....	13
33·95	67·9	2·18	4·36	23·9	693·27	29·9	.....	99·98	47·8	.....	14
283·3	849·9	10·6	31·8	97·0	657·72	29·68	.....	99·77	26·1	.....	15
227·4	737·5	9·8	31·78	79·0	641·7	29·92	.....	100	30·66	.....	16
189·47	189·47	2·985	2·985	22·75	652·85	0·96	.....	25·58	16·5	193·77	17
20·41	20·41	2·983	2·983	84·31	596·4	18·326	.....	86·85	83·47	89·62	18
87·14	43·57	6·54	3·27	54·36	639·16	4·965	4·965	56·48	51·9	143·92	19
122·32	61·16	3·66	1·83	25·71	652·48	1·185	1·185	29·18	25·64	155·91	20
63·72	76·5	9·55	11·46	87·25	632·77	25·126	18·806	95·19	88·87	279·85	21
124·5	124·5	9·838	9·838	67·39	645·09	10·835	10·505	73·88	52·56	198·48	22
251·6	343·1	9·938	13·552	60·6	594·28	8·2	7·95	67·42	27·46	202·28	23
194·06	612·82	9·671	30·54	84·43	654·33	23·22	15·0	93·06	34·48	335·65	24
167·16	18·07	2·652	0·287	9·18	671·01	0·77	.....	21·91	17·03	29·26	25
261·6	592·3	9·771	22·123	68·77	644·99	11·76	7·83	75·83	26·8	297·38	26
332·91	739·8	9·77	21·711	59·24	651·8	7·46	5·414	65·3	22·3	342·5	27
255·72	767·16	9·679	29·037	73·61	641·1	13·45	9·4	79·08	26·14	353·6	28
118·66	16·37	9·966	1·375	38·92	616·85	5·03	.....	56·75	55·3	80·9	29
242·53	606·33	9·89	24·725	71·663	649·82	13·784	8·124	79·68	31·805	332·85	30
245·85	797·34	9·145	29·66	69·825	649·23	11·92	9·59	76·15	29·376	408·89	31
345·85	691·69	2·665	5·33	27·08	669·8	1·145	1·145	28·59	13·188	256·3	32
305·85	611·69	6·059	12·119	46·782	659·2	3·271	3·26	47·97	20·248	300·41	33
328·62	657·25	7·403	14·806	39·636	652·34	2·161	.....	40	22·343	522·1	34
311·0	1244	7·710	30·84	60·411	636·08	7·876	6·67	66·51	22·371	466·96	35
258·44	1292·18	8·853	44·264	79·88	638·08	19·012	15·81	87·8	27·065	474·32	36
249·56	1302·06	9·576	49·962	87·677	637·15	25·624	.....	95·72	29·42	491·56	37
216·0	1296	8·475	50·85	89·732	628·85	27·074	21·67	97·23	29·43	479·77	38
57·53	57·53	3·023	3·023	39·54	647·76	2·186	1·986	40·21	38·858	255·6	39
21·4	21·4	3·629	3·629	89·45	572·29	24·49	22·49	94·5	89·476	84·55	40
133·45	133·45	3·399	3·399	25·07	642·26	1·037	0·976	26·89	23·068	303·43	41
147·29	155·04	9·992	10·518	49·51	650·19	6·064	.....	60·74	47·62	303·08	42
99·45	99·45	8·737	8·737	61·96	629·15	6·934	5·33	63·67	56·744	289·44	43
22·432	11·216	1·957	0·978	55·92	595·34	4·762	4·21	55·6	54·01	53·1	44
45·276	45·276	2·151	2·151	33·97	637·64	1·662	1·55	35·16	34·688	257·1	45
91·197	91·197	1·742	1·742	17·58	629·21	0·698	0·7	20·31	18·258	239·09	46
455·01	455·01	5·559	5·559	15·625	645·23	1·589	0·964	34·36	14·041	198·52	47
200·531	523·12	10·191	26·585	61·80	654·95	8·97	6·12	69·47	36·703	473·3	48
144·807	511·08	9·66	34·094	74·563	649·19	15·47	10·32	82·55	44·731	490·99	49
133·31	533·23	9·594	38·376	80·073	643·97	20·54	13·82	89·81	47·563	498·64	50
133·588	572·52	9·551	40·933	82·86	648·92	21·315	.....	90·79	46·82	512·66	51
310·978	621·95	8·811	17·622	41·145	654·37	4·864	2·51	56·05	24·038	370·49	52
103·32	103·32	3·073	3·073	20·507	634·23	1·092	1·09	27·78	25·343	305·89	53
58·712	117·42	3·4	6·8	43·703	644·5	2·946	2·95	45·91	41·49	354·74	54

TABLE I. (continued).

1. Description.	2. No.	3. Duration of experiment, in minutes.	4. Total pressure of steam in the boiler, in inches of mercury.	5. Head of refrigerating water above its overflow, in inches.	6. Mean temperature of refrigerating water. At its entrance (t).	7. Mean temperature of refrigerating water. At its exit.
Experiments 62, 63, and 64 were made with the steam-tube greasy by rubbing it with oil. Refrigerating water moving in a direction contrary to that of the steam.	55	30	53·47	26·66	6°·88	63°·893
	56	15	49·16	34·66	6·88	84·222
	57	30	51·97	231·4	6·82	13·54
	58	30	57·88	211·3	6·82	20·574
	59	20	58·82	233	6·82	31·987
	60	13½	60·36	235·9	6·82	48·266
	61	10	58·41	223·5	6·82	51·442
	62	20	50·03	211	6·075	16·563
	63	15	49·62	193·5	6·075	34·808
	64	10	47·65	216	6·075	51·417
The same tubes. The steam-tube fresh cleaned. Refrigerating water going in the same direction as the steam.	65	60	50·89	48	6·97	26·808
	66	30	51·84	48	6·97	47·73
	67	15	49·49	48	6·97	71·317
Lead steam-tube 4 feet long, exterior diameter 0·77 inch, interior 0·52 inch, mean area $a = 0{\cdot}6503$ sq. ft. The outer tube 0·87 inch internal diameter. Refrigerating water moving in a direction contrary to that of the steam. In experiments 81, 82, and 83 the receiver was in communication with the atmosphere.	68	30	43·24	1·57	9·4	67·44
	69	30	41·73	4·96	9·14	60·235
	70	20	42·18	13·35	9·14	41·71
	71	30	36·5	1·47	7·34	70·65
	72	30	37·08	13·3	7·3	28·73
	73	20	36·7	25·4	7·3	30·75
	74	30	35·17	1·24	7·28	79·66
	75	30	38·08	26·5	7·24	10·305
	76	30	44·12	29	6·44	11·1
	77	20	42·34	30	6·26	26·1
	78	27½	38·29	7·4	6·2	57·67
	79	30	37·1	0·63	6·22	89·19
	80	60	36·7	1·0	5·0	60·7
	81	30	35·77	13·4	5·8	15·14
	82	30	34·8	18·7	5·32	13·46
	83	30	35·04	1·3	5·0	90·68
Iron steam-tube 4 feet long, exterior diameter 0·74 inch, interior diameter 0·602 inch, mean area $a = 0{\cdot}7026$ sq. ft. Outer tube 0·87 inch internal diameter. Refrigerating water moving contrary to the direction of the steam.	84	15	43·5	14·8	13·54	38·85
	85	20	45·5	11·0	13·54	42·3
Copper steam-tube 4 feet long, area ·7225 sq. ft. Outer tube 0·87 inch interior diameter. A taper glass rod was placed in the axis of the steam-tube; its length was 40 inches, diameter at thick end ·55 inch, at thin end ·3 inch. — The tapered glass rod with its thin end uppermost. Refrigerating water moving in the same direction as the steam.	86	15	52·71	48	8·52	28·313
	87	15	57·39	48	8·52	22·157
	88	9½	58·91	48	8·47	34·233
	89	10	56·43	48	8·42	28·909
— The tapered glass rod with its thin end uppermost. Refrigerating water moving in a direction contrary to that of the steam.	90	20	48·6	10·8	8·08	21·82
	91	15	52·85	30·25	8·02	17·96
	92	15	52·15	27·66	8·02	27·832
	93	10	52·61	32·5	7·92	32·999
— The tapered glass rod with its thick end uppermost. Refrigerating water moving in a direction contrary to that of the steam.	94	15	48·25	32·0	7·67	19·308
	95	15	49·12	25·66	7·67	13·325
	96	11	48·6	25·0	7·54	37·22
Copper steam-tube 4 ft. long, area ·7225 sq. ft. A spiral consisting of 30 turns of copper wire $\frac{1}{16}$th of an inch diameter was wound round it. Half of the spiral was right-handed, the other half left-handed. Outer tube 1·4 inch diameter. Refrigerating water moving in a direction contrary to that of the steam.	97	30	53·86	1·5	16·875	29·501
	98	20	60·27	1·0	16·65	40·625
	99	15	58·64	3·0	16·425	43·99
Copper steam-tube 4 feet long, area ·7225 sq. ft. Outer tube 1·4 inch diameter. Spiral of 45 turns of copper wire ·21 inch thick between the tubes. Refrigerating water moving contrary to the direction of the steam.	100	30	47·42	1·95	15·547	25·0
	101	30	51·8	2·06	15·66	32·456
	102	20	59·45	1·43	15·547	44·634
	103	14	55·96	4·7	15·48	45·034

TABLE I. (continued).

8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.
Weight of refrigerating water, in pounds.		Weight of condensed water, in pounds.		Temperature of the condensed water.	Total heat of steam.	Barometer, minus vacuum-gauge, or pressure in the condenser, in inches of mercury.	Pressure in the condenser immediately after the conclusion of the experiment.	Temperature due to the pressure in the condenser (col. 14) per Regnault (t_2).	Temperature of the refrigerating water at its exit, at the times the vacuum was observed (t_1).	Conduction of heat, per square foot of the surface of the steam-pipe, or $\frac{w}{a}\log\left(\frac{t_2-t}{t_2-t_1}\right)$.	No.
In the experiment.	Per hour (w).	In the experiment.	Per hour.								
64·603	129·21	6·598	13·195	67°·608	625·86	9·108	9·11	69°·83	63°·53	411·66	55
44·447	177·79	6·164	24·656	89·197	646·9	24·947	21·95	95	84·51	523·85	56
252·385	504·77	2·732	5·464	17·437	619·46	0·831	0·831	23·17	13·035	334·13	57
248·947	497·9	5·567	11·135	31·841	639·27	1·622	1·592	34·73	20·136	446·82	58
181·822	545·47	7·623	22·87	50·362	646·56	3·975	3·57	51·88	21·233	589·21	59
132·56	589·14	9·687	43·055	81·526	647·47	19·83	14·83	88·89	47·807	564·26	60
97·79	586·75	7·828	46·969	86·612	642·98	25·01	.....	95·06	50·857	561·39	61
158·2	474·59	2·466	7·398	25·952	680·13	1·115	1·065	28·13	15·8	381·96	62
123·29	493·16	5·948	23·793	58·839	650·11	7·288	5·09	64·78	34·16	444·27	63
90·42	542·5	7·458	44·747	87·877	636·57	27·27	19·97	97·42	50·112	494·05	64
150·75	150·75	5·014	5·014	28·287	624·68	1·817	1·81	36·78	26·808	228·48	65
92·44	184·88	6·524	13·047	56·1	633·66	5·448	4·848	58·44	48·162	412·24	66
53·187	212·75	6·451	25·805	85·906	616·42	22·77	19·37	92·54	71·317	410·55	67
14·785	29·57	1·608	3·216	65·113	598·77	10·06	.....	72·13	67·185	115·51	68
77·582	155·16	6·691	13·382	78·951	671·41	18·22	.....	86·7	60·235	256·55	69
139·27	417·82	7·394	22·183	81·184	694·63	21·53	.....	91·05	42·08	330·53	70
16·883	33·77	1·943	3·886	74·253	624·33	11·45	9·45	75·19	71·09	145·75	71
197·03	394·07	7·271	14·542	66·22	644·6	9·28	9·28	70·25	28·73	252·19	72
218·47	655·42	9·249	27·748	81·92	634·83	21·07	17·1	90·48	30·89	336·18	73
16·109	32·218	2·268	4·535	84·59	598·78	16·56	14·26	84·26	79·53	138·2	74
312·03	624·07	1·323	2·647	18·526	710·4	1·18	.....	29·11	9·955	127·19	75
328·03	656·07	2·133	4·266	15·613	712·49	1·145	1·425	28·59	10·65	212·67	76
264·66	793·98	9·063	27·19	78·337	656·49	15·73	.....	82·96	26·1	365·43	77
101·35	221·12	9·608	20·96	88·926	631·81	27·73	18·7	97·89	57·67	280·2	78
15·472	30·94	2·635	5·27	88·67	575·78	25·27	18·07	95·34	89·19	127·19	79
18·16	18·16	1·743	1·743	60·687	640·94	6·87	.....	63·46	60·7	85·27	80
162·72	325·44	2·366	4·73	20·088	662·31	29·79	.....	99·88	15·14	.....	81
237·66	475·32	2·991	5·982	18·907	652·18	29·82	.....	99·9	13·46	.....	82
11·472	22·944	1·642	3·284	35·552	634·12	30·02	.....	100·1	90·68	.....	83
139	556	6·608	26·432	94·07	626·47	28·52	23·02	98·66	38·85	279·24	84
152·2	456·6	7·917	23·751	89·09	641·98	29·54	22·64	99·64	42·3	264·16	85
137·812	551·25	4·628	18·512	50·701	634·76	4·329	3·93	53·63	28·131	435·25	86
323·68	1294·72	7·804	31·218	54·454	617·12	5·979	5·31	60·44	22·037	540·42	87
204·25	1290	9·718	61·379	88·654	629·53	26·244	.....	96·37	33·955	611·33	88
222·94	1337·64	8·023	48·136	75·579	643·42	15·601	13·1	82·75	28·441	581·02	89
195·1	585·3	4·408	13·224	30·844	631·12	4·354	3·4	53·74	21·346	278·07	90
246·41	985·64	3·997	15·987	39·706	644·99	2·543	1·84	43·07	17·564	433·62	91
240·97	963·88	8·176	32·706	67·187	649·06	11·122	9·39	74·49	27·434	460·81	92
205·04	1230·24	9·166	54·993	86·363	646·65	23·188	20·19	93·03	32·607	583·32	93
251·97	1007·88	4·682	18·726	38·804	658·63	6·201	2·15	61·23	18·828	325·87	94
216·1	864·4	1·825	7·301	23·365	671·96	2·375	0·775	41·77	12·834	196·48	95
172·67	941·84	9·021	49·208	84·147	651·34	25·779	19·28	95·88	36·556	519·07	96
281·89	563·78	5·905	11·81	49·357	645·62	4·041	.....	52·216	29·273	337·12	97
220·546	661·64	9·103	27·31	75·963	654·81	14·45	.....	80·84	40·6	427·66	98
195·827	783·31	9·503	38·01	88·09	655·18	24·49	.....	94·5	44·09	474·29	99
301·116	602·232	4·377	8·754	33·382	670·11	1·545	.....	33·86	24·792	586·25	100
288·678	577·356	7·891	15·783	48·75	657·31	4·222	.....	53·11	32·523	478·15	101
182·162	546·486	9·125	27·375	74·834	653·01	13·65	.....	79·44	45·14	470·53	102
186·022	797·24	9·692	41·537	86·059	652·0	23·214	.....	93·06	45·16	532·07	103

TABLE I. (continued).

1. Description.		2. No.	3. Duration of experiment, in minutes.	4. Total pressure of steam in the boiler, in inches of mercury.	5. Head of refrigerating water above its overflow, in inches.	6. Mean temperature of refrigerating water. At its entrance (t).	7. Mean temperature of refrigerating water. At its exit.
Copper steam-tube 4 feet long, area ·7225 sq. ft. Outer tube 1 inch interior diameter. Between the tubes there was a spiral of 103 convolutions, composed of copper wire ·105 inch thick.	Refrigerating water moving in a direction contrary to that of the steam.	104	30	39·99	21·94	14°085	36°025
		105	30	42·01	24·36	14·04	61·013
		106	20	45·15	245	13·95	37·583
		107	15	42·1	270	13·59	48·768
		108	12	45·63	257	13·59	51·97
		109	50	50·35	66·9	13·567	33·782
		110	80	40·6	58	13·545	46·27
	Refrigerating water moving in the same direction with the steam.	111	30	50·14	48	12·44	23·135
		112	30	56·3	48	12·44	40·29
		113	30	57·66	48	12·6	54·32
Copper steam-tube 2 feet long. Interior diameter ·63 inch, exterior ·75 inch, mean area a=·3612 sq. ft. Outer tube, interior diameter 1 inch. Between the tubes there was a spiral consisting of 50 convolutions of copper wire ·105 inch thick.	Refrigerating water moving in the same direction as the steam.	114	30	52·48	24	12·29	26·8
		115	30	45·42	24	10·935	27·46
		116	30	48·34	24	11·07	23·17
		117	30	50·9	24	11·07	28·08
		118	30	44·02	24	9·88	41·1
		119	30	44·125	24	10·17	45·73
		120	30	42·61	24	10·17	59·27
	Refrigerating water moving in a direction contrary to that of the steam.	121	15	44·32	176	8·01	12·93
		122	14	46·15	207	8·01	22·856
		123	15	37·96	193	8·01	29·825
		124	30	44·41	8·4	7·2	23·07
		125	30	43·84	4·47	7·2	48·74
		126	30	45·22	5·8	7·72	66·31
Copper steam-tube 4 feet long. Interior diameter ·63 inch, exterior diameter ·75 inch, mean area a=·7225 sq. ft. Outer tube, interior diameter 1 inch. Between the tubes there was a spiral consisting of 96 convolutions of copper wire ·105 inch thick.	Refrigerating water moving in a contrary direction to that of the steam.	127	30	42·14	274	3·64	16·48
		128	30	45·37	248	3·64	29·93
		129	20	46·37	218	3·65	51·19
	Refrigerating water moving in the same direction as the steam.	130	30	46·38	48	3·51	32·02
		131	30	52·24	48	3·51	42·02
		132	20	49·07	48	3·51	71·81
Copper steam-tube 6 feet long. Interior diameter ·63 inch, exterior diameter ·75 inch, mean area a=1·0837 sq. ft. Interior diameter of the outer tube 1 inch. Between the tubes there was a spiral consisting of 143 convolutions of copper wire ·105 inch thick.	Refrigerating water moving in a direction contrary to that of the steam.	133	30	39·2	22·1	5·53	82·25
		134	30	38·43	24	5·65	57·77
		135	30	41·06	22·6	5·76	38·22
		136	30	47·75	341	4·95	29·72
		137	30	45·43	315	4·905	32·84
		138	30	51·01	279	3·87	42·52
		139	20	44·8	261	3·87	67·27
		140	15	45·6	250	3·85	77·45
		141	30	42·67	301	2·65	32·07
		142	30	42·02	301	2·65	32·96
		143	30	39·34	327	3·28	37·69
		144	30	41·62	337	3·28	28·22
	Refrigerating water moving in the same direction as the steam.	145	30	43·88	72	4·3	41·8
		146	30	43·28	72	4·3	50·32
		147	30	41·11	72	4·3	76·47
Iron steam-tube 4 feet long. Exterior diameter ·74 inch, interior diameter ·602 inch. Interior diameter of the outer tube ·87 inch. A spiral consisting of 55 convolutions of copper wire ·055 inch thick was placed between the tubes.	Refrigerating water moving in the same direction as the steam.	148	30	37·65	48	4·72	31·11
		149	30	37·84	48	4·72	61·27
	Refrigerating water moving in a direction opposite to that of the steam.	150	30	40·33	282	4·2	21·32
		151	20	40·57	265	4·2	42·7

TABLE I. (continued).

8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.
Weight of refrigerating water, in pounds.		Weight of condensed water, in pounds.		Temperature of the condensed water.	Total heat of steam.	Barometer, minus vacuum-gauge, or pressure in the condenser, in inches of mercury.	Pressure in the condenser immediately after the conclusion of the experiment.	Temperature due to the pressure in the condenser (col. 14) per Regnault (t_2).	Temperature of the refrigerating water at its exit, at the times the vacuum was observed (t_1).	Conduction of heat, per square foot of the surface of the steam-pipe, or $\frac{w}{a}\log\left(\frac{t_2-t}{t_2-t_1}\right)$.	No.
In the experiment.	Per hour (w).	In the experiment.	Per hour.								
86·996	173·99	3·15	6·3	36·208	642·14	1·343?	1·74	31·37?	36·441	.....	104
98·037	196·07	7·628	15·256	60·985	664·7	6·566	6·53	62·47	60·931	935·8	105
208·775	626·32	7·965	23·894	43·349	657·79	2·287?	3·18	41 06?	37·136	.....	106
161·15	644·6	9·617	38·47	66·068	653·06	9·715	7·14	71·31	48·176	815·87	107
132·525	662·62	8·68	43·4	73·931	657·8	13·518	11·42	79·2	51·715	798·15	108
147·9	177·48	4·736	5·683	34·968	666·26	1·084?	1·784	27·65?	36·113	.....	109
145·15	108·86	7·836	5·877	41·66	647·84	2·391?	3·69	41·9 ?	48·23	.....	110
87·375	174·75	1·648	3·296	29·64	596·68	0·798?	0·898	22·5 ?	23·755	.....	111
73·343	146·68	3·698	7·396	45·52	597·88	1·57 ?	3·01	34·14?	41·13	.....	112
98·25	196·5	7·456	14·912	59·77	609·53	4·85 ?	6·95	56·0 ?	55·824	.....	113
84·44	168·88	2·108	4·216	31·9	613·1	0·92?	.....	24·86?	26·856	.....	114
100·94	201·88	2·612	5·224	32·71	671·51	1·496	1·496	33·28	27·2	727·52	115
100·87	201·74	1·902	3·804	29·99	671·7	1·116	1·686	28·14	23·27	700·44	116
55·19	110·38	1·456	2·912	31·92	676·7	1·286	1·34	30·6	27·87	601·2	117
86·82	173·64	4·413	8·826	48·46	662·67	3·44	4·34	48·97	41·07	768·58	118
109·63	219·26	6·368	12·736	58·018	670·22	5·741	6·84	59·56	43·57	684·5	119
102·74	205·48	8·708	17·416	79·96	659·25	15·924	17·124	83·27	59·39	636·38	120
172·9	691·6	1·105	4·42	22·21	756·76	0·67	0·9	19·65	12·22	859·4	121
199·65	855·6	4·845	20·764	54·14	661·32	4·57	.....	54·75	22·406	872·28	122
194·28	777·1	7·399	29·596	78·14	649·52	15·61	.....	82·77	29·553	731·1	123
67·9	135·8	1·732	3·464	27·87	650·02	1·126	0·946	28·3	23·17	531·59	124
58·53	117·06	4·155	8·31	60·86	646·05	5·837	5·83	59·92	49·41	522·6	125
67·03	134·06	7·295	14·59	80·14	618·59	17·033	17·03	84·97	66·56	532·54	126
166·59	333·18	3·454	6·908	14·03	633·32	0·682	0·682	19·95	16·48	713·7	127
173·59	347·18	7·395	14·79	28·68	645·81	1·699	1·94	35·56	29·93	833·78	128
108·9	326·7	8·721	26·163	56·12	649·76	5·555	5·205	58·86	51·19	892·52	129
65·44	130·88	3·141	6·282	33·32	627·3	1·722	1·722	35·8	32·02	388·56	130
68·0	136	4·433	8·866	42·27	632·99	2·717	2·717	44·34	42·38	571·56	131
47·87	143·61	5·906	17·718	74·47	628·06	12·173	11·173	76·65	71·81	539·75	132
37·26	74·52	5·26	10·52	79·24	622·7	16·377	15·852	83·98	82·25	262·27	133
35·14	70·28	3·099	6·198	56·4	647·72	5·159	.....	57·29	57·77	.....	134
31·57	63·14	1·593	3·186	35·8	678·93	1·855	1·875	37·16	38·22	.....	135
133·01	266·02	5·266	10·532	23·82	649·47	1·345	1·245	31·4	29·72	676·6	136
123·45	246·9	5·54	11·08	22·09	644·58	1·594	1·694	34·41	32·8	662·57	137
124·64	249·28	7·894	15·788	33·17	643·42	2·634	3·034	43·74	42·52	802·02	138
76·88	230·64	8·266	24·798	64·68	654·34	8·458	8·258	68·13	67·27	918·05	139
60·01	240·04	7·627	30·508	72·49	651·58	13·293	12·49	78·79	77·45	891·3	140
129·2	258.4	6·149	12·298	23·58	641·74	1·468	1·48	32·94	32·07	846·46	141
125·57	251·14	6·116	12·232	28·55	650·86	1·596	1·55	34·44	32·96	710·76	142
128·76	257·52	7·21	14·42	28·743	643·25	2·106	2·206	39·51	37·69	710·74	143
125·82	251·64	5·018	10·036	19·973	645·31	1·2	1·2	29·4	28·22	719·16	144
65·25	130·5	4·157	8·314	40·9	629·53	2·649	2·75	43·86	41·8	355·84	145
63·18	126·36	5·043	10·086	49·6	626·15	3·922	3·72	51·6	50·32	420·87	146
68·87	137·74	9·133	18·266	75·17	619·37	13·11	11·11	78·45	76·47	460·48	147
59·5	119·0	2·571	5·142	32·82	643·56	1·601	1·6	34·49	31·11	368·43	148
68·92	137·84	6·817	13·634	65·61	637·33	8·392	8·0	67·95	61·27	440·92	149
169·64	339·28	4·857	9·714	28·05	626·0	1·412	1·41	32·25	21·32	455·08	150
116·39	349·17	7·786	23·358	62·45	637·97	7·192	6·69	64·48	42·7	505·87	151

On a cursory examination of the Table, it will become evident that the numbers in column 18, representing the conducting power, increase as the space between the tubes, which serves to convey the refrigerating water, is contracted. It will also be noted that an increase of conduction likewise takes place when the quantity of water transmitted between the same tubes is augmented. I will begin by arranging the results so as to show the effect of altering the velocity of the refrigerating water.

Series 1.—Copper steam-tube. Water space between tubes 0·325 inch.

No. of experiment.	Quantity of refrigerating water.		Conductivity.	
1	97·6		98·1	
2	95·7	345·5	158·4	139·6
5	591		152·2	
7	594·4		149·4	
8	597		138·8	
6	631·8		184·3	
9	658	688·8	151·6	172·5
3	758·9		195·7	
4	798·2		191·9	

Series 2.—Copper steam-tube. Water space between tubes 0·06 inch.

No. of experiment.	Quantity of refrigerating water.		Conductivity.	
29	16·37		80·9	
25	18·07		29·26	
18	20·41		89·62	
19	43·57		143·9	
20	61·16	99·24	155·9	152·67
21	76·5		279·8	
22	124·5		198·5	
17	189·47		193·8	
23	343·1		202·3	
26	592·3		297·4	
30	606·3		332·8	
33	611·7		300·4	
24	612·8	689·04	335·6	334·31
32	691·7		256·3	
27	739·8		342·5	
28	767·2		353·6	
31	797·3		408·9	

Series 3.—Copper steam-tube. Water space between tubes 0·025 inch.

No. of experiment.	Quantity of refrigerating water.		Conductivity.	
44	11·2		53·1	
40	21·4		84·5	
39	57·5		255·6	
45	45·3		257·1	
46	91·2		239·1	
43	99·4	95·19	289·4	281·8
53	103·3		305·9	
54	117·4		354·7	
55	129·2		411·7	
41	133·4		303·4	
42	155		303·1	
56	177·8		523·8	
47	455		198·5	
63	493·2		444·3	
58	497·9		446·8	
57	504·8		334·1	
49	511·1		491	
48	523·1	536·17	473·3	457·06
50	533·2		498·6	
59	545·5		589·2	
51	572·5		512·7	
61	586·7		561·4	
60	589·1		564·3	
52	622		370·5	

Series 4.—Lead steam-tube. Water space between tubes 0·05 inch.

No. of experiment.	Quantity of refrigerating water.		Conductivity.	
80	18·2		85·27	
68	29·6		115·51	
79	30·9		127·2	
74	32·2	74·42	138·2	164·1
71	33·8		145·7	
69	155·2		256·5	
78	221·1		280·2	
72	394·1		252·2	
70	417·8		330·5	
75	624·1	590·24	127·2	270·7
73	655·4		336·2	
76	656·1		212·7	
77	794		365·4	

We deduce from the averages in Series 1 . . . . $C \propto w^{\frac{1}{3\cdot 30}}$.

" 2 . . . . $C \propto w^{\frac{1}{3\cdot 47}}$.

" 3 . . . . $C \propto w^{\frac{1}{3\cdot 57}}$.

" 4 . . . . $C \propto w^{\frac{1}{4\cdot 14}}$.

Suppose we take the average index, then $C \propto w^{\frac{1}{2\cdot25}}$ will express the influence of the quantity of refrigerating water on the conductivity with sufficient accuracy. But it is evident that this relation can only be relied on between certain limits, indicated pretty plainly by the experiments. The influence of a change in the quantity of refrigerating water is doubtless gradually lessened as the flow is increased, and ultimately at a very high velocity the conductivity must necessarily reach a constant value.

To find the influence of the extent of the water space, successively narrowed by diminishing the diameter of the outside tube, we will select those experiments in which the flow of water was nearly the same in quantity.

Width of water space between the tubes.	No.	Quantity of refrigerating water.		Conductivity.	
0·325 inch.	5	591·01		152·24	
	6	631·85		184·3	
	7	594·47	614·46	149·44	155·27
	8	597·01		138·79	
	9	657·99		151·6	
0·06 inch.	24	612·82		335·65	
	26	592·3		297·38	
	30	606·33	622·96	332·85	304·52
	32	691·67		256·3	
	33	611·69		300·41	
0·025 inch.	48	523·12		473·3	
	49	511·08		490·99	
	50	533·23		498·64	
	51	572·52		512·66	
	52	621·95	554·22	370·49	488·34
	57	504·77		334·13	
	59	545·47		589·21	
	60	589·14		564·26	
	61	586·75		561·39	

Reducing the conductivity in each case to the flow of 618 lbs. of water, by the rule just found, we deduce for the spaces ·325, ·06, and ·025, the conductivities 156, 303·7, and 504·4 respectively. Whence, for the circumstances of the experiments, it follows that

$$C \propto S^{\frac{1}{2\cdot188}}.$$

The above laws are neither exact, nor universal in their application, but they afford the means of estimating the probable amount of benefit to be anticipated from increasing the rapidity of the refrigerating stream in such tubes as I have employed, which are indeed of the dimensions most likely to be practically adopted.

I pass now to the consideration of the effect of cleanliness of surface. In the experiments 62, 63, and 64, the outside of the copper steam-tube was made greasy by rubbing it with oil. In the five immediately preceding these the tube was kept perfectly clean, so that water readily adhered to it.

State of surface.	No.	Quantity of refrigerating water.		Conductivity.	
Clean.	57	504·77	544·81	334·13	499·16
	58	497·9		446·82	
	59	545·47		589·21	
	60	589·14		564·26	
	61	586·75		561·39	
Greasy.	62	474·59	503·42	381·96	440·09
	63	493·16		444·27	
	64	542·5		594·05	

The conductivity with the oiled tube, reduced to 544·8 lbs. of refrigerating water by means of the relation we have deduced, will be 450·6: the closeness of this number to 499·16 shows that the influence of a greasy surface is inconsiderable.

The experiments 86 to 96 inclusive, are proper to determine whether any effect can be produced by placing a solid in the axis of the steam-tube.

Description.	No.	Quantity of refrigerating water.		Conductivity.	
Thin end of the tapered rod uppermost.	90	585·3	941·27	278·07	438·95
	91	985·64		433·62	
	92	963·88		460·81	
	93	1230·24		583·32	
Thick end of the rod uppermost.	94	1007·88	938·04	325·87	347·14
	95	864·4		196·48	
	96	941·84		519·07	

Selecting similar experiments, with the exception that the core was not present, we have

No.	Quantity of refrigerating water.		Conductivity.	
27	739·8	721·76	342·5	339·39
28	767·16		353·6	
31	797·34		408·89	
32	691·69		256·3	

The conductivity in the last instance, reduced to 940 lbs. of refrigerating water, will be 367·1, a number which does not differ sufficiently from 439 and 347 to lead us to expect any practical advantage from narrowing the steam space.

Let us now inquire into the effect of changing the direction in which the refrigerating water was transmitted. Its usual direction was contrary to that of the steam and condensed water; but by removing the pipe E (see figure) and pouring the water into the upper part of the outer tube C, it could be made to flow in the same direction. The experiments suitable for ascertaining the effect of changing the direction of flow are collected in the following Tables:—

Series 1.—Thickness of water space 0·06 inch.

Direction of water.	No.	Quantity of refrigerating water.		Conductivity.	
Contrary to the steam.	24	612·82		335·65	
	27	739·8		342·5	
	28	767·16		353·6	
	30	606·33	689·55	332·85	332·89
	31	797·34		408·89	
	32	691·69		256·3	
	33	611·69		300·41	
The same as that of the steam.	34	657·25		522·1	
	35	1244		466·96	
	36	1292·2	1158·3	474·32	486·94
	37	1302·1		491·56	
	38	1296		479·77	

Series 2.—Thickness of water space 0·025 inch.

Direction of water.	No.	Quantity of refrigerating water.		Conductivity.	
Contrary to the steam.	41	133·45		303·45	
	42	155·04		303·08	
	53	103·32	136·04	305·89	367·11
	54	117·42		354·74	
	55	129·21		411·66	
	56	177·79		523·85	
The same as that of the steam.	65	150·75		228·48	
	66	184·88	182·79	412·24	350·42
	67	212·75		410·55	

Thus with the refrigerating water flowing in a direction opposite to that of the steam, we have the conductivities 332·89 and 367·11; whilst with the water flowing in the same direction as the steam, we have the conductivities (referred to the same quantities of refrigerating water) 417·3 and 320·96. The means for the two directions are 350 and 369·13, whence we may conclude that the conductivity is little influenced by the direction in which the water flows.

We will now consider the influence of the kind of metal of which the steam-tubes were made. In the Table will be found results obtained with tubes of copper, iron, and lead.

Metal.	No.	Refrigerating water.		Conductivity.	
Copper.	23	343·1		202·28	
	24	612·82		335·65	
	26	592·3		297·38	
	27	739·8		342·5	
	28	767·16	640·25	353·6	314·43
	30	606·33		332·85	
	31	797·34		408·89	
	32	691·69		256·3	
	33	611·69		300·41	
Iron.	84	556	506·3	279·24	271·7
	85	456·6		264·16	
Lead.	70	417·82		320·53	
	72	394·07		252·19	
	73	655·42	590·24	336·18	270·7
	75	624·07		127·19	
	76	656·07		212·67	
	77	793·98		365·43	

The water spaces around the copper, iron, and lead tubes were respectively ·06, ·065, and ·05 inch wide. By reducing all the mean results to the space ·06 and 640·25 lbs. of water by means of the formulæ we have already deduced, we obtain for the conducting power with the three tubes the numbers 314·4, 302·2, and 255·1 respectively. Taking into account the thickness of the metal, which was ·06 in the copper, ·069 in the iron, and ·125 in the lead tube, we arrive at the conclusion that the resistance to conduction through the metal itself is so small in comparison with the resistance at the bounding surface of the metal and through the adhering films of water (inside as well as outside of the steam-tube), as to be almost inappreciable.

We have seen that the tendency of the water flowing between the tubes is to adhere to their sides, and that a head of water of considerable height is required in order to give the water sufficient velocity to remove the adhering film rapidly. It seemed possible that part of the force due to the head might be employed for the purpose of agitating the water. I have not yet found an opportunity to construct an apparatus for this purpose, but it occurred to me that the same object might be attained by placing a wire bent into the form of a spiral between the tubes. By this means the water would be impelled in a spiral direction, which would contribute largely to the rapid intermixture of the particles of water as they advanced. Accordingly, in experiments 97, 98, and 99, this arrangement was tried for the first time. The spiral (in these three experiments only) was half of it left-handed, and the other half right-handed, so that the rotatory motion produced by the first half was reversed in the second. Although the thickness of the wire which formed the spiral was only one-third of the width of the water space in which it was placed, the effect it produced was marked, as the following results testify:—

No.	Head of water.		Quantity of refrigerating water.		Conductivity.	
97	1·5	1·83	563·78	669·58	337·12	413·02
98	1·0		661·64		427·66	
99	3·0		783·31		474·29	

If we contrast these results with those obtained with the same tubes unfurnished with spirals, we shall find

No.	Head of water.		Quantity of refrigerating water.		Conductivity.	
3	2·27	1·43	758·9	661·34	195·68	166·27
4	2·4		798·16		191·86	
5	0·94		591·01		152·24	
6	1·33		631·85		184·3	
7	1·03		594·47		149 44	
8	0·94		597·01		138·79	
9	1·08		657·99		151·6	

proving that a great increase of conductivity was obtained by the use of the spiral, without entailing the necessity of a much higher head of water.

The effect of increasing the velocity of the spirally directed refrigerating water will appear from the following experiments:—

No.	Head of water.		Quantity of refrigerating water.		Conductivity.	
124	8·4	6·22	135·8	128·97	531·59	528·91
125	4·47		117·06		522·6	
126	5·8		134·06		532·54	
121	176	192	691 6	774·77	859·4	820·93
122	207		855·6		872·28	
123	193		777·1		731·1	

whence we find $C \propto (W)^{\frac{1}{4\cdot679}}$.

By classifying the experiments so as to show the comparative effect of transmitting the refrigerating stream in the same direction with, and opposite to, the steam and condensed water, we obtain the following Table:—

Description.	No.	Quantity of refrigerating water.	Conductivity.
Copper steam-tube 2 feet long. Water in the same direction with the steam.	115	201·88	727·52
	116	201·74	700·44
	117	110·38	601·2
	118	173·64	768·58
	119	219·26	684·5
	120	205·48	636·38
		185·4	686·44
Copper steam-tube 2 feet long. Water moving in the opposite direction to the steam.	121	691·6	859·4
	122	855·6	872·28
	123	777·1	731·1
	124	135·8	531·59
	125	117·06	522·6
	126	134·06	532·54
		451·87	674·92
Copper steam-tube 4 feet long. Water in the same direction as the steam.	130	130·88	388·56
	131	136	571·56
	132	143·61	539·75
		136·83	499·29
Copper steam-tube 4 feet long. Water in the contrary direction.	127	333·18	713·7
	128	347·18	833·78
	129	326·7	892·52
		335·69	813·33
Copper steam-tube 6 feet long. Water in the same direction as the steam.	145	130·5	355·84
	146	126·36	420·87
	147	137·74	460·48
		131·53	412·4
Copper steam-tube 6 feet long. Water in the contrary direction.	136	266·02	676·6
	137	246·9	662·57
	138	249·28	802·02
	139	230·64	918·05
	140	240·04	891·3
	141	258·4	846·46
	142	251·14	710·76
	143	257·52	710·74
	144	251·64	719·16
		250·17	770·85
Iron steam-tube 4 feet long. Water in the same direction as the steam.	148	119	368·43
	149	137·84	440·92
		128·42	404·67
Iron steam-tube 4 feet long. Water in the opposite direction.	150	339·28	455·08
	151	349·17	505·87
		344·22	480·47

The above mean results are collected and averaged as follows:—

Direction of stream.	Quantity of water.	Conductivity.
With the steam.	185·4	686·44
	136·83	499·29
	131·53	412·4
	128·42	404·67
	145·54	500·7
Contrary to the steam.	451·87	674·92
	335·69	813·33
	250·17	770·85
	344·22	480·47
	345·49	684·89

The conductivities for the different directions of the flow of refrigerating water will

therefore be 500·7 and $\left(\frac{145{\cdot}54}{345{\cdot}49}\right)^{\frac{1}{4{\cdot}078}} \times 684{\cdot}89 = 554{\cdot}06$. The difference between the two values is not great. If we average them with the results obtained when the tubes were not furnished with spirals, we shall obtain the following result:—

Tubes employed.	Conductivity. Water going in the same direction.	Conductivity. Water going opposite to the steam.	Ratio of conductivities.
Plain	369·13	350	0·9482
Furnished with spirals.	500·7	554·06	1·1065
Mean	.....	.....	1·0273

showing a trifling advantage on the side of the arrangement in which the refrigerating water goes in a contrary direction to the steam and condensed water, which is, however, too small to be attributed to anything beyond experimental errors.

The quantity of transmitted water being, *cæteris paribus*, nearly proportional to the square root of the height of the head, it is evident that the limit to the economical increase of the conductivity by diminishing the thickness of the water space, or by increasing the velocity of the stream, is soon attained. Hence, as I have already observed, the importance of any method which promotes the rapid removal of the adhering film of water without necessitating a great initial pressure. I have arranged my results, with reference to the head of water in the following Tables, so as to enable a comparison to be readily made in this respect between the plain tubes and those furnished with spirals.

TABLE I.—Plain Tubes.

Description.	No.	Head of water.		Conductivity.	
Copper steam-tube 4 feet long. Thickness of water space 0·325 inch.	2	− 0·15		158·46	
	1	0·2		98·13	
	5	0·47		152·24	
	8	0·47		138·79	
	7	0·52	0·56	149·44	157·83
	9	0·54		151·6	
	6	0·66		184·3	
	3	1·13		195·68	
	4	1·2		191·86	
Copper steam-tube 4 feet long. Thickness of water space 0·06 inch.	18	− 1·4		89·62	
	21	− 0·49		279·85	
	19	− 0·13	0·69	143·92	176·92
	20	0·6		155·91	
	22	1·19		198·48	
	17	4·37		193·77	
	23	6·45		202·28	
	90	10·8		278·07	
	33	11·48		300·41	
	30	12·9	12·08	332·85	286·13
	24	14·07		335·65	
	32	14·15		256·3	
	26	14·73		297·38	

TABLE I.—Plain Tubes (continued).

Description.	No.	Head of water.		Conductivity.	
Copper steam-tube 4 feet long. Thickness of water space 0·06 inch.	31	20·12		408·89	
	27	21·45		342·5	
	28	23·29		353·6	
	96	25·0		519·07	
	95	25·66	26·44	196·48	402·68
	92	27·66		460·81	
	91	30·25		433·62	
	94	32·0		325·87	
	93	32·5		583·32	
	86	48		435·25	
	87	48		540·42	
	88	48		611·33	
	89	48		581·02	
	34	48	48	522·1	511·41
	35	48		466·96	
	36	48		474·32	
	37	48		491·56	
	38	48		479·77	
Copper steam-tube 4 feet long. Thickness of water space 0·025 inch.	40	— 0·1		84·55	
	44	0·5	3·11	53·1	131·58
	45	9·92		257·1	
	39	12·8		255·6	
	43	18·33	21·6	289·44	327·86
	55	26·66		411·66	
	54	28·6		354·74	
	46	28·58		239·09	
	53	28·74		305·89	
	56	34·66	32·95	523·85	335·07
	42	35·61		303·08	
	41	37·16		303·43	
	65	48		228·48	
	66	48	48	412·24	350·42
	67	48		410·55	
	63	193·5		444·27	
	49	206·3		490·99	
	47	210·2		198·52	
	62	211	208·6	381·96	422·18
	58	211·3		446·82	
	50	211·7		498·64	
	64	216		494·05	
	61	223·5		561·39	
	57	231·4		334·13	
	48	232		473·3	
	59	233	240·7	589·21	486·49
	60	235·9		564·26	
	51	237·2		512·66	
	52	292·06		370·49	

TABLE II.—Tubes furnished with Spirals.

Description.	No.	Head of water.	Conductivity.
Copper steam-tube 4 feet long. Water space 0·325 inch. Spiral of 45 turns of wire 0·21 inch thick.	100	1·95	586·25
	101	2·06	478·15
	102	1·43 } 2·53	470·53 } 516·75
	103	4·7	532·07
Copper steam-tube 2 feet long. Water space 0·125 inch. Spiral of 50 turns of wire 0·105 inch thick.	125	4·47	522·6
	126	5·8 } 6·22	532·54 } 528·91
	124	8·4	531·59
	115	24	727·52
	116	24	700·44
	117	24	601·2
	118	24 } 24	768·58 } 686·44
	119	24	684·5
	120	24	636·38
	121	176	859·4
	122	207 } 192	872·28 } 820·93
	123	193	731·1
Copper steam-tube 4 feet long. Water space 0·125 inch. Spiral of 96 turns of wire 0·105 inch thick.	130	48	388·56
	131	48 } 48	571·56 } 499·96
	132	48	539·75
	129	218	892·52
	128	248 } 246·66	833·78 } 813·33
	127	274	713·7
Copper steam-tube 6 feet long. Water space 0·125 inch. Spiral of 143 turns of wire 0·105 inch thick.	133	22·1	262·27
	145	72	355·84
	146	72 } 72	420·87 } 412·4
	147	72	460·48
	140	250	891·3
	139	261	918·05
	138	279	802·02
	141	301	846·46
	142	301 } 301·33	710·76 } 770·85
	137	315	662·57
	143	327	710·74
	144	337	719·16
	136	341	676·6

The averaged results of the preceding Tables are collected together as follows:—

Description.	Head of water.	Conductivity.
Plain tube	0·56, 0·69, 3·11 } 1·44	157·83, 176·92, 131·58 } 155·44
Tube with spiral	2·53	516·75
Plain tube	12·08	286·13
Tube with spiral	12·44	528·91
Plain tube	48, 48 } 48	511·4, 350·42 } 430·91
Tube with spiral	48, 48, 48 } 48	686·44, 499·96, 412·4 } 532·93
Plain tube	240·7	486·49
Tube with spiral	384, 246·66, 200·88 } 277·18	820·93, 813·33, 770·85 } 801·7

The cause of the inferiority of the plain tubes may be attributed in some measure to a want of perfect concentricity and truth in the pipes, resulting in an irregular action of the refrigerating water, the greatest quantity of which would thus be transmitted through the widest part of the water space. In the arrangement with spirals, the width of the water space was too great for any such circumstance to have a sensible influence. I think, however, that the imperfections of the tubes and of their concentricity were not such as to account for the great advantage which appeared to be produced by the spirals in my experiments, and I therefore attribute it to the continuous intermixture of the particles of water favoured by that arrangement.

The following is a summary of the principal foregoing results:—

1st. The pressure in the vacuous space is sensibly equal in all parts.

2nd. In the arrangement in which the steam is introduced into a tube whilst the refrigerating water is transmitted along a concentric space between the steam-tube and a larger tube in which it is placed, it is a matter of indifference in which direction the water is transmitted. Hence,

3rd. The temperature of the vacuous space is sensibly equal in all parts.

4th. The resistance to conduction is to be attributed almost entirely to the film of water in immediate contact with the outside and inside surfaces of the tube, and is little influenced by the kind of metal of which the tube is composed, or by its thickness in the limits of ordinary tubes, or even by the state of its surface as to greasiness or oxidation.

5th. The narrowing of the steam space by placing a rod in the axis of the steam-tube does not produce any sensible effect.

6th. The conductivity increases as the rapidity of the stream of water is augmented. In the circumstances of my experiments, the conduction was nearly proportional to the cube root of the velocity of the water; but at very low velocities it evidently increases more rapidly than according to this law, whilst at high velocities it increases less and less rapidly as it gradually approaches a limit determined by the resistance of the metal and of the film of water adhering to the inside surface of the tube.

7th. The conductivity increases so slowly in relation to the height of the head of water, that the limit to the economical increase of the latter is soon attained.

8th. By means of a contrivance for the automatical agitation of the particles of the refrigerating stream, such as the spirals I have employed, an improvement in the conductivity for a given head of water takes place.

9th. The total heat of steam above 0° Cent., determined by the average of the 151 experiments, is 644°·28 for a pressure of 47·042 inches.

The experiments in which air was employed as the refrigerating agent were made in a similar manner to those in which water was used. At high pressures the air was propelled by the condensing pump used by Professor THOMSON and myself in our experiments, and at low pressures a large organ-bellows was employed. The temperature of the air at its exit was obtained by placing the thermometer immediately over the concentric space between the tubes, varying its position from time to time so as to obtain an average result for the entire section of the channel.

TABLE II.—Atmospheric Air, the refrigerating agent,

1.	2.	3.	4.	5.	6.	7.
Description.	No.	Duration of experiment, in minutes.	Total pressure of steam in the boiler, in inches of mercury.	Pressure required to propel the air, in inches of water.	Mean temperature of the refrigerating air.	
					At its entrance (t).	At its exit (t_1).
Copper steam-tube 4 feet long. Exterior diameter 0·75 inch, interior 0·63 inch. Outer tube 0·8 inch interior diameter.	1	60	73·3	231	13·83	94·12
	2	60	72·16	201	13·83	90·49
	3	60	82·1	228	19·03	99·4
The same copper steam-tube. Outer tube 0·87 inch interior diameter.	4	48	62·74	31·8	13·18	81·64
	5	60	73·16	31·48	14·4	86·3
The same copper steam-tube. Outer tube 1 inch interior diameter.	6	60	51·23	1·36	10·94	80·64
	7	60	43·58	3·5	12·53	76·83
	8	60	41·64	3·5	13·86	73·7
	9	60	41·51	5·52	11·74	72·84
	10	60	46·53	5·52	11·38	72·86
The same copper steam-tube. Outer tube interior diameter 1·4 inches.	11	48	43·67	5·52	10·26	42·07
	12	60	53·16	5·52	8·8	43·44
	13	60	48·9	5·52	9·47	44·2
	14	60	42·33	1·28	10·48	48·47
	15	60	49·05	1·3	10·93	49·88
The same tubes. A spiral of 30 turns of copper wire $\frac{1}{10}$th inch thick was wound round the steam-tube. Half of this spiral was right-handed, the other half left-handed.	16	60	43·54	1·3	10·57	73·58
	17	60	42·32	5·32	14·87	67·29
Copper steam-tube 2 feet long. Exterior diameter 0·75 inch. Outer tube 1·4 inch interior diameter. A spiral of 20 turns of copper wire 0·21 inch thick between the tubes.	18	60	41·76	1·44	9·13	69·13
	19	60	46·88	3·55	8·46	63·33
Copper steam-tube 1 foot long. Exterior diameter 0·75 inch. Outer tube 1·4 inch interior diameter. A spiral of 10 turns of copper wire 0·21 inch thick between the tubes.	20	60	45·04	1·44	6·43	52·87
	21	60	45·06	3·55	8·23	46·73

propelled in a direction contrary to that of the Steam.

8.	9.	10.	11.	12.	13.	14.	15.	16.	17.
Quantity of water in pounds, equal in capacity for heat to the refrigerating air.		Weight of condensed water, in pounds.		Temperature of condensed water.	Total heat of steam.	Barometer, minus vacuum-gauge, or pressure in the condenser, in inches of mercury.	Temperature due to the pressure in the condenser per Regnault's tables (t_2).	Conduction of heat per square foot of the surface of the steam-pipe, or $\frac{w}{a}\log\left(\frac{t_2-t}{t_2-t_1}\right)$.	No.
In experiment.	Per hour (w).	In experiment.	Per hour.						
6·614	6·614	1·09	1·09	95·29	582·48	25·88	96	34·58	1
5·622	5·622	0·754	0·754	93·55	665·15	21·19	90·63	49·08	2
6·244	6·244	0·85	0·85	71	661·39	30·5	100·56	36·75	3
5·28	6·6	0·69	0·86	71·68	595·55	22·744	92·5	18·16	4
6·707	6·707	0·996	0·996	84·07	568·22	27·268	97·42	18·66	5
4·562	4·562	0·661	0·661	90·31	571·35	27·26	97·41	10·36	6
8·298	8·298	0·948	0·948	90·4	653·23	27·857	98·01	16·03	7
8·3	8·3	0·865	0·865	83·97	658·16	25·446	95·52	15·17	8
10·157	10·157	1·129	1·129	90·55	640·23	26·328	96·45	17·94	9
10·157	10·157	1·133	1·133	92·22	643·37	26·25	96·38	18·06	10
25·208	31·51	1·375	1·719	68·68	651·86	27·33	97·49	19·78	11
32·085	32·085	2·076	2·076	98·43	633·8	30·01	100·08	21·19	12
32·14	32·14	2·08	2·08	97·73	634·37	30·05	100·11	21·49	13
14·97	14·97	1·156	1·156	98·25	590·21	30·03	100·1	11·43	14
13	13	1·006	1·006	97·21	600·56	30·146	100·21	10·31	15
8·4	8·4	1·109	1·109	97·37	574·63	30·09	100·16	14·13	16
18·5	18·5	1·92	1·92	99·39	604·48	30·366	100·42	24·29	17
4·64	4·64	0·548	0·548	102·61	610·64	30·06	100·13	13·83	18
7·744	7·744	0·748	0·748	102·55	670·61	30·08	100·15	19·55	19
5·578	5·578	0·506	0·506	96·86	608·81	30·04	100·11	21·14	20
9·122	9·122	0·629	0·629	99·12	657·47	30·06	100·13	27·42	21

On examining the Table of results with air as the refrigerating agent, we may remark,—

1st. That a film of air does not *adhere* to the surface of the tube so tenaciously as a film of water does. This is evident from a comparison of Nos. 1, 2 and 3, with 4 and 5; from which it appears that for the spaces ·025 and ·06 inch the pressures able to propel equal quantities of air were as 7·66 to 1, or nearly as the squares of the velocities. When water was employed in the same tubes, these pressures were as 18·8 to 1.

2ndly. That the velocity of the elastic fluid appears to exercise a much more considerable influence on the conductivity than it does in the case of water.

3rdly. That spirals exercise a beneficial influence. This will be noted on comparing Nos. 6 to 15 with Nos. 16 and 17.

The very small conductivity when Air is the refrigerating agent will probably prevent its being employed for the condensation of steam, except in very peculiar cases.

I must remark, in conclusion, that the above research, however laborious, has left much to be accomplished. One of my chief objects was to obtain figures which might prove useful to practical men, and I have therefore confined myself to such tubes as were most likely to be generally used. In taking up the subject afresh, greater accuracy might be attained by the use of a sheaf of tubes, so as, by condensing a larger quantity of steam, to diminish the amount of temperature corrections. It would also be desirable to employ tubes of great thickness, so as to obtain the conductivity of metals after eliminating the resistance of the fluid film. The effect of irregularities in the water space might also be exactly determined, and the action of arrangements for agitating the refrigerating water more completely discussed than I have been able to do in the present memoir.

IX. *On the Effect produced on the Deviations of the Compass by the Length and Arrangement of the Compass-Needles; and on a New Mode of correcting the Quadrantal Deviation.* *By* Archibald Smith, *Esq., M.A., F.R.S., late Fellow of Trinity College, Cambridge; and* Frederick John Evans, *Esq., R.N., Superintendent of the Compass Department of Her Majesty's Navy.*

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Introductory observations.

In the mathematical investigations of the deviation of the compass which have been hitherto published, and in the practical methods for correcting the deviation which have been proposed by Captain Flinders, Mr. Barlow, and Mr. Airy, the assumption is tacitly or expressly made that the length of the compass-needle may be considered as infinitesimal compared with the distance of the nearest disturbing iron. Expressed mathematically, as only even powers of the ratio of the projection of the needle on the direction of the disturbing iron to the distance of the disturbing iron enter into the expressions, the assumption is that the square and higher powers of that ratio may be neglected. By this assumption the formulæ which express the deviation are materially simplified; and on the supposition that the iron of a ship consists entirely of iron of one or other of the two extreme qualities described magnetically as "hard" iron and "soft" iron, the following expression is accurately true:—

$$\sin\delta = A\cos\delta + B\sin\zeta' + C\cos\zeta' + D\sin(\zeta+\zeta') + E\cos(\zeta+\zeta');$$

in which ζ is the azimuth of the ship's head measured *eastward* from the *correct magnetic north*;

ζ' is the same azimuth, but measured from the direction of the *disturbed needle*;

$\delta = \zeta - \zeta'$ is the *easterly* deviation of the needle;

A, D, E are coefficients depending only on the distribution of the soft iron of the ship, and being independent of the magnetic dip and force, and therefore not changing with a change in the geographical position of the ship.

B and C are coefficients depending partly on the distribution of the hard and soft iron of the ship, and partly on the dip and horizontal force, and therefore changing with a change of geographical position of the ship, as well as with a change in the magnetism of the hard iron in the ship.

If the soft iron of the ship be symmetrically arranged on each side of the fore-and-aft vertical plane which passes through the compass, the coefficients A and E are zero, and in all ships the deviations of which have been examined, these coefficients are so small

that they may be neglected, so that we have in all such cases

$$\sin\delta = B\sin\zeta' + C\cos\zeta' + D\sin(\zeta+\zeta');$$

and if the deviation be of such an amount that we may take δ for $\sin\delta$, we have

$$\delta = B\sin\zeta' + C\cos\zeta' + D\sin(\zeta+\zeta').$$

The first two terms of this expression represent a deviation which is zero when the ship's head is on either of two opposite points, called the neutral points, and which is easterly when the ship's head is in one of the semicircles terminated by these points, and westerly when it is in the other semicircle, and which deviation is therefore called the *Semicircular Deviation*.

The third term represents a deviation which is zero when $\frac{\zeta+\zeta'}{2}$, *i. e.* the azimuth of the ship's head measured from a line half-way between the correct magnetic north and the direction of the disturbed needle $=0°$, $90°$, $180°$, or $270°$, and which has its maximum (without regard to sign) when $\frac{\zeta+\zeta'}{2}=45°$, $135°$, $225°$, or $315°$, or, in other words, which has its zero-points when the ship's head is near the four cardinal points, its maximum values (independently of sign) when the ship's head is near the centre of the N.E., S.E., S.W., and N.W. quadrants, and is hence called the *Quadrantal Deviation*.

The deviation may be corrected either mathematically or mechanically.

In the Royal Navy, as a rule, no mechanical correction is applied to the compasses. A standard compass, having a card 7·6 inches in diameter, is fixed in a convenient position in the ship, at an elevation of about 5 feet above the deck, and as far as possible from any iron, and the deviations on each course are ascertained and allowed for by reference to a table or curve of deviations constructed for the particular ship from actual observation. In the Royal Navy, therefore, the distance of the nearest iron from the compass is in general so great that the assumption to which we have referred may be made without sensible error.

In iron ships of the Mercantile Marine, the mechanical method of correction is extensively practised, which was originally proposed by Mr. Airy in his well-known paper in the 'Philosophical Transactions' for 1839, and which is described in greater detail in his 'Results of Experiments on the Disturbance of the Compass in Iron-built Ships' (Weale, 1840). In this method the semicircular deviation is corrected by two or more horizontal magnets, the part B being corrected by a magnet or magnets directed fore-and-aft, the part C by a magnet or magnets directed athwart-ships. Each magnet is fixed with its centre in either the fore-and-aft or the athwart-ships vertical plane passing through the centre of the compass, or in the intersection of these planes. The quadrantal deviation D is corrected by masses of soft iron placed on each side of and at the same level with the compass-needle. When the distance and position of the ship's iron and of the correctors are such that the square of the ratio of the projection of the needle on the direction of the disturbing iron to the distance of the disturbing iron may be neglected, the correction thus made may be considered as perfect for the place and

time at which it is made. But when this is not the case, from the iron of the ship being too near the compass, or from the correctors being from necessity brought too near the compass, or from the length of the compass-needle itself being excessive, an error is introduced which it is the object of this paper to consider.

That the effect of this error has been for some time felt by practical compass adjusters, appears probable from the difficulties which are reported to have been experienced in correcting the deviation of certain ships by Mr. AIRY'S method, and from the advantage reported to have been derived in some of such cases from the use of compasses with two needles; but we are not aware that the particular nature of the error to which we refer has been hitherto pointed out, or considered either experimentally or theoretically.

Experiments.

The attention of Mr. EVANS was drawn to this subject by the observations made in the Great Eastern on her experimental voyage from the Thames to Portland, and afterwards while she was lying at Holyhead and Southampton*. The standard compass of this ship was fitted with needles of unusual length, viz. two needles of 11½ inches in length placed near each other. Its deviations had been carefully corrected by Mr. GRAY of Liverpool, by magnets and soft iron, but after such corrections, and when the deviation was nearly corrected on the cardinal and quadrantal points, there were errors of between 5° and 6° on some of the intermediate points. The importance of this case has induced us to append a Table of the deviations (Appendix I.) of this compass on the points on which the deviations were observed as the ship swung to her moorings on the days of observation at Holyhead in October 1859, and in Southampton Water in June 1860. These observations indicated the existence of a considerable error, which was neither semicircular nor quadrantal, and thus apparently of some source of error which had not been taken into account by Mr. AIRY in his plan of mechanical correction. In order to ascertain the cause of these apparently anomalous results, Mr. EVANS instituted a series of experiments on the deviations produced on compass-needles of different lengths by magnets and soft iron placed in different positions with respect to them.

For this purpose a bar magnet 12 inches long was placed on a horizontal table revolving round a fixed vertical axis, on which axis a compass was placed at different elevations. The magnet was placed at distances of 17 and 20 inches from the centre of the compass, and in one set of observations was turned endways, and in another sideways to the compass. Observations were made on single edge bar needles of 3, 6 and 12 inches in length, and on Admiralty Standard compass cards of 7·6 and 3·8 inches diameter. Observations were also made of the deviations produced by two cylinders of soft iron arranged as if for correcting the quadrantal deviation, on a 7½-inch single needle, and on an Admiralty Standard compass.

* See Mr. EVANS's paper, entitled "Reduction and Discussion of the Deviations of the Compass observed on board of all the Iron-built Ships, and a selection of the Wood-built Steam-ships in H.M. Navy, and the Iron Steam-ship Great Eastern," Philosophical Transactions, 1860, p. 337.

The Admiralty Standard compass card, it may be observed, is constructed with parallel needles, placed as chords of the circular rim of the card, and so arranged that the moment of inertia of the card about every diameter is the same, the object of this arrangement being to prevent the "wabbling" motion of a card of which the moments of inertia are unequal. It was long ago observed by Mr. SMITH, that for this purpose, with two uniform needles, it is necessary and sufficient that the ends of the needles should be separated by 60° of arc. The object is therefore obtained with *two* uniform needles, the extremities of which are 30° measured along the circumference of the card, from the extremities of the diameter which is parallel to them, and which needles are therefore chords of 60°, or with *four* uniform needles placed two and two with their extremities at equal distances on each side of the chords of 60°. The last is the arrangement adopted in the Admiralty Standard compass, the distances of the extremities of the needle being 15° on each side of the chords of 60°, so that the extremities of the needle are placed at 15° and 45° on each side of the extremities of the parallel diameter. The importance of this arrangement with reference to the present question will be seen hereafter.

The details of Mr. EVANS's experiments are given in the Table in Appendix II.

The peculiarities of the several Tables of observations are most conveniently discussed by representing the deviations graphically by means of a curve, according to the method known as NAPIER'S method, and mathematically by means of the formula

$$\delta = A + B\sin\zeta' + C\cos\zeta' + D\sin 2\zeta' + E\cos 2\zeta' + F\sin 3\zeta' + G\cos 3\zeta' + H\sin 4\zeta' + K\cos 4\zeta' + \&c.,$$

in which, as before, δ represents the deviation, ζ' the azimuth of the line corresponding in the experiments to the fore-and-aft line in a ship.

The curves which accompany this paper have been drawn in this way from the actual observations made with the magnet and cylinders of soft iron in the same horizontal plane with the needle. It has not been thought necessary to lay down the curves representing the observations with the elevated compass, but their peculiarities will be described.

From the curves the values of the deviations on each of the thirty-two points, reckoned from the *disturbed* direction of the needle, have been obtained by measurement, and the values of the coefficients A, B, C, D, E, F, G, H, K computed by the method of least squares by the formulæ given in part in the Philosophical Transactions for 1846, p. 350, and more conveniently in the "Supplement to the Practical Rules for ascertaining the Deviations of the Compass which are caused by the Ship's Iron," published by order of the Lords Commissioners of the Admiralty (London, POTTER, 1855).

The following are the values of the coefficients so obtained:—

Compass cards used.		Deviation caused by	A	B	C	D	E	F	G	H	K
Single needle	inches. 3 ...	Bar magnet placed endways.	−0° 9′	−30° 28′	+0° 1′	−0° 1′	+0° 20′	−0° 8′	−0° 3′	−0° 6′	−0° 2′
	6 ...		−0 19	−30 47	+0 8	+0 9	+0 19	−1 25	−0 2	+0 1	−0 1
	12 ...		−0 9	−31 45	+0 10	+1 4	+0 11	−5 38	−0 2	+0 16	+0 5
Compound needles	3½ ...		−0 5	−35 17	+0 1	−0 38	−0 9	+0 55	+0 11	−0 8	−0 6
	7½ ...		−0 12	−36 26	+0 4	0 0	+0 8	+0 39	+0 1	−0 2	+0 6
Single needle	3 ...	Bar magnet placed sideways.	−0 4	+24 11	−1 12	−0 36	+0 15	−0 59	+0 30	+0 1	+0 12
	6 ...		+0 2	+24 34	−1 0	−0 33	+0 25	−4 30	−0 22	+0 2	+0 10
	12 ...		−0 16	+25 47	−1 12	−0 45	+1 44	−9 56	+0 20	+0 40	−0 32
Compound needles	3½ ...		+0 56	+35 44	−0 22	−0 25	−0 21	+0 19	−0 4	+0 12	+0 18
	7½ ...		+0 15	+36 16	+0 1	−1 46	+0 53	+1 16	−0 1	+0 1	+0 5
Compound needles	7½ ...	Soft iron cylinders.	−0 13	+ 0 5	−0 5	−7 48	−0 13	−0 1	−0 1	−0 37	+0 3
Single needle	7½ ...		−0 20	− 0 1	−0 18	−8 3	+0 7	+0 5	+0 1	+2 47	−0 17

The following are briefly the results of the experiments:—

1. Deviations produced by magnets in the same horizontal plane with the compass, and at distances of $18\frac{1}{4}$ and $19\frac{3}{4}$ inches from the centre of the compass:—

With the 3-inch single needle these deviations are nearly semicircular.

With the 6-inch single needle, and still more strikingly with the 12-inch single needle, a large sextantal deviation is introduced, and the semicircular deviation is increased, the sextantal deviation and the increase of the semicircular deviation being proportional approximately to the square of the length of the needle.

A striking difference will be observed in the appearance of the curves of deviations caused by a magnet placed endways and the curves of deviations by a magnet placed sideways. In the first case, the semicircular curve is broadened and flattened by the introduction of the sextantal deviation. This it will be seen arises from the coefficients F and B having the same signs. In the second case the semicircular curve is narrowed and peaked. This arises from the coefficients F and B having different signs.

These peculiarities, as will be seen in the sequel, agree with the mathematical deductions.

In the deviations of the Admiralty Standard compass cards, whether the magnets were placed endways or sideways, the sextantal deviation almost entirely disappeared.

2. Deviations produced by magnets above or below the level of the compass:—

The mathematical investigation shows that when the difference of level is less than half the horizontal distance, the semicircular deviation is increased by increasing the length of the needle, when greater, diminished; and with this the observations agree, the following being the results.

With the 3-inch single needle the deviation was in each case nearly semicircular.

With the 12-inch single needle raised 20 inches above the magnet, when the horizontal distance was $6\frac{1}{2}$ inches, the deviation was nearly semicircular, showing only a very slight tendency to the introduction of a sextantal part. As the horizontal distance increased, the proportion of the sextantal to the semicircular deviation increased.

With an Admiralty Standard compass there was no sextantal deviation.

3. Deviations produced by soft iron at the level of the compass:—

In this case there was no sextantal deviation, but with a 7½-inch single needle there was a large octantal term in addition to the normal quadrantal deviation, while *with an Admiralty Standard compass card the octantal term almost entirely disappeared, leaving only a true quadrantal deviation.*

These results, as to the deviation of single needles, explained the residual error found in the corrected standard compass of the Great Eastern. These errors were evidently partly sextantal and partly octantal, and had been caused by the great length of the needles and by the proximity of the magnets and soft iron correctors, particularly of the latter.

The remarkable features observed in the deviation of the Admiralty Standard compass suggested the idea that the arrangement of the needles in that compass might produce, in the case of deviations caused by a magnet or mass of soft iron in close proximity to it, a compensation of the sextantal and octantal deviations, and this, on the subject being investigated mathematically, proved to be the case, this particular arrangement of needles reducing to zero the coefficients of the terms involving the square of the ratio of the length of the needle to the distance of the disturbing iron; so that this remarkable result was obtained, that the arrangement of needles which produces the equality in the moments of inertia is by a happy coincidence the same as that which prevents the sextantal deviation in the case of correcting magnets, and the octantal deviation in the case of soft iron correctors. The consequence is that, by the employment of Admiralty Standard compass cards, or of cards with two needles each 30° from the central line, correcting magnets and soft iron correctors may be placed much nearer the compass than can safely be done with a single-needle compass card, and that the large deviations found in iron ships may be thus far more accurately corrected.

The mathematical investigation further shows, what we have already adverted to, the advantage, when a magnet is used to correct a large deviation in a single needle, of the magnet being placed as nearly as possible directly above or below the centre of the needle. A magnet so placed has the further advantage of causing no error from heeling.

The mathematical investigation led to another result, which has also been confirmed by experiment, viz. that the sextantal deviation of a single needle caused by a magnet at the same level may be prevented by using, instead of a single magnet, two equal and similar magnets, similarly placed with regard to the needle, but arranged so as to form an equilateral triangle with the centre of the compass. Such a pair of magnets produces a semicircular deviation without any sextantal deviation. But this result, however interesting theoretically, is probably not one which can be made practically useful.

The length of the single needle may also be a cause of error in a different way. The magnetism of one end of the needle induces magnetism in any soft iron near it, which magnetism reacts on and causes a deviation of the needle. This deviation likewise is prevented by the arrangement of the needles in the Admiralty Standard compass.

Before leaving this part of the subject, it is proper to say, with reference to the mode of correcting the compass proposed by Mr. AIRY, that the peculiar errors considered in this paper are of a kind and amount which could hardly have been contemplated by Mr. AIRY. The sextantal error becomes wholly insensible, even with a large semicircular deviation, when the magnets being in the same horizontal plane, are more than six lengths of the needle from the compass, or when being above or below the compass they are considerably within that distance. The octantal errors become insensible when the soft iron correctors are more than two lengths of the needle from the compass. It is not probable that Mr. AIRY contemplated that the correctors would be brought within these distances. It is only with the large semicircular and quadrantal deviations of iron ships, which the compass adjusters of the present day are not afraid to correct by magnets and soft iron brought in close proximity to the compass, and with the needles of extraordinary length, which are considered suitable to ships of extraordinary size, that the errors in question become sensible or material.

New Mode of Correcting the Quadrantal Deviation.

The correction of the quadrantal deviation by Mr. AIRY'S method, although theoretically more perfect than the correction of the semicircular deviation, is practically more embarrassing, When made, it remains perfect, notwithstanding any change in the independent magnetism, or in the geographical position of the ship; but the increased use of iron in the construction even of iron vessels, and perhaps some change in the quality of the iron used, has greatly increased the amount of the quadrantal deviation. From 1° and 1° 6′, the quadrantal deviations of the Rainbow and Ironsides on which Mr. AIRY'S observations were made in 1839, it has increased to an average of 3° or 4° in iron Ships of War, and of 7°or 8° in some iron vessels of the Mercantile Marine. The correction of such deviations by soft iron requires, from the comparative weakness of induced magnetism, the employment of large masses of iron, brought so near the compass that large octantal errors are caused in the single-needle compass, and not wholly avoided by the use of the Admiralty Standard compass, and opening further sources of error in the independent magnetism of the corrector and the magnetism induced in it by the compass-needle.

A mode of correcting the quadrantal deviation by a permanent magnet, which shall furnish the requisite amount of force without being brought into too close proximity to the compass, is therefore a desideratum. Such a correction cannot be obtained from a magnet in a fixed position in the ship, which can only correct deviations proportional to the sines and cosines of odd multiples of the azimuth of the ship's head; but it occurred to Mr. EVANS that it may be obtained from the reciprocal action of two compasses arranged as in an ordinary double binnacle. It is easily shown mathematically that two such compasses of equal strength produce on each other a *negative* quadrantal deviation, together with (in the case of single-needle compasses) a small octantal deviation, without introducing any other error; and as the quadrantal error to be corrected is, in all

cases of iron vessels which have been hitherto examined, *positive* *, the arrangement furnishes a correction obeying the required law. The results of an experiment originally made for the purpose of demonstrating the danger of this arrangement of two compasses, will be found in the well-known work on the 'Deviations of the Compass,' by the late Captain EDWARD JOHNSON, R.N., F.R.S., Superintendent of the Compass Department of the Royal Navy, Table V., 2nd edition, p. 59. In this experiment the compasses were the Admiralty Standard, placed two feet apart, and the effect of the reciprocal action of the compasses on each other was to produce a negative quadrantal deviation in the one of 8° 6′, in the other of 8° 18′, without producing any other appreciable deviation. As the amount of deviation produced is inversely as the cube of their mutual distance, these compasses, at the distance of $2\frac{1}{2}$ feet, would have produced a negative quadrantal deviation on each other of about 4°, and would therefore have corrected the usual amount of quadrantal deviation found in iron ships without the introduction of soft iron correctors.

A similar experiment, made by Mr. EVANS with two single needles of 6 inches in length, placed 1 foot 6 inches apart, gave a negative quadrantal deviation of 6° 40′, with an octantal deviation of +58′. Two such compasses so arranged would therefore correct a quadrantal deviation of the largest class, without the introduction, even with single needles, of a material octantal error, and this octantal error may be corrected by the employment of two compasses, each with two or four needles arranged in the manner before described.

It is perhaps not unworthy of remark, as an incident in the history of this subject, that an experiment which is cited by Captain JOHNSON as a warning against placing compasses near each other as in the ordinary double binnacle, and which was the cause of an Admiralty order that compasses should in no case be brought within $4\frac{1}{2}$ feet of each other, should in the course of time have become the means of correcting an error which the change in the material and mode of construction of ships has brought into prominence.

The defects in this method of correcting the quadrantal deviation appear to be—

1. The two compasses being in different positions, may, particularly in ships built head east and west, have different independent deviations. Some care must therefore be used to select a place at which the magnetism of the ship is nearly constant within the area occupied by the two compasses; and this mode of correction will probably be found more applicable to large ships, in which the magnetism is more uniform, than in small vessels, in which there may be great changes in the magnetism with small changes in position.

2. The possible decrease in the force of the needles. In this respect the defect is common to this, with every mode of correcting deviations by magnets. In the case of the Admiralty Standard compass, the proved permanency of the magnetism is such as to show that this defect may be disregarded.

* See Mr. EVANS's paper, Philosophical Transactions, 1860, p. 337.

3. The effective power of each needle in correcting a quadrantal deviation is inversely proportional to the horizontal force of the earth at the place. A quadrantal deviation completely corrected in England, would therefore reappear to nearly half its amount near the magnetic equator; the correction would, however, become again perfect as the vessel went further to the south. In lower magnetic latitudes, or more accurately when the horizontal force is greater than in the place of correction, the correction, though insufficient, would be beneficial. In higher magnetic latitudes, the quadrantal deviation would be over-corrected. These defects admit of being remedied by a provision for adjusting the mutual distances of the compasses, and it is probably only in very high magnetic latitudes that this mode of correction would have to be abandoned.

On the whole, we venture to anticipate that this mode of correcting the quadrantal deviation will be found of advantage in the case of corrected steering compasses in large iron-built ships.

Mathematical Investigation of the Effect of the Length and Arrangement of the Needles.

In the following part of the paper it has not been thought necessary to give the details of the mathematical operations. Those expressions only are given which are directly applicable in illustration of the experiments described in the preceding part.

In this investigation a bar magnet, and likewise a compass needle, is supposed to consist of two particles of N. and S. magnetism, separated by a finite interval.

Let A be a magnetic particle whose force at the unit of distance on a unit of the opposite magnetism . . . . . $=M$

B be the extremity of a needle whose force . . . . . $=m$

The force of A on B is $\frac{Mm}{\overline{AB}^2}$.

If C be the centre of the needle, the force of A to turn B round C is

$$Mm\frac{BC}{\overline{AB}^3}\sin ABC = Mm\frac{BC.AC}{\overline{AB}^3}\sin ACB.$$

Let $BC=a$, $CA=b$, $AB=c$, $ACB=\zeta'$.

Then, since

$$c^2=a^2+b^2-2ab\cos\zeta'$$

$$\frac{1}{c^3}=\frac{1}{b^3}\left\{1+\frac{9}{4}\frac{a^2}{b^2}+3\frac{a}{b}\cos\zeta'+\frac{15}{4}\frac{a^2}{b^2}\cos 2\zeta'+\&c.\right\},$$

going as far as terms involving $\frac{a^2}{b^2}$.

The force to turn B round C is therefore

$$=Mm\frac{a}{b^2}\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}\right)\sin\zeta'+\frac{3}{2}\frac{a}{b}\sin 2\zeta'+\frac{15}{8}\frac{a^2}{b^2}\sin 3\zeta'\right\}.$$

Let $-m'$ be the force of the other end B′ of the needle, for which $a=-a$.

The force of $-m'$ to turn B′ round C is therefore

$$=\mathrm{M}m'\frac{a}{b^3}\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}\right)\sin\zeta'-\frac{3}{2}\frac{a}{b}\sin 2\zeta'+\frac{15}{8}\frac{a^2}{b^2}\sin 3\zeta'\right\};$$

and the whole force to turn the needle round C,

$$=\mathrm{M}(m+m')\frac{a}{b^3}\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}\right)\sin\zeta'+\frac{3}{2}\frac{a}{b}\frac{m-m'}{m+m'}\sin 2\zeta'+\frac{15}{8}\frac{a^2}{b^2}\sin 3\zeta'\right\}.$$

It appears from this equation that the effect of one end of a bar magnet on a single-needle compass of finite length is,—

1st, to increase the deviation which would be produced in an infinitesimal needle in the proportion of $1+\frac{3}{8}\frac{a^2}{b^2}:1$;

2nd, to cause a sextantal deviation of which the coefficient is $\frac{15}{8}\cdot\frac{a^2}{b^2}$, or five times the increase of the semicircular deviation.

3rd. To cause a quadrantal deviation of which the coefficient is $\frac{3}{2}\cdot\frac{a}{b}\cdot\frac{m-m'}{m+m'}$.

If the two ends of the needle are of equal strength, this term disappears; but if one end is stronger than the other, there will be a small quadrantal deviation, the effect of which will be shown in the diagram by a shifting of the points of intersection of the long needle curves with the short needle curves.

When $m=m'$, the force to turn the needle $2a$ in length about its centre is

$$2\mathrm{M}m\frac{a}{b^3}\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}\right)\sin\zeta'+\frac{15}{8}\frac{a^2}{b^2}\sin 3\zeta'\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

It will easily be found from these formulæ that the force of one end of a magnetic bar to turn two parallel needles placed symmetrically on each side of a diameter, the ends of each being α from the end of the diameter, is

$$=4\mathrm{M}m\frac{a}{b^3}\cos\alpha\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}\right)\sin\zeta'+\frac{15}{8}\frac{a^2}{b^2}\frac{\cos 3\alpha}{\cos\alpha}\sin 3\zeta'\right\}, \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

in which a represents the diameter of the circle of which the needles are respectively chords. The coefficient of the sextantal deviation, having $\cos 3\alpha$ for a factor, will be zero if $3\alpha=90°$ or $\alpha=30°$. Hence, if the two needles are placed with their ends 30° on each side of the diameter, and therefore 60° from each other, there will be no sextantal deviation. This is the simplest form of the Admiralty Standard compass.

If there are two pairs of parallel needles placed symmetrically on each side of a diameter, the ends of the needles being distant α and α' from the extremities of the diameter, the force to turn the card is

$$=4\mathrm{M}m\frac{a}{b^3}\cos\frac{\alpha+\alpha'}{2}\cos\frac{\alpha-\alpha'}{2}\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}\right)\sin\zeta'+\frac{15}{8}\frac{a^2}{b^2}\frac{\cos 3\frac{\alpha+\alpha'}{2}}{\cos\frac{\alpha+\alpha'}{2}}\frac{\cos 3\frac{\alpha-\alpha'}{2}}{\cos\frac{\alpha-\alpha'}{2}}\sin 3\zeta'\right\}. \quad . \quad (3.)$$

The coefficient of the sextantal terms having $\cos 3\frac{\alpha+\alpha'}{2}$ as a factor is zero if $\frac{\alpha+\alpha'}{2}=30°$, or if the ends of the needles be at equal angular distances on each side of the point of 30°. It is therefore zero if one pair of needles be 15° from the diameter, the other at 45° from the diameter, which is the usual construction of the Admiralty Standard compass.

In each case it will be seen that the semicircular deviation is increased by the length of the needle in the proportion of

$$1+\frac{3}{8}\frac{a^2}{b^2}:1.$$

These results hold good if, instead of one magnetic particle, any number of magnetic particles or of magnets act on the compass.

If we go back to equation (1.), and if, instead of finding the effect of a single magnetic particle on two or four needles, we inquire into the effect of two equal magnetic particles at equal distances from the compass, but in different azimuths ζ' and ζ'' on a *single needle*, we find the force to turn the needle to be

$$4\mathrm{M}m\frac{a}{b^2}\cos(\zeta'-\zeta'')\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}\right)\sin\frac{\zeta'+\zeta''}{2}+\frac{15}{8}\frac{a^2}{b^2}\frac{\cos 3\frac{\zeta'-\zeta''}{2}}{\cos\frac{\zeta'-\zeta''}{2}}\sin 3\frac{\zeta'+\zeta''}{2}\right\} \quad . \quad . \quad (4.)$$

The coefficient of the sextantal deviation having $\cos 3\frac{\zeta'-\zeta''}{2}$ for a factor is zero if $\zeta'-\zeta''=60°$; so that two similar bar magnets placed similarly with reference to a single-needle compass, but at azimuths differing 60° from each other, will produce a semicircular deviation, but no sextantal deviation.

If the point M, instead of being in the plane of the needle, be at a height h above it, b being now the distance from the centre of the needle, of the projection of M on the horizontal plane:—

The force to turn a needle of length $2a$ about its centre

$$=\frac{2\mathrm{M}mab}{(b^2+h^2)^{\frac{3}{2}}}\left[\left\{1+\frac{3}{8}\frac{a^2b^2}{(b^2+h^2)^2}\left(1-4\frac{h^2}{b^2}\right)\right\}\sin\zeta+\frac{15}{8}\cdot\frac{a^2b^2}{(b^2+h^2)^3}\sin 3\zeta\right] \quad . \quad . \quad . \quad . \quad (5.)$$

The equation shows that for a given semicircular deviation the sextantal deviation produced by a magnet raised above, or depressed below the level of the compass, is less than that produced by a magnet in the same horizontal plane in the proportion of

$$\frac{b^2}{(b^2+h^2)^2}:\frac{1}{b'^2},$$

in which b' is the horizontal distance of the magnet in the same horizontal plane. But inasmuch as in order to produce the same semicircular deviation we must have

$$\frac{b}{(b^2+h^2)^{\frac{3}{2}}}=\frac{1}{b'^2},$$

the above ratio becomes $b:\sqrt{b^2+h^2}$, which represents the advantage gained in diminishing the sextantal deviation by the correcting magnets being placed as nearly as possible below or above the compass.

This expression, compared with equation (1.), shows that the same arrangement which prevents a sextantal deviation when the magnet is in the same horizontal plane does so when it is elevated or depressed.

The expression for the semicircular deviation shows that it is increased or diminished by an increase of the length of the needle, according as h is less or greater than $\frac{1}{2}b$.

If we desire to know the amount of sextantal deviation produced by a bar magnet on a single needle in the same horizontal plane, we must consider the effect of both ends of the magnet. The expression becomes, however, too complicated for use when the problem is stated generally. In order to simplify the formulæ, we may consider the magnet directed first endways and then sideways to the needle.

1. If the bar magnet be in the same horizontal plane and directed to the needle:— Let b and b' be the distances from the centre of the needle to the two ends of the magnet.

The force to turn the single needle compass is

$$=2\mathrm{M}ma\left(\frac{1}{b^3}-\frac{1}{b'^3}\right)\left[\left\{1+\frac{3}{8}a^2\left(\frac{1}{b^2}+\frac{1}{b'^2}\right)\right\}\sin\zeta'+\frac{15}{8}a^2\left(\frac{1}{b^2}+\frac{1}{b'^2}\right)\sin 3\zeta'\right]. \quad . \quad . \quad . \quad (6.)$$

If the magnet be short this becomes

$$=4\mathrm{M}\frac{ma}{b^3}(b'-b)\left\{\left(1+\frac{3}{4}\frac{a^2}{b^2}\right)\sin\zeta'+\frac{15}{4}\frac{a^2}{b^2}\sin 3\zeta'\right\}.$$

2. If the bar magnet be in the same horizontal plane and directed sideways. Let β be the angle subtended by the half of the magnet.

$$\text{Force}=2\mathrm{M}\frac{ma}{b^3}\sin\beta\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}\right)\sin\zeta'-\frac{15}{8}\frac{a^2}{b^2}\frac{\sin 3\beta}{\sin\beta}\sin 3\zeta'\right\}, \quad . \quad . \quad . \quad . \quad (7.)$$

ζ' being the angle between the needle and a line parallel to the magnet.

If in the last equation the correcting bar be short, β will be small and $\frac{\sin 3\beta}{\sin\beta}=3$, and proportion of the coefficient of the sextantal part to that of the semicircular part will be

$$\frac{45}{8}\frac{a^2}{b^2}:1+\frac{3}{8}\frac{a^2}{b^2}.$$

If $\frac{a}{b}=\frac{1}{12}$, or if the distance of the magnet be six times the length of the needle, this ratio will be $\frac{1}{25{\cdot}7}$. If the semicircular deviation is very large, the sextantal deviation should not be allowed to exceed this proportion, and therefore with single bar needles the correcting compass should not be placed within six lengths of the needle of it.

The sign of the sextantal part being the same as that of the semicircular in equation

(6.), shows that its effect is to lower and broaden the curve; the sign being different in equation (7.) when β is less than 30°, shows that its effect is to heighten and narrow it.

The effect of soft iron correctors on the deviation of the compass, leaving out of consideration for the present the effect on the needle of the magnetism induced by the needle in the soft iron reacting on the needle, will be found by substituting for the constant coefficient M a coefficient $M' \sin \zeta' + M'' \cos \zeta'$, which will represent the magnetic state of the soft iron having magnetism induced in it by the action of the horizontal force of the earth in the different positions of the ship's head.

It will be easily seen, without repeating the calculation, that this will give a constant, a quadrantal, and an octantal deviation instead of a semicircular and a sextantal deviation, as in the case of a bar magnet; and that the octantal deviation will be reduced to zero by the same arrangement of needles in the compound card which reduces the sextantal deviation to zero.

When one end of the needle induces magnetism in soft iron in its neighbourhood, the disturbing force is itself proportional to the force of the needle; so that in the above equations we must substitute $\frac{\lambda m}{AB^3}$ for M, so that the force to turn B round C will be

$$\lambda m^3 \frac{BC}{AB^4} \sin ABC$$

$$= \lambda m^3 \frac{BC.AC}{AB^5} \sin ACB$$

$$= \lambda m^3 \frac{a}{b^4} \left\{ \left(1 + \frac{15}{8} \frac{a^2}{b^2}\right) \sin \zeta' + \frac{5}{2} \frac{a}{b} \sin 2\zeta' + \frac{35}{8} \frac{a^2}{b^2} . \sin 3\zeta' \right\}.$$

The comparison of this formula with those given above will show that the same construction of the compass which prevents a sextantal error arising from the length of the needle when a permanent magnet affects it, prevents the like error when the needle acts on and is reacted on by soft iron.

If we carry the expansion to the fourth power of $\frac{a}{b}$, expression (1) becomes

$$2Mm \frac{a}{b^3} \left\{ \left(1 + \frac{3}{8} \frac{a^2}{b^2} + \frac{15}{64} \frac{a^4}{b^4}\right) \sin \zeta' + \left(\frac{15}{8} \frac{a^2}{b^2} + \frac{105}{128} \frac{a^4}{b^4}\right) \sin 3\zeta' + \frac{315}{128} \frac{a^4}{b^4} \sin 5\zeta' \right\}.$$

Expression (3) becomes

$$4Mm \frac{a}{b^3} \cos \frac{\alpha+\alpha'}{2} \cos \frac{\alpha-\alpha'}{2} \left\{ \left(1 + \frac{3}{8} \frac{a^2}{b^2} + \frac{15}{64} \frac{a^4}{b^4}\right) \sin \zeta' \right.$$

$$\left. + \left(\frac{15}{8} \frac{a^2}{b^2} + \frac{105}{128} \frac{a^4}{b^4}\right) \frac{\cos 3 \frac{\alpha+\alpha'}{2} \cos 3 \frac{\alpha-\alpha'}{2}}{\cos \frac{\alpha+\alpha'}{2} \cos \frac{\alpha-\alpha'}{2}} \sin 3\zeta' + \frac{315}{128} \frac{a^4}{b^4} \frac{\cos 5 \frac{\alpha+\alpha'}{2} \cos 5 \frac{\alpha-\alpha'}{2}}{\cos \frac{\alpha+\alpha'}{2} \cos \frac{\alpha-\alpha'}{2}} \sin 5\zeta' \right\};$$

so that if $\frac{\alpha-\alpha'}{2} = 18°$, or if one pair of needles be 12° from the diameter, the other 48°, the term involving $\sin 5\zeta'$, which may be called the decantal term, will also vanish.

If, as in the Admiralty compass, $\frac{\alpha-\alpha'}{2}=15°$, the factor $\frac{\cos 5\frac{\alpha+\alpha'}{2}\cos 5\frac{\alpha-\alpha'}{2}}{\cos\frac{\alpha+\alpha'}{2}\cos\frac{\alpha-\alpha'}{2}}$ becomes $-\tan 15°$, or the decantal term is reduced to about one-fourth of the amount produced by a single needle.

Expression (6) becomes

$$2\mathrm{M}ma\left(\frac{1}{b^3}-\frac{1}{b'^3}\right)\left[\left\{1+\frac{3}{8}a^2\left(\frac{1}{b^2}+\frac{1}{b'^2}\right)+\frac{15}{64}a^4\left(\frac{1}{b^4}+\frac{1}{b^2b'^2}+\frac{1}{b'^4}\right)\right\}\sin\zeta'\right.$$
$$\left.+\left\{\frac{15}{8}a^2\left(\frac{1}{b^3}+\frac{1}{b'^3}\right)+\frac{105}{128}a^4\left(\frac{1}{b^4}+\frac{1}{b^3}\frac{1}{b'^3}+\frac{1}{b'^4}\right)\right\}\sin 3\zeta'+\frac{315}{128}a^4\left(\frac{1}{b^4}+\frac{1}{b^2b'^2}+\frac{1}{b'^4}\right)\sin 5\zeta'\right].$$

If the magnet be short this becomes

$$=4\mathrm{M}ma\,\frac{(b'-b)}{b^3}\left\{\left(1+\frac{3}{4}\frac{a^2}{b^2}+\frac{45}{64}\frac{a^4}{b^4}\right)\sin\zeta'+\left(\frac{15}{4}\frac{a^2}{b^2}+\frac{315}{128}\frac{a^4}{b^4}\right)\sin 3\zeta'+\frac{945}{128}\frac{a^4}{b^4}\sin 5\zeta'\right\}.$$

Expression (7) becomes

$$2\mathrm{M}m\,\frac{a}{b^3}\sin\beta\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}+\frac{15}{64}\frac{a^4}{b^4}\right)\sin\zeta'-\left(\frac{15}{8}\frac{a^2}{b^2}+\frac{105}{128}\frac{a^4}{b^4}\right)\frac{\sin 3\beta'}{\sin\beta}\sin 3\zeta'+\frac{315}{128}\frac{a^4}{b^4}\frac{\sin 5\beta}{\sin\beta}\sin 5\zeta'\right\},$$

which, if β be small, becomes

$$2\mathrm{M}m\,\frac{a}{b^3}\sin\beta\left\{\left(1+\frac{3}{8}\frac{a^2}{b^2}+\frac{15}{64}\frac{a^4}{b^4}\right)\sin\zeta'-\left(\frac{45}{8}\frac{a^2}{b^2}+\frac{315}{128}\frac{a^4}{b^4}\right)\sin 3\zeta'+\frac{1575}{128}\frac{a^4}{b^4}\sin 5\zeta'\right\}.$$

Hence if the deviation produced by two short needles at equal distances, placed sideways on the east and west side of the needle, be corrected by one short magnet of the same kind placed on the north side and directed endways, the residual error will be

$$\text{Dev.}\left\{\left(\frac{3}{8}\frac{a^2}{b^2}+\frac{15}{32}\frac{a^4}{b^4}\right)\sin\zeta'+\left(\frac{75}{8}\frac{a^2}{b^2}+\frac{315}{64}\frac{a^4}{b^4}\right)\sin 3\zeta'-\frac{315}{64}\frac{a^4}{b^4}\sin 5\zeta'\right\};$$

so that by such an arrangement the sextantal and decantal errors may be obtained almost freed from the semicircular error, and the effect of different arrangements of needles in diminishing these errors more easily tested.

These results, it will be observed, suppose the magnetism of the needles to be collected in one point at each extremity; but as the points in which the magnetism may with least error be considered as collected lie a short distance from the extremities, and therefore must be considered as lying at greater angular distances from the central line than the extremities, the consequence is that in the Admiralty Standard compass cards the sextantal and octantal errors are slightly over-corrected; and the accuracy of the correction might doubtless be increased, with little or no injury to the performance of the compass in other respects, by bringing the needles a little within the regulated distances of 30° and 60°.

Mathematical Investigation of the Effect of Two Needles on each other.

1. The investigation of the deviation produced by two needles of equal size and power

on each other is not difficult. Such needles, if otherwise acted on by the same forces, will remain parallel to each other. Let AB A′B′ represent the needles, a their common length, b their mutual distance, and m their common force.

The force of each to turn the other will be

$$-m\frac{ab^2}{2}\cos\zeta'\left\{\frac{1}{\overline{AB'}^3}-\frac{1}{\overline{A'B'}^3}\right\};$$

and if H be the horizontal force of the earth, they will produce a deviation

$$\delta=-\frac{m}{H}\cdot\frac{b}{2}\cos\zeta'\left\{\frac{1}{\overline{AB'}^3}-\frac{1}{\overline{A'B'}^3}\right\}$$

$$=-\frac{3}{2}\frac{m}{H}\frac{a}{b^3}\left\{\left(1+\frac{5}{12}\frac{a^2}{b^2}\right)\sin 2\zeta'+\frac{35}{24}\frac{a^2}{b^2}\sin 4\zeta'\right\}.$$

If one of these needles be directed towards the other, and the binnacle be moved in azimuth till the needles are at right angles to each other, the deviation produced in the second needle will be

$$-2\frac{m}{H}\frac{a}{b^3}\left\{1-\frac{1}{4}\frac{a^2}{b^2}\right\}.$$

The quadrantal coefficient, or the greatest deviation which the needles will produce on each other when left free, is therefore three-fourths of the greatest deviation which one of the needles can cause in the other when placed in the most favourable position.

2. The proportion of the octantal term introduced to the quadrantal is

$$\frac{\frac{35}{24}\frac{a^2}{b^2}}{1+\frac{5}{4}\frac{a^2}{b^2}}.$$

If $\frac{a}{b}=\frac{1}{3}$ this term is $=\frac{35}{226}=\frac{1}{6\cdot5}$, so that with a quadrantal deviation of 6° 30′ this should introduce an octantal deviation of 1°. We may therefore safely fix three times the length of the needle as the limit of distance within which single-needle compasses should not be allowed to approach each other in order to avoid the octantal error. It will be observed that this theoretical result coincides, as nearly as possible, with the experiment which gave for a quadrantal deviation of 6° 41′, produced by two needles at a distance of three times their length, an octantal deviation of 0° 58′.

3. If instead of two single-needle compasses we have the reciprocal action of two double-needle compasses, the distances of the ends from the diameter which is parallel to the needles being α, the deviation produced is

$$-3\frac{ma\cos\alpha}{Hb^3}\left\{\left(1+\frac{5}{12}\frac{a^2}{b^2}\cos^2\alpha\right)\sin 2\zeta'-\frac{35}{24}\frac{a^2}{b^2}\frac{\cos 3\alpha}{\cos\alpha}\sin 4\zeta'\right\};$$

so that the octantal term is made to vanish by the same arrangement of two needles that we have already described.

Conclusions.

The conclusions which we draw from these investigations are the following:—

I. All mechanically corrected compasses should have compound needles arranged as in the Admiralty Standard compass, or two parallel needles the extremities of which are 60° distant from each other.

If the needles are rather nearer each other than the distance given by the above rule, the correction of the sextantal and octantal errors will be rather more perfect without injuring the quality of the compass in other respects. If the longer needles in the Admiralty Standard compass are a little nearer each other, the decantal and dodecantal errors will be likewise more perfectly corrected.

II. If a single-needle compass be corrected, the following rules should be attended to:—

1. The needle should not be more than 6 or 7 inches long.

2. A magnet at the same level should not be nearer the centre of the needle than six lengths of the needle.

3. Let the last-mentioned distance be called the "nearest horizontal distance;" then if the magnet be below the level of the compass it may be brought nearer the compass, but not nearer than the foot of the perpendicular dropped from the "nearest horizontal distance" on the line joining the centre of the needle and the magnet. So that if this direction makes an angle of 30° with the vertical, the magnet should not come nearer than three lengths of the needle.

4. If possible no soft iron correctors should be brought within two lengths of the needle from the centre of the needle, and on no account within one and a half length.

III. In correcting the quadrantal deviation by the reciprocal action of two compasses, the following rules are to be attended to:—

The two compasses are to be placed, as in the common double binnacle, at a distance from each other, to be determined in a manner to be described.

A place must be selected for the double binnacle, such that no iron will be very near the compasses, in order that the independent deviations of the two compasses may be the same.

The ship must be swung so as to ascertain the amount of the quadrantal deviation in the positions of the double binnacle before the operation of correction is commenced, experience showing that, except in the immediate neighbourhood of masses of iron, the quadrantal deviation is nearly the same in all the positions in which a compass could be placed.

The compass-maker should have previously matched his compasses in pairs, the compasses of each pair being the same in all respects, and having the same power. He should also have ascertained, by previous trial in his workshop, with the compasses placed so as to bear N.E. and S.W. and N.W. and S.E. from each other (by the disturbed needles), and should have marked on a scale to accompany the compasses, the distances from each other at which they produce deviations of 2°, 3°, 4°, 5°, 6°, 7°, 8°.

The quadrantal deviation of the ship being known, the compasses should be fixed at that distance apart at which they produce the deviation required to be corrected.

The semicircular deviation is then to be corrected in the usual way, except that the semicircular deviations of both compasses may be corrected by the same two magnets, placed respectively fore-and-aft and athwart-ship, and half-way between the two compasses.

The adjustment of the compasses may be made with less preliminary observation, but with more calculation, in the following way:—

For each compass, let the amount of deflection which it will produce on a test-needle at a distance of, say, 1 foot 6 inches when directed towards the centre of the test-needle and at right-angles to it, be ascertained and recorded.

A pair of such needles will produce on each other at a distance of 1 foot 6 inches a quadrantal deviation of three-fourths of the recorded amount, and at any other distance a deviation which bears to three-fourths of the recorded deviation the proportion which the cube of 18 bears to the cube of the number of inches required.

In conclusion, we would urge most strongly the importance of selecting for the position of the compass or compasses a place where the deviation is moderate in amount, and nearly uniform for some distance around the compasses. No compass should be placed in a position in which the original deviation in England much exceeds two points.

Appendix I.

Deviations of the Standard Compass of the Steam-ship Great Eastern, after compensation with magnets and soft iron.

Ship's head by standard compass.	Holyhead, 23rd to 24th October, 1859.	Southampton, 12th to 15th June, 1860.
	° ′	° ′
North.		
N by E.		
N.N.E.	0 0	
N.E by N.	1 0 W.	
N.E.	2 20 W.	
N.E by E.	3 45 W.	
E.N.E.	4 45 W.	
E by N.	4 15 W.	
East.	2 30 W.	
E by S.	0 0	
E.S.E.	2 0 E.	
S.E by E.	2 0 E.	
S.E.	1 0 E.	
S.E by S.	0 20 W.	
S.S.E.	1 40 W.	
S by E.		1 20 E.
South.		1 15 E.
S by W.		0 0
S.S.W.		1 45 E.
S.W by S.		2 0 E.
S.W.		0 0
S.W by W.		4 0 W.
W.S.W.		5 20 W.
W by S.		3 0 W.
West.		1 30 E.
W by N.	4 15 E.	4 45 E.
W.N.W.	4 25 E.	6 0 E.
N.W by W.	3 30 E.	5 40 E.
N.W.	1 30 E.	4 0 E.
N.W by N.	0 40 W.	1 0 E.
N.N.W.	2 10 W.	
N by W.		

Note.—The points of the compass left blank are those on which no observation could be made, from the direction of the wind, on the days of observation, not allowing the ship to swing to these points.

Appendix II.

Experiments, Magnet Endways. (Plate V.)

Compass-needles placed on a fixed pivot in centre of revolving table; south pole of a 12-inch bar magnet placed 18·25 inches from the pivot, 2·75 inches below its level.

Magnetic direction of point opposite bar magnet.	Deviation of Single needles.		
	3 inches.	6 inches.	12 inches.
North.	0 0	0 10 E.	2 50 E.
N by E.	12 20 W.	14 0 W.	22 30 W.
N.N.E.	20 55 „	22 15 „	25 25 „
N.E.	29 45 „	29 5 „	25 55 „
E.N.E.	30 35 „	30 0 „	26 10 „
East.	27 20 „	27 35 „	27 50 „
E.S.E.	21 55 „	22 55 „	27 10 „
S.E.	14 50 „	16 15 „	20 45 „
S.S.E.	7 30 „	8 25 „	10 50 „
South.	0 0	0 0	0 0
S.S.W.	7 50 E.	8 15 E.	11 0 E.
S.W.	14 55 „	15 40 „	20 15 „
W.S.W.	21 25 „	22 0 „	26 5 „
West.	26 30 „	26 40 „	26 45 „
W.N.W.	29 10 „	28 30 „	25 20 „
N.W.	28 30 „	27 50 „	25 30 „
N.N.W.	20 30 „	21 55 „	25 10 „
N by W.	12 10 „	14 5 „	22 30 „

Compass-needles placed on a fixed pivot in centre of revolving table; south pole of a 12-inch bar magnet placed 18 inches from the pivot, 1 inch below level.

Magnetic direction of point opposite bar magnet.	Deviation of Compound needles.	
	3½ inches.	7½ inches.
North.	1 0 W.	0 0
N by E.	15 35 „	15 55 W.
N.N.E.	25 50 „	27 55 „
N.E.	36 10 „	36 55 „
E.N.E.	34 30 „	35 55 „
East.	29 50 „	31 10 „
E.S.E.	22 30 „	24 15 „
S.E.	15 50 „	16 35 „
S.S.E.	8 5 „	8 30 „
South.	0 10 E.	0 0
S.S.W.	8 20 „	8 30 E.
S.W.	15 35 „	16 40 „
W.S.W.	24 10 „	24 5 „
West.	30 20 „	31 15 „
W.N.W.	35 50 „	35 40 „
N.W.	36 20 „	36 40 „
N.N.W.	26 20 „	27 10 „
N by W.	16 35 „	16 25 „

Note.—*These experiments were arranged to represent the deviation of an iron ship having a negative semicircular deviation.*

Experiments, Magnet Sideways. (Plate V.)

Compass-needles placed on a fixed pivot in centre of revolving table; a 12-inch bar magnet placed on the table at right angles to the line drawn from its centre to the pivot; the distance from the centre to the pivot 19·75 inches, and on same level.

Magnetic direction of neutral line.	Deviation of Single needles.		
	3 inches.	6 inches.	12 inches.
North.	1 0 W.	0 45 W.	0 0
N.N.E.	5 0 E.	4 45 E.	1 45 E.
N.E.	11 10 „	10 15 „	6 10 „
E.N.E.	17 5 „	16 40 „	13 55 „
East.	22 20 „	22 50 „	23 10 „
E.S.E.	25 15 „	27 0 „	33 0 „
S.E.	24 5 „	25 30 „	34 20 „
S.E by S.	21 0 „	20 30 „	12 50 „
S.S.E.	16 0 „	13 50 „	5 10 „
South.	1 50 E.	2 0 E.	2 5 E.
S.S.W.	12 40 W.	10 0 W.	0 0
S.W by S.	18 50 „	17 30 „	5 45 W.
S.W.	23 0 „	24 15 „	40 0 „
W.S.W.	25 30 „	27 45 „	36 0 „
West.	23 15 „	23 55 „	25 25 „
W.N.W.	18 35 „	17 55 „	15 5 „
N.W.	13 0 „	11 50 „	6 50 „
N.N.W.	7 0 „	5 50 „	1 40 „

Compass-needles placed on a fixed pivot in centre of revolving table; a 12-inch bar magnet placed on the table at right angles to the line drawn from its centre to the pivot; the distance from the centre to the pivot 17 inches, and on same level.

Magnetic direction of neutral line.	Deviation of Compound needles.	
	3½ inches.	7½ inches.
North.	0 30 E.	0 30 E.
N.N.E.	8 50 „	8 50 „
N.E.	17 0 „	17 0 „
E.N.E.	24 30 „	24 10 „
E.S.E.	32 0 „	30 0 „
S.E.	36 0 „	33 50 „
S.S.E.	37 25 „	35 30 „
S.S.E.	30 30 „	32 10 „
S by E.	18 45 „	26 15 „
South.	3 0 E.	6 40 E.
S by W.	14 35 W.	21 45 W.
S.S.W.	27 30 „	30 50 „
S.W.	33 40 „	36 0 „
W.S.W.	33 40 „	35 0 „
West.	30 0 „	30 40 „
W.N.W.	23 50 „	24 30 „
N.W.	16 30 „	17 0 „
N.N.W.	8 10 „	8 20 „

APPENDIX II. (continued.)

Experiments with Soft iron Cylinders (with hemispherical ends) directed towards centre of Compass. (Plate V.)

Ends of Cylinders placed 4½ inches from end of needles, or in same position as adopted for correcting the quadrantal deviation of an Iron ship.

Length of Cylinders, including ends, 12 inches, diameter 3 inches.

Magnetic direction of point 90° from cylinders.	Deviation of compound needles, card 7·6 inches.	Deviation of single bar needle 7·5 inches.
	° ′	° ′
North.	0 0	0 20 W.
N. by E.	5 10 W.	1 40 „
N.N.E.	6 50 „	4 10 „
N.E by N.	8 0 „	6 50 „
N.E.	7 35 „	9 0 „
N.E by E.	6 40 „	9 10 „
E.N.E.	4 30 „	8 0 „
E by N.	2 20 „	4 30 „
East.	0 0	0 50 W.
E by S.	2 5 E.	3 15 E.
E.S.E.	3 50 „	7 10 „
S.E by E.	5 45 „	8 45 „
S.E.	7 10 „	8 40 „
S.E by S.	7 45 „	7 25 „
S.S.E.	7 10 „	4 45 „
S by E.	5 15 „	1 45 „
South.	0 0	0 0
S by W.	5 5 W.	1 30 W.
S.S.W.	6 40 „	4 0 „
S.W by S.	7 35 „	6 15 „
S.W.	7 20 „	8 40 „
S.W by W.	6 45 „	9 20 „
W.S.W.	4 40 „	8 0 „
W by S.	2 35 „	4 35 „
West.	0 0	0 30 W.
W by N.	2 5 E.	4 0 E.
W.N.W.	3 45 „	6 50 „
N.W by W.	6 30 „	8 50 „
N.W.	7 20 „	8 20 „
N.W by N.	7 55 „	6 50 „
N.N.W.	7 30 „	3 20 „
N by W.	5 5 „	1 40 „

POSTSCRIPT.

Since this paper was read to the Royal Society, Mr. EVANS has observed the deviations of the corrected Standard compass of the Great Eastern at Milford. The position of the compass had been altered, but without any new adjustment being made.

The maximum deviation found was 7° 50′. The coefficients of deviation obtained were

A.	B.	C.	D.	E.	F.	G.	H.	K.
+0° 38′	−1° 53′	−2° 55′	−2° 28′	+0° 48′	+0° 12′	+0° 4′	+3° 16′	−0° 4′

Probably nearly all these errors would have been avoided if the Admiralty Standard card had been used; for it is to be observed that the introduction of the errors depending on the length of the needle not only affects the deviations directly, but affects them indirectly, by disguising and preventing the proper correction of the semicircular and quadrantal errors.

Since this paper was read we have also made the following experiment:—

Two 6-inch bar magnets were placed at distances of 1 ft. $1\frac{3}{4}$ in. to the east and west of the centre of the needle, the north end of each bar magnet being directed to the north. Three similar bar magnets tied together were placed with their centres at a distance of 1 ft. $5\frac{3}{4}$ in. north of the centre of the needle, and with their ends directed to the north. In this position they were found to correct, as nearly as possible, the semicircular deviation caused by the first two bars. The following Table gives the results of observation on needles of different kinds. In this experiment the superiority of the Admiralty Standard compass card to the others was very marked.

Correct magnetic direction of bar magnet.	3-inch single needle.	6-inch single needle.	12-inch single needle.	2-needle card, each needle $5\frac{5}{8}$ inches.	2-needle card, each needle $8\frac{1}{2}$ inches.	Admiralty Standard compass card.
North.	0° 10′ W.	0° 10′ E.	0° 5′ E.	0° 10′ W.	0° 10′ E.	0° 45′ W.
N. 30° E.	0 15 E.	2 45 E.	10 55 E.	1 10 W.	0 10 W.	0 45 W.
N. 60° E.	0 25 W.	0 10 W.	7 35 E.	0 20 W.	0 15 W.	0 40 W.
East.	1 5 W.	3 15 W.	11 45 W.	1 10 E.	2 0 E.	0 25 W.
S. 60° E.	0 0	0 15 E.	1 50 E.	0 10 E.	0 0	0 20 W.
S. 30° E.	0 45 E.	3 10 E.	11 45 E.	0 0	1 10 E.	0 10 E.
South.	0 10 W.	0 25 W.	2 0 W.	0 15 E.	1 0 E.	0 0
S. 30° W.	1 25 W.	3 30 W.	11 30 W.	1 5 E.	1 0 E.	0 0
S. 60° W.	0 25 W.	0 10 W.	1 0 W.	0 10 E.	1 0 E.	0 0
West.	0 30 E.	3 0 E.	12 45 E.	1 0 W.	0 50 W.	0 10 W.
N. 60° W.	0 5 W.	0 25 W.	9 35 W.	0 0	0 55 E.	0 10 W.
N. 30° W.	1 0 W.	3 10 W.	10 45 W.	0 40 E.	0 40 E.	0 20 W.

From these values the following are the coefficients obtained.

	A.	B.	C.	D.	E.	F.	G.	H.	K.	L.	M.
3 inch single needle...	−0° 16′	0° 2′	0° 1′	−0° 15′	0° 3′	0° 50′	0° 1′	0° 19′	0° 5′	0° 1′	0° 0′
6 „ „ „	−0 8	0 6	0 2	−0 7	−0 6	3 8	0 12	−0 5	0 0	−0 5	0 3
12 „ „ „	0 1	2 32	−0 14	1 52	−0 1	11 34	0 40	−2 28	0 14	−3 13	−0 12
$5\frac{5}{8}$ „ double needle...	0 4	0 3	−0 21	−0 9	0 2	−0 38	0 3	−0 4	−0 1	0 10	0 7
$8\frac{1}{2}$ „ „ „	0 33	0 9	−0 24	−0 10	0 5	−0 52	−0 5	−0 7	0 2	0 43	0 4
Admiralty Standard ...	−0 17	−0 6	−0 21	−0 5	0 0	−0 10	−0 3	−0 3	−0 3	0 3	0 1

X. *Liquid Diffusion applied to Analysis.*

By THOMAS GRAHAM, *F.R.S., Master of the Mint.*

Received May 8,—Read June 13, 1861.

THE property of volatility, possessed in various degrees by so many substances, affords invaluable means of separation, as is seen in the ever-recurring processes of evaporation and distillation. So similar in character to volatility is the Diffusive power possessed by all liquid substances, that we may fairly reckon upon a class of analogous analytical resources to arise from it. The range also in the degree of diffusive mobility exhibited by different substances appears to be as wide as the scale of vapour tensions. Thus hydrate of potash may be said to possess double the velocity of diffusion of sulphate of potash, and sulphate of potash again double the velocity of sugar, alcohol, and sulphate of magnesia. But the substances named belong all, as regards diffusion, to the more "volatile" class. The comparatively "fixed" class, as regards diffusion, is represented by a different order of chemical substances, marked out by the absence of the power to crystallize, which are slow in the extreme. Among the latter are hydrated silicic acid, hydrated alumina, and other metallic peroxides of the aluminous class, when they exist in the soluble form; with starch, dextrin and the gums, caramel, tannin, albumen, gelatine, vegetable and animal extractive matters. Low diffusibility is not the only property which the bodies last enumerated possess in common. They are distinguished by the gelatinous character of their hydrates. Although often largely soluble in water, they are held in solution by a most feeble force. They appear singularly inert in the capacity of acids and bases, and in all the ordinary chemical relations. But, on the other hand, their peculiar physical aggregation with the chemical indifference referred to, appears to be required in substances that can intervene in the organic processes of life. The plastic elements of the animal body are found in this class. As gelatine appears to be its type, it is proposed to designate substances of the class as *colloids*, and to speak of their peculiar form of aggregation as the *colloidal condition of matter*. Opposed to the colloidal is the crystalline condition. Substances affecting the latter form will be classed as *crystalloids*. The distinction is no doubt one of intimate molecular constitution.

Although chemically inert in the ordinary sense, colloids possess a compensating activity of their own arising out of their physical properties. While the rigidity of the crystalline structure shuts out external impressions, the softness of the gelatinous colloid partakes of fluidity, and enables the colloid to become a medium for liquid diffusion, like water itself. The same penetrability appears to take the form of cementation in

such colloids as can exist at a high temperature. Hence a wide sensibility on the part of colloids to external agents. Another and eminently characteristic quality of colloids, is their mutability. Their existence is a continued metastasis. A colloid may be compared in this respect to water while existing liquid at a temperature under its usual freezing-point, or to a supersaturated saline solution. Fluid colloids appear to have always a *pectous** modification; and they often pass under the slightest influences from the first into the second condition. The solution of hydrated silicic acid, for instance, is easily obtained in a state of purity, but it cannot be preserved. It may remain fluid for days or weeks in a sealed tube, but is sure to gelatinize and become insoluble at last. Nor does the change of this colloid appear to stop at that point. For the mineral forms of silicic acid, deposited from water, such as flint, are often found to have passed, during the geological ages of their existence, from the vitreous or colloidal into the crystalline condition (H. Rose). The colloidal is, in fact, a dynamical state of matter; the crystalloidal being the statical condition. The colloid possesses ENERGIA. It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality. To the gradual manner in which colloidal changes take place (for they always demand time as an element), may the characteristic protraction of chemico-organic changes also be referred.

A simple and easily applicable mode of effecting a diffusive separation is to place the mixed substance under a column of water, contained in a cylindrical glass jar of 5 or 6 inches in depth. The mixed solution may be conducted to the bottom of the jar by the use of a fine pipette, without the occurrence of any sensible intermixture. The spontaneous diffusion, which immediately commences, is allowed to go on for a period of several days. It is then interrupted by siphoning off the water from the surface in successive strata, from the top to the bottom of the column. A species of cohobation has been the consequence of unequal diffusion, the most rapidly diffusive substance being isolated more and more as it ascended. The higher the water column, sufficient time being always given to enable the most diffusive substance to appear at the summit, the more completely does a portion of that substance free itself from such other less diffusive substances as were originally associated with it. A marked effect is produced even where the difference in diffusibility is by no means considerable, such as the separation of chloride of potassium from chloride of sodium, of which the relative diffusibilities are as 1 to 0·841. Supposing a third metal of the potassium group to exist, standing above potassium in diffusibility as potassium stands above sodium, it may be safely predicated that the new metal would admit of being separated from the other two metals by an application of the jar-diffusion above described.

A certain property of colloid substances comes into play most opportunely in assisting diffusive separations. The jelly of starch, that of animal mucus, of pectin, of the vege-

* Πηκτὸς, *curdled*. As fibrin, casein, albumen. But certain liquid colloid substances are capable of forming a jelly and yet still remain liquefiable by heat and soluble in water. Such is gelatine itself, which is not pectous in the condition of animal jelly; but may be so as it exists in the gelatiferous tissues.

table gelose of PAYEN, and other solid colloidal hydrates, all of which are, strictly speaking, insoluble in cold water, are themselves permeable when in mass, as water is, by the more highly diffusive class of substances. But such jellies greatly resist the passage of the less diffusive substances, and cut off entirely other colloid substances like themselves that may be in solution. They resemble animal membrane in this respect. A mere film of the jelly has the separating effect. Take for illustration the following simple experiment.

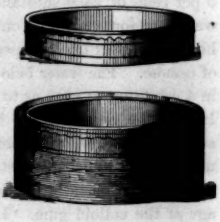
A sheet of very thin and well-sized letter paper, of French manufacture, having no porosity, was first thoroughly wetted and then laid upon the surface of water contained in a small basin of less diameter than the width of the paper, and the latter depressed in the centre so as to form a tray or cavity capable of holding a liquid. The liquid placed upon the paper was a mixed solution of cane-sugar and gum-arabic, containing 5 per cent. of each substance. The pure water below and the mixed solution above were therefore separated only by the thickness of the wet sized paper. After twenty-four hours the upper liquid appeared to have increased sensibly in volume, through the agency of osmose. The water below was found now to contain three-fourths of the whole sugar, in a condition so pure as to crystallize when the liquid was evaporated on a water-bath. Indeed the liquid of the basin was only in the slightest degree disturbed by sub-acetate of lead, showing the absence of all but a trace of gum. Paper of the description used is sized by means of starch. The film of gelatinous starch in the wetted paper has presented no obstacle to the passage of the crystalloid sugar, but has resisted the passage of the colloid gum. I may state at once what I believe to be the mode in which this takes place.

The sized paper has no power to act as a filter. It is mechanically impenetrable, and denies a passage to the mixed fluid as a whole. Molecules only permeate this septum, and not masses. The molecules also are moved by the force of diffusion. But the water of the gelatinous starch is not directly available as a medium for the diffusion of either the sugar or gum, being in a state of true chemical combination, feeble although the union of water with starch may be. The hydrated compound itself is solid, and also insoluble. Sugar, however, with all other crystalloids, can separate water, molecule after molecule, from any hydrated colloid, such as starch. The sugar thus obtains the liquid medium required for diffusion, and makes its way through the gelatinous septum. Gum, on the other hand, possessing as a colloid an affinity for water of the most feeble description, is unable to separate that liquid from the gelatinous starch, and so fails to open the door for its own passage outwards by diffusion.

The separation described is somewhat analogous to that observed in a soap-bubble inflated with a gaseous mixture composed of carbonic acid and hydrogen. Neither gas, as such, can penetrate the water-film. But the carbonic acid, being soluble in water, is condensed and dissolved by the water-film, and so is enabled to pass outwards and reach the atmosphere; while hydrogen, being insoluble in water, or nearly so, is retained behind within the vesicle.

It may perhaps be allowed to me to apply the convenient term *dialysis* to the method of separation by diffusion through a septum of gelatinous matter. The most suitable of all substances for the dialytic septum appears to be the commercial material known as vegetable parchment or parchment-paper, which was first produced by M. Gaine, and is now successfully manufactured by Messrs. De la Rue. This is unsized paper, altered by a short immersion in sulphuric acid, or in chloride of zinc, as proposed by Mr. T. Taylor. Paper so metamorphosed acquires considerable tenacity, as is well known; and when wetted it expands and becomes translucent, evidently admitting of hydration. A slip of 25 inches in length was elongated 1 inch in pure water, and 1·2 inch in water containing one per cent. of carbonate of potash. In the wetted state parchment-paper can easily be applied to a light hoop of wood, or better, to a hoop made of sheet gutta percha, 2 inches in depth and 8 or 10 inches in diameter, so as to form a vessel like a sieve in form (fig. 1). The disc of parchment-paper used should exceed in diameter the hoop to be covered by 3 or 4 inches, so as to rise well round the hoop. It may be bound to the hoop by string, or by an elastic band, but should not be firmly secured. The parchment-paper must not be porous. Its soundness will be ascertained by sponging the upper surface with pure water, and then observing that no wet spots show themselves on the opposite side. Such defects may be remedied by applying liquid albumen, and then coagulating the same by heat. Mr. De la Rue recommends the use of albumen in cementing parchment-paper, which thus may be formed into cells and bags very useful in dialytic experiments. The mixed fluid to be dialysed is poured into the hoop upon the surface of the parchment-paper to a small depth only, such as half an inch. The vessel described (*dialyser*) is then floated in a basin containing a considerable volume of water, in order to induce the egress of the diffusive constituents of the mixture. Half a litre of urine, dialysed for twenty-four hours, gave its crystalloidal constituents to the external water. The latter, evaporated by a water-bath, yielded a white saline mass. From this mass urea was extracted by alcohol in so pure a condition as to appear in crystalline tufts upon the evaporation of the alcohol.

Fig. 1.—Hoop Dialyser.



1. *Jar-diffusion.*

The mode of diffusing more lately followed, which I have already alluded to as jar-diffusion, is extremely simple, and gives results of more precision than could possibly be anticipated. The salt is allowed to rise from below into a cylindrical column of water, and after a fixed time, the proportion of salt which has risen to various heights in the column is observed. The water was contained in a plain cylindrical glass jar, of about 152 millimetres (6 inches) in height and 87 millimetres (3·45 inches) in width. In operating, seven-tenths of a litre of water were first placed in the jar, and then one-tenth

of a litre of the liquid to be diffused was carefully conveyed to the bottom of the jar by means of a fine pipette. The whole fluid column then measured 127 millimetres (5 inches) in height. So much as five or six minutes of time were occupied in emptying the pipette at the bottom of the jar, and extremely little disturbance was occasioned in the superincumbent water, as could be distinctly seen when the liquid introduced by the pipette was coloured. The jar was then left undisturbed, to allow diffusion to proceed; the experiments being always conducted in an apartment of constant, or nearly constant temperature. When a certain time had elapsed, the diffusion was interrupted by drawing off the liquid from the top, by means of a small siphon, slowly and deliberately as the liquid had been first introduced, in portions of 50 cubic centimetres, or one-sixteenth of the whole volume. The open end of the short limb of the siphon was kept in contact with the surface of the liquid in the jar, and the portion of liquid drawn off was received in a graduated measure. By evaporating each fraction separately, the quantity of salt which had risen into equal sections of the liquid column was ascertained. From the bottom of two jars, A and B for instance, a 10 per cent. solution of chloride of sodium was diffused for a period of fourteen days. The whole quantity of salt present in each jar was 10 grammes, which was found at the end to be distributed as follows in the different sectional strata of fluid, numbering them from the top downwards:—

In the first or highest stratum, 0·103 and 0·105 gramme of salt in A and B respectively; in the second stratum, 0·133 and 0·125; in the third stratum, 0·165 and 0·158; in the fourth stratum, 0·204 and 0·193; in the fifth stratum, 0·273 and 0·260; in the sixth stratum, 0·348 and 0·332; in the seventh stratum, 0·440 and 0·418; in the eighth stratum, 0·545 and 0·525; in the ninth stratum, 0·657 and 0·652; in the tenth stratum, 0·786 and 0·747; in the eleventh stratum, 0·887 and 0·875; in the twelfth stratum, 0·994 and 0·984; in the thirteenth stratum, 1·080 and 1·100; in the fourteenth stratum, 1·176 and 1·198; in the fifteenth and sixteenth strata together, 2·209 and 2·324 grammes. With differences so moderate in amount between corresponding strata in the two experiments, this method of observing diffusion may claim a considerable degree of precision.

In similar experiments made at the same time and temperature with sugar, gum-arabic and tannin of nut-galls, the final distribution of each substance was different in each case, and the results may be placed together in illustration of unequal diffusibility, as exhibited by this method of observation. Two experiments were made on each substance, as with chloride of sodium, but the mean result only need be stated.

TABLE I.—Diffusion of 10 per cent. solutions (10 grammes of substance in 100 cub. cent. of fluid) into pure water, after fourteen days, at 10° (50° FAHR.).

Number of stratum (from above downwards).	Chloride of sodium.	Sugar.	Gum.	Tannin.
1	·104	·005	·003	·003
2	·129	·008	·003	·003
3	·162	·012	·003	·004
4	·198	·016	·004	·003
5	·267	·030	·003	·005
6	·340	·059	·004	·007
7	·429	·102	·006	·017
8	·535	·180	·031	·031
9	·654	·305	·097	·069
10	·766	·495	·215	·145
11	·881	·740	·407	·288
12	·991	1·075	·734	·556
13	1·090	1·435	1·157	1·050
14	1·187	1·758	1·731	1·719
15 and 16	2·266	3·783	5·601	6·097
	9·999	10·003	9·999	9·997

The superimposed column of water being 111 millimetres (4·38 inches) in height, the chloride of sodium, it will be observed, has diffused in sensible quantity to the top, and could have risen higher; the upper layer being found to contain 0·104 gramme of salt, or 1 per cent. of the whole quantity present. The apex of the diffusion column of sugar appears to have just reached the top of the liquid in the fourteen days of the experiment, for ·005 gramme only of that substance is found in the first stratum, followed by ·008, ·012, ·016, and ·030 in the following strata. Again, no gum appears to be carried by diffusion higher than the seventh stratum (2·2 inches), which stratum contains ·006 gramme, followed by ·031 gramme in the eighth stratum. The minute quantities of substance shown in the first to the sixth stratum, and which do not altogether exceed ·020 gramme, are no doubt the result of accidental dispersion, arising probably from a movement of the upper fluid occasioned by slight inequalities of temperature. The diffusion of tannin is even less advanced than that of gum; but the former numbers are apparently influenced by a partial decomposition, to which tannin is known to be liable, and which gives rise to new and more highly diffusible substances.

Experiments continued, like those last described, for a constant time, do not exhibit the exact relative diffusibilities, although these could be obtained by proceeding to ascertain, by repeated trial, the various times required to bring about a similar distribution and equal amount of diffusion in all the salts. The numbers observed, however, may afford data for the deduction of the relative diffusibilities by calculation.

A particular advantage of the new method is the means which it affords of ascertaining the absolute rate or velocity of diffusion. It becomes possible to state the distance which a salt travels per second in terms of the metre. It is easy to see that such a constant must enter into all the chronic phenomena of physiology, and that it holds a place in vital science not unlike the time of the falling of heavy bodies in the physics of

gravitation. It may therefore be not amiss to place here in a short tabular form the results observed of the diffusion of a few more substances, conducted in the same manner as the preceding.

TABLE II.—Diffusion of 10 per cent. solutions for fourteen days.

Number of stratum (from above downwards).	Sulphate of magnesia, at 10°.	Albumen, at 13° to 13°·5.	Caramel, at 10° to 11°.
1	·007		
2	·011		
3	·018		
4	·027		
5	·049		
6	·085	.....	·003
7	·133	.....	·005
8	·218	·010	·010
9	·331	·015	·023
10	·499	·047	·033
11	·730	·113	·075
12	1·022	·343	·215
13	1·383	·855	·705
14	1·803	1·892	1·725
15 and 16	3·684	6·725	7·206
	10·000	10·000	10·000

The sulphate of magnesia was anhydrous. The albumen was purified by WURTZ's method. The caramel was partly purified by precipitation by alcohol, as recommended by FRÉMY, and further by other means which will again be referred to. It will be remarked that the diffusion of sulphate of magnesia exhibited above is very similar to that of sugar in a former Table, but is slightly less advanced. The similarity in diffusibility of these two substances had already been observed in the experiments of former papers. The fall in rate on passing from these crystalloids to the colloids tannin, albumen, and caramel is very striking. The elevation in the liquid column attained by albumen or by caramel is moderate indeed compared with that of crystalline substances. Of albumen, which will be looked upon with most interest, no portion whatever was found in the seven higher strata. It appeared to the extent of 0·010 gramme in the eighth stratum, 0·015 in the ninth stratum, 0·047 in the tenth stratum, 0·113 in the eleventh stratum, 0·343 in the twelfth stratum; while the great mass of this substance remained in the four lower strata. The diffused albumen did not appear to lose its coagulability, or to be otherwise altered. It will be seen immediately that the diffusion of sugar advances as much in two days as the albumen above in fourteen days (Table IV.).

The diffusion of caramel is the slowest of all, and does not much exceed in fourteen days the diffusion of sugar in a single day.

It was considered useful to possess examples of the progress of diffusion, in one or two selected substances, for successive periods of time, so as to exemplify the continuous progress of diffusion in these substances. Such a chronological progress of diffusion in

a particular substance becomes a standard of comparison for single experiments on the diffusion of other substances. The substances selected were chloride of sodium and cane-sugar.

TABLE III.—Diffusion of a 10 per cent. solution of Chloride of Sodium in different times.

Number of stratum.	In four days, at 9° to 10°.	In five days, at 11°·75.	In seven days, at 9°.	In fourteen days, at 10°.
1	·004	·004	·013	·104
2	·004	·006	·017	·129
3	·005	·011	·028	·162
4	·011	·020	·051	·198
5	·023	·040	·081	·267
6	·040	·075	·134	·340
7	·080	·134	·211	·429
8	·145	·233	·318	·535
9	·261	·368	·460	·654
10	·436	·589	·640	·766
11	·706	·762	·850	·881
12	1·031	1·090	1·057	·991
13	1·416	1·357	1·317	1·090
14	1·815	1·697	1·527	1·187
15 and 16	4·023	3·613	3·294	2·266
	10·000	9·999	9·998	9·999

TABLE IV.—Diffusion of a 10 per cent. solution of Cane-sugar in different times.

Number of stratum.	In one day, at 10°·75.	In two days, at 10°.	In six days, at 9°.	In seven days, at 9°.	In eight days, at 9°.	In fourteen days, at 10°.
1	.....	.....	·001	·002	·002	·005
2	.....	.....	·002	·002	·003	·008
3	.....	.....	·002	·003	·003	·012
4	.....	.....	·002	·004	·004	·016
5	.....	.....	·003	·004	·007	·030
6	.....	.....	·005	·007	·012	·059
7	.....	.....	·011	·020	·031	·102
8	·002	·002	·024	·051	·072	·180
9	·002	·008	·071	·121	·154	·305
10	·005	·027	·170	·260	·304	·495
11	·024	·107	·376	·507	·555	·740
12	·133	·344	·727	·897	·858	1·075
13	·597	·930	1·282	1·410	1·365	1·435
14	1·850	1·940	1·930	1·950	1·955	1·758
15 and 16	7·386	6·641	5·392	4·760	4·674	3·783
	9·999	9·999	9·998	9·998	9·999	10·003

The scheme of the diffusion of the chloride of sodium may afford terms of comparison for the metallic salts, acids and other highly diffusible substances, while the scheme of sugar will be found more useful in appreciating the diffusion of organic and other less diffusible substances. In comparing the two Tables together, it appears that a fourteen days' diffusion of sugar is greater in amount than a four days' diffusion of chloride of sodium, but less than a five days' diffusion of the same substance. The diffusion of

chloride of sodium appears to be pretty nearly three times greater (or more rapid) than that of sugar.

The following experiments were made upon hydrochloric acid and chloride of sodium at a somewhat lower temperature and for times which are different, but which give a nearly equal diffusion for each substance.

TABLE IV. *bis.*—10 per cent. solutions.

Number of stratum.	Hydrochloric acid, in grammes. Three days at 5°.	Chloride of sodium, in grammes. Seven days at 5°.
1	·003	·003
2	·006	·009
3	·012	·010
4	·022	·026
5	·043	·055
6	·086	·082
7	·162	·165
8	·308	·270
9	·406	·403
10	·595	·595
11	·837	·823
12	1·080	1·085
13	1·163	1·270
14	1·578	1·615
15 and 16	3·699	3·589
	10·000	10·000

The diffusion of hydrochloric acid in three days corresponds closely with the diffusion of chloride of sodium in seven days. The times of equal diffusion for these two substances, at the temperature of the experiment, appear accordingly to be 1 (hydrochloric acid) and 2·33 (chloride of sodium). Hydrochloric acid and the allied hydracids, with other monobasic acids, are the most diffusive substances known. The general results of several series of experiments may be expressed approximately by the following numbers:—

Approximate times of equal diffusion.

Hydrochloric acid	1
Chloride of sodium	2·33
Sugar	7
Sulphate of magnesia	7
Albumen	49
Caramel	98

It is curious to observe the effect of changing the liquid atmosphere in which diffusion takes place, which is water in all these experiments, and replacing it by another fluid, namely alcohol. Two substances were diffused in the usual manner, but with this difference, that the substances were dissolved in alcohol, and the solutions placed under a column of the same liquid in the jar. The alcohol was of sp. gr. 0·822 (90 per cent.).

TABLE V.—Diffusion in Alcohol of 10 per cent. solutions of Iodine and of Acetate of Potash in seven days.

Number of stratum.	Iodine at 14°.	Acetate of potash, at 14° to 15°.
1	·028	·055
2	·033	·057
3	·046	·061
4	·038	·063
5	·037	·064
6	·039	·066
7	·081	·070
8	·143	·071
9	·263	·072
10	·417	·095
11	·637	·285
12	·936	·619
13	1·235	1·157
14	1·506	1·907
15 and 16	4·561	5·358
	10·000	10·000

TABLE V. *bis*.—Diffusion in Alcohol of a 10 per cent. solution of Resin, for seven days, at 14°·5.

Number in stratum.	Diffusate, in grammes.
1	·017
2	·017
3	·018
4	·017
5	·019
6	·020
7	·022
8	·024
9	·025
10	·080
11	·210
12	·498
13	·992
14	1·700
15 and 16	6·341
	10·000

The experiments were conducted in the absence of light, and there is no reason to believe that the iodine acted chemically upon the alcohol. The diffusion is more advanced in the iodine than in the acetate of potash, but in both is moderate in amount, confirming the early experiments with phials, which appeared to show that the diffusion process was several times slower in alcohol than in water. The small quantities of iodine found in each of the six superior strata are nearly equal, and were no doubt accidentally elevated by the mobility of this fluid, arising from its high dilatability by heat compared with that of water at the same low temperature. The diffusion may be

considered then as confined to the nine lower strata, and considerably resembles that of sugar in water for eight days.

The diffusion of acetate of potash is still less advanced than that of iodine, and is probably confined to the six lower strata, the salt found in the higher strata presenting in its distribution the appearance of having been carried there by a movement of the fluid consequent upon heat-dilatation, and not by diffusion. The diffusion of acetate of potash in alcohol observed during seven days approximates to that of sugar in water during six days (Table IV.).

I now proceed to observations of the simultaneous diffusion of two substances in the same fluid. The great object of this class of experiments was to separate salts of unequal diffusibility, and to test the application of diffusion as an analytical process. A mixture of two salts being placed at the bottom of the jar, it may be expected that the salts will diffuse pretty much as they do when they are diffused separately; the more diffusive salt travelling most rapidly, and showing itself first and always most largely in the upper strata. The early experiments of diffusion from phials had shown indeed that inequality of diffusion is increased by mixture, and the actual separation is consequently greater than that calculated from the relative diffusibilities of the mixed substances. Chlorides of potassium and sodium diffuse nearly in the proportion of 1 to 0·841, according to the earlier experiments. They may afford, therefore, the means of observing the amount of separation that may be produced by a very moderate difference in diffusibility. A mixture of 5 grammes of each salt in the usual 100 cub. cent. of water was diffused.

TABLE VI.—Diffusion of a mixture of 5 per cent. of Chloride of Potassium and 5 per cent. of Chloride of Sodium, for seven days, at 12° to 13°.

Number of stratum.	Chloride of potassium.	Chloride of sodium.	Total diffusate.
1	·018	·014	·032
2	·025	·015	·040
3	·044	·014	·058
4	·075	·017	·092
5	·101	·034	·135
6	·141	·063	·204
7	·185	·104	·289
8	·252	·151	·403
9	·330	·212	·542
10	·349	·351	·700
11	·418	·458	·876
12	·511	·559	1·070
13	·552	·684	1·236
14	·615	·772	1·387
15 and 16	1·385	1·551	2·936
	5·001	4·999	10·000

In the upper part of the Table chloride of potassium always appears in excess, but not in so large a proportion in the first three strata as in the fourth. This inequality

may be partly owing to mechanical dispersion of the mixed solution, but is to be referred chiefly, I believe, to errors of analysis from a loss of the chloride of potassium difficult to avoid in the determination of minute proportions of that salt by means of chloride of platinum. Of 92 milligrammes of salt found in the fourth stratum, 75 milligrammes, or 81·5 per cent., are chloride of potassium. The first six strata contain together 561 milligrammes, of which 404 milligrammes, or 72 per cent., that is nearly three-fourths, are chloride of potassium. We have to descend to the tenth stratum before the salts are found in equal proportions. The progression is then inverted, and chloride of sodium comes to preponderate in the lower strata.

It is evident that the preceding experiment might be so conducted as to diffuse away the chloride of potassium and leave below a mixture containing chloride of sodium in relative excess, to as great an extent as the chloride of potassium is found above, in the last experiment.

Further, the mixture in which chloride of potassium was concentrated in the experiment described, so as to form 72 per cent. of the whole mixture, might be subjected again to diffusion in the same manner. In an experiment upon a mixture of 7·5 grammes of chloride of potassium and 2·5 grammes of chloride of sodium, the six upper strata gave 640 milligrammes of salt, of which 610 milligrammes, or 95·3 per cent., were chloride of potassium. It is obvious that by repeating this diffusive rectification a sufficient number of times, a portion of the more diffusive salt might be obtained at last in a state of sensible purity.

The preceding example illustrates the separation of unequally diffusive metals or bases; the following example, on the other hand, the separation of unequally diffusive acids united with a common base. Chloride of sodium and sulphate of soda diffuse separately in the phial experiments in the proportion of 1 to 0·707.

TABLE VII.—Diffusion of 5 per cent. of Chloride of Sodium and 5 per cent. of anhydrous Sulphate of Soda, for seven days, at 10° to 10°·75.

Number of stratum.	Chloride of sodium, in grammes.	Sulphate of soda, in grammes.	Total diffusate, in grammes.
1	·009	.....	·009
2	·013	·001	·014
3	·024	·002	·026
4	·038	·003	·041
5	·060	·006	·066
6	·095	·012	·107
7	·141	·029	·170
8	·203	·059	·262
9	·278	·115	·393
10	·360	·205	·565
11	·473	·317	·790
12	·560	·507	1·067
13	·637	·694	1·331
14	·718	·909	1·627
15 and 16	1·390	2·141	3·531
	4·999	5·000	9·999

Here the separation is still more sensible than before with the bases. The six upper strata contain 263 milligrammes of salt, of which 239 milligrammes, that is 90·8 per cent., are chloride of sodium. The salt of the upper eight strata amounts to 695 milligrammes, of which 583 milligrammes, or 83·9 per cent., are chloride of sodium.

How long the diffusion should be continued in a liquid column of limited height, such as in these experiments, so as to produce the greatest separation, is a question of some interest, which can only be answered by experiment. The last diffusion was accordingly repeated, with the difference that it was continued for double the former time.

TABLE VIII.—Diffusion of 5 per cent. of Chloride of Sodium and 5 per cent. of Sulphate of Soda, for fourteen days, at 10° to 11°.

Number of stratum.	Chloride of sodium, in grammes.	Sulphate of soda, in grammes.	Total diffusate, in grammes.
1	·077	·005	·082
2	·089	·009	·098
3	·105	·014	·119
4	·130	·026	·156
5	·161	·044	·205
6	·199	·072	·271
7	·240	·111	·351
8	·289	·173	·462
9	·337	·241	·578
10	·392	·334	·726
11	·433	·433	·866
12	·487	·539	1·026
13	·525	·646	1·171
14	·555	·745	1·300
15 and 16	·979	1·609	2·588
	4·998	5·001	9·999

The salt contained in the three upper strata amounts to 299 milligrammes, of which 271, or 90·6 per cent. of the whole, are chloride of sodium. The upper five strata yield 660 milligrammes of salt, of which 562 milligrammes, or 85·1 per cent., are chloride of sodium. These proportions are not dissimilar to those deduced from the former Table, and show that little is gained in the way of separation by extending the diffusion period from seven to fourteen days; unless indeed the column of fluid be increased in height at the same time.

It might be worth observing whether the separation of two unequally diffusive metals can be favoured by varying the acid, or form of combination; whether, for instance, the hydrates of potash and soda would not separate to a greater extent than has been observed of the chlorides of potassium and sodium, the separate diffusibilities of the former substances being as 1 to 0·7, while that of the latter are as 1 to 0·841. I have not, however, pursued this branch of the subject.

The separation of the same metals from each other may possibly be favoured in another manner. In the preceding experiments (Table VI.) the two metals were in

union with the same acid, or rather both were in the state of chloride. But the metals might be used in combination with different acids, and these acids themselves might be of equal or of unequal diffusibility. If of equal diffusibility, such as nitric and hydrochloric acids, no reason appears why the acids should affect the amount of separation. But if the acids are unlike in diffusibility, the case is not so clear. If, for instance, the potassium were in the form of chloride and the sodium of that of sulphate, might not the diffusion of the potassium be promoted by the highly diffusive chlorine with which it is associated, and the diffusion of the soda, on the other hand, be retarded by its association with the slowly diffusive sulphuric acid? Will, in fine, the separation of the metals be greater from a mixture of chloride of potassium and sulphate of soda, or even from sulphate of potash and chloride of sodium, than from the two chlorides, or from the two sulphates? The inquiry, it will be remarked, raises the whole question of the distribution of acid and base in solutions of mixed salts. It will be illustrated by a comparison of the diffusion of chloride of potassium mixed with sulphate of soda, with the diffusion of sulphate of potash mixed with the chloride of sodium, the salts being taken in equivalent proportions.

TABLE IX.—Diffusion of a mixture of 5·12 per cent. of Chloride of Potassium and 4·88 per cent. of Sulphate of Soda (equivalent proportions), for seven days, at 14°.

Number of stratum.	Potassium, in grammes.	Sulphuric acid, in grammes.	Total diffusate, in grammes.
1	·023	·002	·024
2	·035	·002	·030
3	·048	·004	·045
4	·064	·009	·066
5	·092	·016	·097
6	·128	·032	·149
7	·174	·058	·215
8	·242	·105	·316
9	.....	.....	·441
10	.....	.....	·615
11	.....	.....	·815
12	.....	.....	1·042
13	.....	.....	1·290
14	.....	.....	1·517
15 and 16	.....	.....	3·346
			10·008

TABLE X.—Diffusion of a mixture of 4·01 per cent. of Chloride of Sodium and 5·99 per cent. of Sulphate of Potash (equivalent proportions), for seven days, at 14°.

Number of stratum.	Potassium, in grammes.	Sulphuric acid, in grammes.	Total diffusate, in grammes.
1	·028	·002	·023
2	·034	·002	·030
3	·049	·004	·044
4	·064	·009	·065
5	·092	·015	·096
6	·128	·031	·149
7	·172	·059	·219
8	·242	·104	·315
9	.....	.....	·435
10	.....	.....	·600
11	.....	.....	·797
12	.....	.....	1·025
13	.....	.....	1·261
14	.....	.....	1·480
15 and 16	.....	.....	3·467
			10·016

The weight of the mixed salt was always 10 grammes. The diffusions exhibited in the two Tables are strikingly similar, and indeed may be considered as identical. It thus appears that the diffusion of the metals is not affected by the acid with which they are in combination. The result is quite in harmony with BERTHOLLET'S view, that the acids and bases are indifferently combined, or that a mixture of chloride of potassium and sulphate of soda is the same thing as a mixture of sulphate of potash and chloride of sodium, when the mixtures are in a state of solution. With two acids very unequal in their affinity for bases, the result possibly might be very different.

2. *Effect of Temperature on Diffusion.*

Diffusion is promoted by heat, and separations may accordingly be effected in a shorter time at high than at low temperatures. In a series of observations made upon hydrochloric acid, the diffusion of that substance was carefully determined at 15°·5 (60° F.), and at three higher points, advancing by 11°·11 (20° F.). The ratios of the diffusions observed were as follows:—

Diffusion of hydrochloric acid at 15°·55 (60° F.), 1
" " at 26°·66 (80° F.), 1·3545
" " at 37°·77 (100° F.), 1·7732
" " at 48°·88 (120° F.), 2·1812.

The increments of diffusibility, 0·3545, 0·4187, and 0·408 for equal increments of temperature, are probably affected by small errors of observation, but they appear to indicate that the diffusion increases at a higher, although not greatly higher, rate than the temperature. The average increase of diffusibility for the whole range of temperature observed is 0·03543, or $\frac{1}{28}$ for each degree (0·01969, or $\frac{1}{50}$ nearly for 1° F.).

The preceding experiments were made by diffusing a 2 per cent. solution of hydrochloric acid from wide-mouth phials immersed in a jar of water, as in my former experiments*. The times were observed in which an equal amount of the acid (0·777 gramme from three phials) was diffused out. These times of equal diffusion were 72 hours at 15°·55 (60° F.); 53·15 hours at 26°·66 (80° F.); 40·6 hours at 37°·77 (100° F.); and 33 hours at 48°·88 (120° F.).

Fig. 2.

The diffusate from a 2 per cent. solution of chloride of potassium in similar circumstances was 0·6577 gramme,

In 101·75 hours, at 15°·55 (60° F.); and
In 41·93 hours, at 48°·88 (120° F.).

The diffusate from a 2 per cent. solution of chloride of sodium was 0·6533 gramme,

In 124·75 hours, at 15°·55 (60° F.);
In 49·60 hours, at 48°·88 (120° F.).

In equal times the diffusate would be

For chloride of potassium	at 15°·55 (60° F.),	1
" "	at 48°·88 (120° F.),	2·426
For chloride of sodium	at 15°·55 (60° F.),	1
" "	at 48°·88 (120° F.),	2·6151.

As the ratio between the diffusates of hydrochloric acid, at the same two temperatures, was 1 to 2·1812, it appears that the acid is less increased in diffusibility than the salts at the higher temperature; chloride of sodium also is slightly more increased than chloride of potassium. The more highly diffusive the substance the less does it appear to gain by heat. Chloride of sodium appears to be sensibly $2\frac{1}{2}$ times more diffusible at 48°·88 (120° F.) than at 15°·55 (60° F.): this gives an average increase of 0·014, or $\frac{1}{71}$ for 1 degree (0·025 for 1° F., or $\frac{1}{40}$). The inequality of diffusion which the three substances referred to exhibit at a low temperature, becomes therefore less at high temperatures; and it would appear to be the effect of a high temperature to assimilate diffusibilities. Heat, then, although it quickens the operation of diffusion, does not appear otherwise to promote the separation of unequally diffusive substances.

The results in such experiments are less disturbed by changes of temperature, if at all gradual, than might be supposed. A sensible separation was obtained of hydrochloric acid and chloride of sodium from each other, in a solution containing 2 per cent. of each substance, when the water-jar was heated up from 15°·55 to 95° C. in two hours, and maintained at the latter temperature during four hours more. Diffusion appeared to be accelerated about six times at the higher temperature.

At low temperatures, again, diffusion is proportionally slow. The ratio of diffusibility

* Philosophical Transactions, 1850, p. 25.

of the following salts at two different temperatures appeared to be,—

For chloride of potassium	at 5°·3 (41°·5 F.), 1;	at 16°·6 (62° F.), 1·4413
For chloride of sodium	at 5°·3 (41°·5 F.), 1;	at 17°·4 (63°·4 F.), 1·4232
For nitrate of soda	at 5°·3 (41°·5 F.), 1;	at 17°·4 (63°·4 F.), 1·4475
For nitrate of silver	at 5°·3 (41°·5 F.), 1;	at 17°·4 (63°·4 F.), 1·3914.

The salts are unequally affected to a sensible extent; and it will be observed that the superiority of chloride of potassium over chloride of sodium, in diffusibility, is increased at the low temperature.

Within the range of temperature of the preceding experiments, the diffusibility of chloride of sodium being taken as 1 at 17°·4 (63°·4 F.), it becomes 0·7026 at 5°·3 (41°·5 F.); or it diminishes 0·0246, or $\frac{1}{40\cdot7}$, for a depression of 1° (0·0136, or $\frac{1}{73\cdot5}$, for a depression of 1° F.).

3. *Dialysis.*

Passing from liquid diffusion in the water-jar, I may advert first to the diffusion of crystalloids through a gelatinous or colloid mass, the circumstance of the experiment being varied as little as possible from those of jar-diffusion.

Ten grammes of chloride of sodium and 2 grammes of the Japanese gelatine, or gelose of PAYEN, were dissolved together in so much hot water as to form 100 cub. cents. of fluid. Introduced into the empty diffusion-jar and allowed to cool, this fluid set into a firm jelly, occupying the lower part of the jar, and containing of course 10 per cent. of chloride of sodium. Instead of placing pure water over this jelly, it was covered by 700 cub. cents. of a solution containing 2 per cent. of the same gelose, cooled so far as to be on the point of gelatinizing; the jar at the same time being placed in a cooling mixture, in order to expedite that change. The jar with its contents was now left undisturbed for eight days at the temperature 10°. After the lapse of this time the jelly was removed from the jar in successive portions of 50 cub. cents. each from the top, and the proportion of chloride of sodium in the various strata ascertained. The results were very similar to those obtained in diffusing the same salt in a jar of pure water. The diffusion in the gelose appeared more advanced in eight days than diffusion in water for seven days, as will be seen by comparing the gelose experiment below with a water experiment on chloride of sodium which had been conducted at nearly the same temperature (Table III.).

TABLE XI.—Diffusion of a 10 per cent. solution of Chloride of Sodium in the jelly of gelose, for eight days, at 10°.

Number of stratum.	Diffusate, in grammes.
1	·015
2	·015
3	·026
4	·035
5	·082
6	·130
7	·212
8	·350
9	·486
10	·630
11	·996
12	1·172
13	1·190
14	1·203
15 and 16	3·450
	9·992

Diffusion of a crystalloid thus appears to proceed through a firm jelly with little or no abatement of velocity. With a coloured crystalloid, such as bichromate of potash, the gradual elevation of the salt to the top of the jar is beautifully illustrated. On the other hand, the diffusion of a coloured colloid such as caramel through the jelly, appears scarcely to have begun after eight days had elapsed. The diffusion of a salt into the solid jelly may be considered as cementation in its most active form.

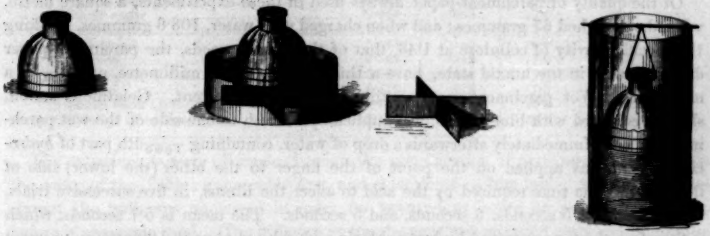
Numerous experiments were made on the diffusion of crystalloids through various dialytic septa, such as gelatinous starch, coagulated albumen, gum-tragacanth, besides animal mucus and parchment-paper, which all tended to prove how little the diffusive process was interfered with by the intervention of colloid matter. Salts appeared to preserve their usual relative diffusibility unchanged. The same partial separation of mixed salts was observed as in the water-jar. With a mixture, for instance, of equal parts of chlorides of potassium and sodium in the dialyser, the first tenth part of the mixture which passed through was found to consist of 59·17 per cent. of chloride of potassium and 40·83 per cent. of chloride of sodium. Double salts also, such as alum, and the sulphate of copper and potash, which admit of being resolved into pairs of unequally diffusive salts, were largely decomposed upon the dialyser, as they are in the water-jar. The effect of heat in promoting diffusion appeared, however, to be diminished in dialysis, at least with a parchment-paper septum. Thus the diffusion from a 2 per cent. solution of chloride of sodium in a constant period of three hours was,—

		Ratio.
At 10°	. . . 0·738 grm.	1
At 20	. . . 0·794 grm.	1·07
At 30	. . . 0·892 grm.	1·20
At 40	. . . 1·017 grm.	1·37.

The rate of diffusion in water alone, without the septum, would have been doubled by an equal rise of temperature instead of being increased one-third only as above.

The small glass bell-jar (fig. 3) formerly used as an osmometer, was conveniently

Fig. 3.—Bulb Dialyser.



applied to dialytic experiments. Two sizes of the bulb were employed, 3·14 and 4·44 inches in diameter respectively, and of which the dialytic septa possessed an area very nearly of $\frac{1}{100}$dth and $\frac{1}{300}$dth of a square metre (15·6 and 7·8 square inches). With 100 cub. cents. of fluid in the osmometer (the volume usually employed), the septum of the smaller instrument was covered to a depth of about 20 millimetres (0·8 inch), and the septum of the larger to a depth of 10 millimetres (0·4 inch). The thinner the stratum, the more exhaustive the diffusion in a given time. It is generally unadvisable to cover the septum deeper than 10 or 12 millimetres (half an inch), where a considerable diffusion is desired within twenty-four hours. The following practical observations may be found useful in applying the dialyser to actual cases of analysis. They refer to the parchment-paper septum, which is much the most convenient for use.

With a 2 per cent. solution of chloride of sodium, containing 2 grammes of the salt, and covering a septum of nearly 0·01 square metre (15·6 square inches) in area, to a depth of 10 millimetres, the salt which diffused in five hours amounted to 0·75 gramme, and in twenty-four hours to 1·657 gramme, leaving behind 0·343 gramme, or 17·1 per cent. of the original salt. The following experiments, made with the same osmometer and solution, show the effect of reducing the volume of liquid placed in the dialyser. The proportion of salt which diffused out in twenty-four hours was—

From 100 cub. cents. of solution 86 per cent.
From 50 cub. cents. of solution 92 per cent.
From 25 cub. cents. of solution 96 per cent.

In all cases the volume of water outside into which the salt escaped was ample, being from five to ten times greater than the volume of fluid placed in the dialyser, and it was changed during the continuance of the experiment. A much less volume of external water suffices, provided it is changed at intervals of a few hours. The temperature was

10° to 12°. It will be observed that these volumes correspond to a depth of liquid in the dialyser of 0·4, 0·2, and 0·1 inch respectively.

The time of travelling through the thickness of the parchment-paper itself may be observed, and is worthy of remark.

Of the quality of parchment-paper always used in these experiments, a square metre, when dry, weighed 67 grammes; and when charged with water, 108·6 grammes. Taking the specific gravity of cellulose at 1·46, that of the lighter woods, the parchment-paper described will, in the humid state, have a thickness of 0·0877 millimetre, or $\frac{1}{11{\cdot}41}$ of a millimetre. Wet parchment-paper so thin is highly translucent. Gelatinous starch, slightly coloured with blue litmus, was applied by a brush to one side of the wet parchment-paper. Immediately afterwards a drop of water, containing $\frac{1}{1000}$dth part of *hydrochloric acid*, was applied on the point of the finger to the other (the lower) side of the paper. The time required by the acid to affect the litmus, in five successive trials, was 6 seconds, 5·5 seconds, 6 seconds, and 5 seconds. The mean is 5·7 seconds, which is therefore the time required by hydrochloric acid, diluted already 1000 times, to travel a distance of 0·0877 of a millimetre, by the agency of diffusion. The temperature was 15°.

With hydrochloric acid diluted twice as much as before (water containing 0·0005 dry acid), the average time of passage was 10·4 seconds, or nearly double the preceding time.

Water containing $\frac{1}{1000}$dth of sulphuric acid (an acid less rapidly diffused than hydrochloric acid) reddened the litmus in 9·1 seconds, and when doubly diluted in 16·5 seconds.

These results are not affected, it is believed, by any sensible diffusive movement on the part of the litmus. The diffusion of that colouring matter, in a colloid medium, is so slow that it may be entirely disregarded. The acid, therefore, is not met in its way by the litmus, but really travels the entire distance expressed by the thickness of the parchment-paper. The first experiments related give a diffusive velocity, in water, to hydrochloric acid, already diluted one thousand times, of 0·0154 millimetre per second, and 0·924 millimetre in one minute.

The few following dialytic experiments may be recorded for the sake of the practical points which they bring out. They were made in the smaller osmometer, with 100 cub. cents. of a solution containing 10 grammes of each of the various substances. The area of the parchment-paper septum was 0·005 square metre, and the depth of the stratum of fluid placed upon it 20 millimetres. The substances diffused were all crystalloids, with the exception of gum-arabic.

TABLE XII.—Dialysis through Parchment-paper during twenty-four hours, at 10° to 15°.

Ten per cent. solutions.	Diffusate, in grammes.	Relative diffusate.	Osmose, in grammes of water.	Relative osmose.
Gum-arabic	0·029	·004	5·0	·263
Starch-sugar	2·000	·266	17·0	·894
Cane-sugar	1·607	·214	15·3	·805
Milk-sugar	1·387	·185	15·0	·789
Mannite	2·621	·349	17·6	·926
Glycerine	3·300	·440	17·6	·926
Alcohol	3·570	·476	7·6	·400
Starch-sugar (second experiment)	2·130	·284	16·8	·884
Chloride of sodium	7·500	1	19·0	1

The experiments were all made through the same portion of parchment-paper, and in the order of the Table; gum-arabic first, and chloride of sodium last. After every experiment the bulb was immersed in water for twenty-four hours, to purify the septum, before it was again used. The diffusion of starch-sugar was repeated early and late in the series of experiments, with little change in the result, showing considerable uniformity in the action of the parchment-paper; the first diffusate of starch-sugar being 2 grammes, and the second 2·13 grammes. Yet the parchment-paper had been in contact with water or some solution for a whole fortnight between the two observations referred to.

A layer of animal mucus, taken from the stomach of the pig, 12 millimetres in thickness (10 grammes of humid mucus being spread over 0·005 square metre of surface), was applied, between two discs of calico, to the diffusion-bulb used above, the parchment-paper being first removed.

TABLE XIII.—Dialysis through Animal Mucus during twenty-five hours, at 10° to 15°.

Ten per cent. solutions.	Diffusate, in grammes.	Proportional diffusate.	Osmose, in grammes of water.
Gum-arabic	·023	·004	+29
Starch-sugar	1·821	·360	+ 7·6
Cane-sugar	1·753	·347	+ 4·6
Milk-sugar	1·328	·262	+ 7·1
Mannite	1·895	·375	+ 5·0
Alcohol	2·900	·573	+ 7·2
Starch-sugar	1·765	·349	+ 7·0
Glycerine	2·554	·505	+ 7·5
Chloride of sodium	5·054	1	− 0·2

The relative diffusibilities of the different substances present a considerable degree of similarity in the two Tables, and are equally analogous to the diffusibilities of the same substances observed in pure water. The intervention of a colloid septum cannot be said to have impeded much the diffusion of any of these substances except the colloid gum.

The dialysis through parchment-paper of several other organic substances, both crystalloids and colloids, may be brought together, in comparison with the chloride of sodium as a standard. The larger osmometer bulb was used, and the parchment-paper

was now changed in each experiment. The substance in solution amounted to 2 grammes, the depth of fluid in the dialyser to 10 millimetres (0·4 of an inch), and the surface of the septum to 0·01 square metre (15·6 square inches).

TABLE XIV.—Dialysis through Parchment-paper during twenty-four hours, at 12°.

Two per cent. solutions.	Diffusate, in grammes.	Proportional diffusate.
Chloride of sodium	1·657	1
Picric acid	1·690	1·020
Ammonia	1·404	·847
Thein	1·166	·703
Salicin	·835	·503
Cane-sugar	·783	·472
Amygdalin	·517	·311
Extract of quercitron	·305	·184
Extract of logwood	·280	·168
Catechu	·265	·159
Extract of cochineal	·086	·051
Gallo-tannic acid	·050	·030
Extract of litmus	·033	·019
Purified caramel	·009	·005

Picric acid and thein were actually diffused from 1 per cent. solutions, and the numbers observed are multiplied by 2. The crystallizable principles, thein, salicin, and amygdalin, appear greatly more diffusible than gallo-tannic acid, or than gum, as has been already seen. Such inequality of rate is likely to facilitate the separation of vegetable principles by the agency of dialysis.

4. *Preparation of Colloid Substances by Dialysis.*

The purification of many colloid substances may be effected with great advantage by placing them on the dialyser. Accompanying crystalloids are eliminated, and the colloid is left behind in a state of purity. The purification of soluble colloids can rarely be effected by any other known means, and dialysis is evidently the appropriate mode of preparing such substances free from crystalloids.

Soluble Silicic Acid.—A solution of silica is obtained by pouring silicate of soda into diluted hydrochloric acid, the acid being maintained in large excess. But in addition to hydrochloric acid, such a solution contains chloride of sodium, a salt which causes the silica to gelatinize when the solution is heated, and otherwise modifies its properties. Now such soluble silica, placed for twenty-four hours in a dialyser of parchment-paper, to the usual depth of 10 millimetres, was found to lose in that time 5 per cent. of its silicic acid, and 86 per cent. of its hydrochloric acid. After four days on the dialyser, the liquid ceased to be disturbed by nitrate of silver. All the chlorides were gone, with no further loss of silica. In another experiment 112 grammes of silicate of soda, 67·2 grammes of dry hydrochloric acid, and 1000 cub. cents. of water were brought together, and the solution placed upon a hoop dialyser, 10 inches in diameter. After four days the solution had increased to 1235 cub. cents., by the action of osmose; colloid

bodies being generally highly osmotic. The solution now gave no precipitate with nitrate of silver, and contained 60·5 grammes of silica, 6·7 grammes of that substance having been lost. The solution contained 4·9 per cent. of silicic acid.

The pure solution of silicic acid so obtained may be boiled in a flask, and considerably concentrated, without change; but when heated in an open vessel, a ring of insoluble silica is apt to form round the margin of the liquid, and soon causes the whole to gelatinize. The pure solution of hydrated silicic acid is limpid and colourless, and not in the least degree viscous, even with 14 per cent. of silicic acid. The solution is the more durable the longer it has been dialysed and the purer it is. But this solution is not easily preserved beyond a few days, unless considerably diluted. It soon appears slightly opalescent, and after a time the whole becomes pectous somewhat rapidly, forming a solid jelly transparent and colourless, or slightly opalescent, and no longer soluble in water. This jelly undergoes a contraction after a few days, even in a close vessel, and pure water separates from it. The coagulation of the silicic acid is effected in a few minutes by a solution containing $\frac{1}{10.000}$th part of any alkaline or earthy carbonate, but not by caustic ammonia, nor by neutral or acid salts. Sulphuric, nitric, and acetic acids do not coagulate silicic acid, but a few bubbles of carbonic acid passed through the solution produce that effect after the lapse of a certain time. Alcohol and sugar, in large quantity even, do not act as precipitants; but neither do they protect silicic acid from the action of alkaline carbonates, nor from the effect of time in pectizing the fluid colloid. Hydrochloric acid gives stability to the solution: so does a small addition of caustic potash or soda.

This pure water-glass is precipitated on the surface of a calcareous stone without penetrating, apparently from the coagulating action of soluble lime-salts. The hydrated silicic acid then forms a varnish, which is apt to scale off on drying. The solution of hydrated silicic acid has an acid reaction, somewhat greater than that of carbonic acid. It appears to be really tasteless (like most colloids), although it occasions a disagreeable persistent sensation in the mouth, after a time, probably from precipitation.

Soluble hydrated silicic acid, when dried in the air-pump receiver, at 15°, formed a transparent glassy mass of great lustre, which was no longer soluble in water. It retained 21·99 per cent. of water after being kept two days over sulphuric acid.

The colloidal solution of silicic acid is precipitated by certain other soluble colloids, such as gelatine, alumina, and peroxide of iron, but not by gum nor caramel. As hydrated silicic acid, after once gelatinizing, cannot be made soluble again by either water or acids, it appears necessary to admit the existence of two allotropic modifications of that substance, namely, soluble hydrated silicic acid, and insoluble hydrated silicic acid, the fluid and pectous forms of this colloid.

The ordinary soluble silicate of soda is not at all colloidal, but diffuses as readily through a septum as the sulphate of soda does. Several crystalline hydrated silicates of soda are known (Fritzsche).

The amorphous silicic acid obtained by drying and calcining the jelly, and the vitreous

acid obtained by igneous fusion, have both a specific gravity of about 2·2, according to H. Rose*, and appear to be the same colloidal substance; while the specific gravity of crystalloidal silicic acid (rock-crystal and quartz) is about 2·6.

Soluble silicic acid forms a peculiar class of compounds, which like itself are colloidal, and differ entirely from the ordinary silicates. The new compounds are interesting from their analogy to organic substances, and from appearing to contain an acid of greatly higher atomic weight than ordinary silicic acid. Like gallo-tannic acid, gummic acid, and the other organic colloidal acids, silicic acid combines with gelatine; the last substance appearing to possess basic properties. *Silicate of gelatine* falls as a flaky, white and opake precipitate, when the solution of silicic acid is gradually added to a solution of gelatine in excess. The precipitate is insoluble in water, and is not decomposed by washing. Silicate of gelatine prepared in the manner described, contains 100 silicic acid to about 92 gelatine. This is a greater proportion of gelatine than in the gallo-tannate of gelatine, and requires for soluble silicic acid a higher equivalent than that of gallo-tannic acid. In the humid state the gelatine of this compound does not putrefy.

The acid reaction of 100 parts of soluble silicic acid is neutralized by 1·85 part of oxide of potassium, and by corresponding proportions of soda and ammonia. The *colli*-silicates or *co*-silicates thus formed are soluble and more durable than fluid silicic acid, but they are pectized by carbonic acid or by an alkaline carbonate, after standing for a few minutes. The co-silicate of potash forms a transparent hydrated film on drying *in vacuo*, which is not decomposed by water, and appears to require about ten thousand parts of water to dissolve it. The silicate of soda which Forchhammer obtained by boiling freshly precipitated silicic acid with carbonate of soda, and collecting the precipitate which falls on cooling, contains 2·74 per cent. of soda, and is represented by $NaO+36SiO_2$ (Gmelin). This silicate is probably a co-silicate of soda in the pectous condition. Soluble silicic acid produces a gelatinous precipitate in lime-water, containing 6 per cent. and upwards of the basic earth. This and the other insoluble earthy co-silicates appear not to be easily obtained in a definite state. They gave out a more basic silicate to water on washing. The composition of these salts, and that also of the co-silicate of gelatine, were found to vary according as the mode of preparation was modified. When a solution of gelatine was poured into silicic acid in excess, the co-silicate of gelatine formed gave, upon analysis, 100 silicic acid with 56 gelatine, or little more than half the gelatine stated above as found in that compound prepared with the mode of mixing the solutions reversed. The gallo-tannate of gelatine is known to offer the same variability in composition.

The gelatine used in the preceding experiments was isinglass (colle de poisson), purified by solution in hydrochloric acid and subsequent dialysis. As the acid escapes by diffusion, a jelly is formed in the dialyser. This jelly is free from the earthy matter, amounting to about 0·4 per cent. in isinglass, and is not liable to putrefaction.

Cosilicic acid also precipitates both *albuminic acid* and pure *casein*.

* Annales de Chimie, 3 sér. t. lvii. p. 163.

Soluble Alumina.—We are indebted to Mr. WALTER CRUM for the interesting discovery that alumina may be held in solution by water alone in the absence of any acid. But two soluble modifications of alumina appear to exist, alumina and metalumina. The latter is Mr. CRUM'S substance.

A solution of the neutral chloride of aluminium ($Al_2 Cl_3$), placed on the dialyser, appears to diffuse away without decomposition. But when an excess of hydrated alumina is previously dissolved in the chloride, the latter salt is found to escape by diffusion in a gradual manner, and the hydrated alumina, retaining little or no acid, to remain behind in a soluble state. A solution of alumina in chloride of aluminium, consisting at first of 52 parts of alumina to 48 of hydrochloric acid, after a dialysis of six days, contained 66·5 per cent. of alumina; after eleven days 76·5 per cent.; after seventeen days 92·4 per cent., and after twenty-five days the alumina appeared to be as nearly as possible free from acid, as traces only of hydrochloric acid were indicated by an acid solution of nitrate of silver. But in such experiments the alumina often pectizes in the dialyser before the hydrochloric acid has entirely escaped.

Acetate of alumina with an excess of alumina gave similar results. The alumina remained fluid in the dialyser for twenty-one days, and when it pectized was found to retain 3·4 per cent. of acetic acid, which is in the proportion of 1 equivalent of acid to 28·2 equivalents of alumina.

Soluble alumina is one of the most unstable of substances, a circumstance which fully accounts for the difficulty of preparing it in a state of purity. It is coagulated or pectized by portions, so minute as to be scarcely appreciable, of sulphate of potash and, I believe, by all other salts; and also by ammonia. A solution containing 2 or 3 per cent. of alumina was coagulated by a few drops of well-water, and could not be transferred from one glass to another, unless the glass was repeatedly washed out by distilled water, without gelatinizing. Acids in small quantity also cause coagulation; but the precipitated alumina readily dissolves in an excess of the acid. The colloids gum and caramel also act as precipitants.

This alumina is a mordant, and possesses indeed all the properties of the base of alum and the ordinary aluminous salts. A solution containing 0·5 per cent. of alumina may be boiled without gelatinizing, but when concentrated to half its bulk it suddenly coagulated. Soluble alumina gelatinizes when placed upon red litmus paper, and forms a faint blue ring about the drop, showing a feeble alkaline reaction. Soluble alumina is not precipitated by alcohol nor by sugar. No pure solution of alumina, although dilute, remained fluid for more than a few days.

Like hydrated silicic acid, then, the colloid alumina may exist either fluid or pectous, or it has a soluble and insoluble form, the latter being the gelatinous alumina as precipitated by bases. It is evident that the extraordinary coagulating action of salts upon hydrated alumina must prevent the latter substance from ever appearing in a soluble state when liberated from combination by means of a base.

Colloidal alumina possesses also, I believe, a high atomic weight, like cosilicic acid.

The chloride of alumina with excess of alumina referred to above appears to be, either in whole or in part, a colloidal hydrochlorate of alumina, containing the latter substance with its large colloidal equivalent, and may be really neutral in composition. The soluble basic persalts of iron, tin, &c. are likewise all colloidal, and have no doubt a similar constitution. Such colloidal salts are themselves slowly decomposed on the dialyser, being resolved into the crystalloidal acid which escapes and the colloidal oxide which remains behind.

Soluble Metalumina.—Mr. Crum first pointed out a singular relation of acetic acid to alumina, which has never been explained. Sulphate of alumina, when precipitated by acetate of lead or baryta, gives a binacetate of alumina, with one equivalent of free acetic acid; the neutral teracetate of alumina not appearing to exist. It was further observed that, by keeping a solution of this binacetate in a close vessel at the boiling-point of water for several days, nearly the whole acetic acid came to be liberated, without any precipitation of alumina occurring at the same time. Mr. Crum boiled off the free acetic acid, or the greater part of it, and thus obtained his soluble alumina. The same result may be arrived at by dialysing a solution of acetate of alumina that has been altered by heat. In three days the acetic acid was reduced on the dialyser to 11 per cent., giving 1 equiv. acetic acid to 8 equivs. alumina; in six days to 7·17 per cent. acid; in thirteen days to 2·8 per cent. acid, or 1 equiv. acid to 33 equivs. alumina. The alumina exists in an allotropic condition, being no longer a mordant; and forming, when precipitated, a jelly that is not dissolved by an excess of acid. Metalumina resembles alumina in being coagulated by minute proportions of acids, bases, and of most salts. Mr. Crum found the solution of metalumina to require larger quantities of acetates, nitrates, and chlorides to produce coagulation than of the former substances. The solution of metalumina is tasteless, and entirely neutral to test-paper, according to my own observation.

Like alumina, the present colloid has therefore a fluid and a pectous form, the liquid soluble metalumina, and the gelatinous insoluble metalumina.

Soluble Peroxide of Iron.—A solution of hydrated peroxide of iron may be obtained by a process exactly analogous to that for soluble alumina. Perchloride of iron in solution is first saturated with hydrated peroxide of iron, added by small quantities at a time; or carbonate of ammonia may be added in a gradual manner to perchloride of iron, so long as the precipitated oxide continues to be redissolved on stirring. These red solutions of iron have lately been carefully investigated by Mr. Ordway (Silliman's Journal, 3 ser. xxxix. 197), by M. Bechamp (Annales de Chimie, 3 sér. lvii. 293), and by M. Scheurer-Kestner (Ib. lv. 330). It is observed that the act of solution of the hydrated peroxide by the chloride of iron is a gradual process, demanding time. The quantity of oxide taken up will go on increasing for a long time, if digestion in the cold is continued. Mr. Ordway found chloride of iron to take up so much as 18 equivalents of peroxide of iron in the course of five months. This slowness of action is highly characteristic of colloids. Only monobasic acids, such as hydrochloric and nitric, serve for

preparing such solutions; sulphuric and other polybasic acids giving insoluble subsalts with excess of ferric oxide, or of any other aluminous oxide. The red liquid so obtained is already a colloidal hydrochlorate of peroxide of iron, but requires to be dialysed for a sufficient time. Such a compound possesses an element of instability in the extremely unequal diffusibility of its constituents. Beginning with perchloride of iron, containing five or six equivalents of peroxide in solution, the whole solid matter also amounting to 4 or 5 per cent. of the liquid, and the latter forming a stratum of the usual depth of about half an inch in the dialyser, it was found that hydrochloric acid diffused out accompanied only by a small proportion of the iron. After eight days, the deep red solution in the dialyser contained peroxide of iron and hydrochloric acid, in the proportion of 97·6 per cent. of the former to 2·4 per cent. of the latter. In nineteen days the hydrochloric acid was reduced to 1·5 per cent., which gives 1 equiv. of acid to 30·3 equivs. peroxide of iron. The last solution was transferred to a phial, in which it remained fluid for twenty days, and then spontaneously pectized.

The peracetate of iron, prepared by double decomposition, is incapable of dissolving hydrated peroxide of iron, as is well known, but still may be made a source of soluble peroxide; as the salt referred to is itself decomposed to a great extent by diffusion on the dialyser. About one-half of the iron was lost by a diffusion of eighteen days, in a particular experiment, leaving on the dialyser a red liquid, in which ninety-four parts of peroxide of iron were still associated with six parts of acetic acid.

Water containing about 1 per cent. of hydrated peroxide of iron in solution has the dark red colour of venous blood. The solution may be concentrated by boiling to a certain point, and then pectizes. The red solution is coagulated in the cold by traces of sulphuric acid, alkalies, alkaline carbonates, sulphates and neutral salts in general, but not by hydrochloric, nitric, and acetic acids, nor by alcohol or sugar. The coagulum is a deep red-coloured jelly, resembling the clot of blood, but more transparent. Indeed the coagulation of this colloid is highly suggestive of that blood, from the feeble agencies which suffice to effect the change in question, as well as from the appearance of the product. The coagulum formed by a precipitant, or in the course of time, without any addition having been made to the solution of peroxide of iron, is no longer soluble in water, hot or cold; but it yields readily to dilute acids. It is, in short, the ordinary hydrated peroxide of iron. Here then, again, we have a soluble and insoluble form of the same colloidal substance. Native hematite, which presents itself in mammillary concretions, is no doubt colloidal.

Soluble Metaperoxide of Iron.—The soluble peroxide of iron of M. Péan de Saint-Gilles* appears to be the analogue of metalumina. It was also prepared by the prolonged action of heat upon a pure solution of the acetate. The characteristic properties of this substance, which indicate its allotropic nature, are the orange-red colour and the opalescent appearance of its solution. The metaperoxide of iron is entirely precipitated of a brown ochreous appearance by a trace of sulphuric acid, or of an alkaline salt, and

* Comptes Rendus, 1855, p. 568.

is insoluble in all cold acids, even when the latter are concentrated. The solubility of metaperoxide of iron in water appears to be more precarious, if possible, than that of the colloid alumina. It would no doubt be more safely prepared by diffusing away the acetic acid of the altered acetate of iron, than it is by boiling off that acid; as the solution is said to become precipitable by heat before the whole acetic acid is expelled.

Ferrocyanide of Copper.—Many of the insoluble ferrocyanides are crystalline precipitates, but the compound above named, and the different varieties of prussian blue, appear to be strictly colloidal.

Certain anomalous properties long observed in these compounds come thus to be explained. The ferrocyanide of copper precipitated from ferrocyanide of potassium and sulphate of copper, is a reddish-brown gelatinous precipitate, and carries down a portion of the potash salt. It is obtained of greater purity, like the other insoluble ferrocyanides, by the use of ferrocyanic acid as the precipitant. Ferrocyanide of copper is then darker in colour, and still more highly gelatinous. It is well known that this substance appears as a transparent almost colourless jelly, when precipitated from strong solutions. This colloidal matter assumes colour on the addition of water, in consequence of further hydration, following in this respect the analogy of the crystalloid salts of copper. The ferrocyanide of copper, when once precipitated, may be washed without loss, and exhibits no symptoms of solubility. But it has been remarked that the same salt, when produced by mixing the precipitating salts dissolved in not less than two or three thousand times their weight of water, gives a wine-red solution with no precipitate. This is the soluble condition of the colloid. When the red solution is placed in the dialyser the salt of potash diffuses out, and the whole ferrocyanide of copper is retained behind in solution.

Precipitated ferrocyanide of copper is not dissolved by oxalic acid, nor by oxalate of potash, but dissolves freely in about one-fourth of its weight of neutral oxalate of ammonia. The ferrocyanide of copper must be washed beforehand, to ensure solubility. A solution holding 3 or 4 per cent. of ferrocyanide of copper is of a dark reddish-brown colour, intermediate in tint between the acetate and meconate of iron. The solution is transparent, but assumes a peculiar appearance of opacity when seen by light reflected from its surface. The same appearance was observed by Péan de Saint-Gilles in his metaperoxide of iron.

When a red solution, such as that described, was dialysed, the oxalate of ammonia came away in a gradual manner; 30·6 per cent. of the oxalate of ammonia were found in the colourless diffusate of the first twenty-four hours; 31 per cent. of the same salt in the diffusate of the next three days, and 18·2 per cent. in the diffusates of the following seven days, making altogether 79·8 per cent., or four-fifths of the whole oxalate of ammonia originally introduced. A small portion of the ammoniacal salt is retained with force, as might be expected from a ferrocyanide. Although the diffusate appeared colourless, it was found to contain a little oxide of copper, namely, 0·041 gramme (of which 0·022 gramme diffused out in the first twenty-four hours), from 2 grammes of ferrocyanide of copper placed in the dialyser.

The liquid ferrocyanide of copper, both before and after being dialysed, may be heated without change, but it is pectized by foreign substances with extreme facility. This effect is produced by a minute addition of nitric, hydrochloric, and sulphuric acids in the cold, and of oxalic and tartaric acids with the aid of a slight heat. It is remarkable that acetic acid does not pectize the ferrocyanide of copper and many other colloids. Sulphate of potash, sulphate of copper, and metallic salts generally appear to pectize the red liquid. The oxalate of ammonia, if any is present, remains in solution.

Neutral Prussian Blue.—The blue precipitate from perchloride of iron and ferrocyanide of potassium, or ferrocyanic acid, is a bulky hydrate, which dries up into gummy masses, so far resembling a colloid. The precipitate dissolves readily with the aid of a gentle heat, in one-sixth of its weight of oxalic acid, giving the well-known solution of prussian blue, used as an ink. Prussian blue is equally soluble in the oxalate and binoxalate of potash. When the solution of prussian blue in oxalic acid was placed on the dialyser, no colouring matter came through, but 28 per cent. of the oxalic acid diffused away in the first twenty-four hours, accompanied by traces of peroxide of iron. The oxalic acid appears to leave the colloidal solution very slowly and incompletely, 8 per cent. diffusing away in the second twenty-four hours, 11 per cent. in the next four days, and 2 per cent. in the following six days. The colloidal solution of prussian blue was pectized by small additions of sulphate of zinc and several other metallic salts, but required larger quantities of the alkaline salts for precipitation.

Ferridcyanide of Iron.—The blue precipitate from the ferridcyanide of potassium and a protosalt of iron is soluble in oxalic acid and the binoxalate of potash, but not in the neutral oxalates. This blue liquid is quite incapable of passing through the dialyser, and is equally colloidal with ordinary prussian blue. So also is *basic prussian blue* prepared by the spontaneous oxidation of precipitated ferrocyanide of protoxide of iron. This last colloid might probably be purified with advantage upon the dialyser.

The ammonio-tartrate of iron, ammonio-citrate of iron, and similar pharmaceutical preparations are chiefly colloidal matters.

Sucrate of Copper.—The deep blue liquid obtained by adding potash to a mixed solution of chloride of copper and sugar appears to contain a colloidal substance. Placed on a dialyser for four days, the blue liquid became green, and no longer contained either potassium or chlorine; it in fact consisted of oxide of copper united with twice its weight of sugar. The external liquid remained colourless, and gave no indication of copper when tested with sulphuretted hydrogen. The colloidal solution of sucrate of copper was sensitive in the extreme to pectizing agents. Salts and acids generally gave a bluish-green precipitate; even acetic acid had that effect. The precipitate, or pectous sucrate, after being well-washed, consisted of oxide of copper with about half its weight of sugar, and is therefore a subsucrate. When the green liquid is heated strongly, it gives a bluish-green precipitate, and does not allow the copper to be readily reduced to the state of suboxide. The subsucrate of copper possesses considerable vivacity of colour, and might be used as a pigment. A solution of sucrate of copper absorbs carbonic acid from the air with great avidity.

The sucrate of copper dries up into transparent films of an emerald green colour. These films are not altered in appearance or dissolved in cold or boiling alcohol. In water they are resolved into sugar and the pectous subsucrate of copper.

Sucrate of Peroxide of Iron.—The perchloride of iron with an addition of sugar is not precipitated by potash, provided the temperature is not allowed to rise. The peroxide of iron combined with the sugar is colloidal, and remains on the dialyser without loss. At a certain stage, however, the sugar appears to leave the peroxide of iron, and a gelatinous subsucrate of iron pectizes. The subsucrate of iron thrown down from the soluble sucrate, by the addition of sulphate of potash, consisted of about 22 parts of sugar to 78 parts of peroxide of iron.

Sucrate of Peroxide of Uranium.—A similar solution may be obtained by adding potash to a mixture of the nitrate or chloride of uranium with sugar, avoiding heat. The solution is of a deep orange-yellow colour, and on the dialyser soon loses the whole of its acid and alkali. This fluid sucrate has considerable stability, but is readily pectized by salts, like the sucrate of copper. The subsucrate pectized has considerable solubility in pure water.

Sucrate of Lime.—The well-known solution of lime in sugar forms a solid coagulum when heated. It is probably, at a high temperature, entirely colloidal. The solution obtained on cooling passes through the septum, but requires a much longer time than a true crystalloid like the chloride of calcium.

The blue solution of tartrate of copper in caustic potash contains a colloidal compound, which has not been fully examined.

Soluble Chromic Oxide.—The definite terchloride of chromium, being a crystalloid, diffuses away entirely when placed in solution upon the dialyser. This salt dissolves, with time, a certain portion of freshly-precipitated hydrated chromic oxide, and becomes of a deeper green colour. Such a solution, after dialysis for twenty-two days, contained 8 hydrochloric acid to 92 chromic oxide; and after thirty days, 4·3 acid to 95·7 oxide, or 1 equiv. acid to 10·6 equivs. oxide. After thirty-eight days, the solution gelatinized in part upon the dialyser, and then contained 1·5 acid to 98·5 oxide, or 1 equiv. acid to 31·2 equivs. chromic oxide. This last solution, which may be taken to represent soluble chromic oxide, is of a dark green colour, and admits of being heated, and also of being diluted with pure water without change. It was gelatinized with the usual facility by traces of salts and other reagents which affect colloid solutions, and was then no longer soluble in water, even with the assistance of heat. It appeared to be the green hydrated oxide of chromium, as that substance is usually known. A metachromic oxide may possibly be obtained by heating and dialysing the acetate, but I have not attempted to form it.

Mr. Ordway succeeded in dissolving an excess of the hydrated *uranic oxide* and of *glucina* in the chloride of uranium and of glucinum respectively. The dialysis of such solutions may be reasonably expected to yield soluble uranic oxide and soluble glucina.

It appears, then, that the hydrated peroxides of the aluminous type, when free, are

colloid bodies; that two species of each of these hydrated oxides exist, of which alumina and metalumina are the types; one derived from an unchanged salt, and the other from the heated acetate of the base; further, that each of these species has two forms, one soluble and the other insoluble, or coagulated. This last species of duality should be well distinguished from the preceding allotropic variability of the same peroxide. The possession of a soluble and an insoluble (fluid and pectous) modification is not confined to hydrated silicic acid and the aluminous oxides, but appears to be very general, if not universal, among colloid substances. The double form is typified in the fibrin of blood.

The precipitated and gelatinous *peroxide of tin* is largely soluble in the bichloride of the same metal. Such a solution, when placed in the dialyser, allows the whole chlorine of the salt and a portion of the tin to diffuse away. Peroxide of tin, or stannic acid, remains behind, but not in a soluble state. It forms in the dialyser a semitransparent gelatinous cake, which after a few days is entirely free from chlorine. The original solution, containing excess of stannic acid, was diluted to various degrees, but was dialysed always with the same result. The coagulum was insoluble in hot or cold water, but dissolved readily in dilute acids. It was evidently the peroxide of tin unaltered.

The *metastannic acid*, or nitric acid peroxide of tin of BERZELIUS, forms a solid compound with a small quantity of hydrochloric acid. This compound is not dissolved by an excess of acid, but is soluble in pure water. The solution placed in the dialyser is readily decomposed, and leaves behind a semitransparent gelatinous mass of pure hydrated metastannic acid, insoluble both in water and acids. There appears, then, to be no soluble form of either hydrated stannic or metastannic acid, although both are colloidal substances.

Precipitated *titanic acid* was dissolved in hydrochloric acid and submitted to dialysis. The hydrochloric acid readily diffused away, leaving hydrated titanic acid, gelatinous and insoluble, upon the dialyser. The proportion of titanic acid, which escaped from the dialyser and was lost, amounted to 0·050 gramme out of 2·5 grammes. Titanic acid thus resembles stannic acid in not presenting itself in the form of a fluid colloid.

Metallic protoxides are not soluble in their neutral salts, and cannot therefore be submitted to dialysis in the same conditions as the preceding peroxides. It was observed, however, that oxide of copper and oxide of zinc, when dissolved in ammonia, are capable of diffusing through a colloidal septum, and are therefore not colloids themselves. The water outside the dialyser should be charged with ammonia in such an experiment.

5. *Dialysis of Organic Colloid Substances.*

Tannin.—The tannin employed was that extracted from gall-nuts by the ether process of PELOUZE. A two per cent. solution of this substance, covering a surface of paper-parchment of the area of about $\frac{1}{100}$th of a square metre, or 15·6 square inches, to a

depth of 10 millimetres, was diffused at 10° to 13° of temperature. The diffused matter amounted, in successive periods of twenty-four hours, to ·073, ·040, ·021, ·021, ·024 and ·024 gramme, derived from the two grammes in solution. Probably the earlier diffusates were increased by the presence of a little gallic acid, which, being a crystalloid, would no doubt be rapidly eliminated by diffusion. The latter observations indicate that tannin passes through a paper-parchment septum about 200 times less rapidly than chloride of sodium does, in similar circumstances as to temperature and strength of solution. The diffusates from the tannin solution gave a precipitate with gelatin, and therefore contained tannin unaltered. But the diffusates probably contained also throughout some products of decomposition of a crystalloid character.

To the low diffusibility of tannin may be ascribed the remarkably slow penetration of skins by that substance in the ordinary operation of tanning leather. Tannin appears to form compounds of much stability with certain other colloids, as tanno-gelatine, and the compound with albumen which appears to be the primary basis of the vegetable cell (Frémy).

Gum.—The diffusate obtained from a solution containing 2 grammes of gum-arabic, in experiments corresponding in their conditions with the experiments upon tannin just related, was ·013 gramme per day. The power of gum to penetrate the colloid septum appears, therefore, to be one-half less than that of tannin, and 400 times less than the diffusibility of chloride of sodium. Gum gave the same amount of diffusate with a mucus septum as with parchment-paper. When substances of the crystalloid class are mixed with the gum, the diffusion of the latter appears to be still further reduced, and may even be entirely extinguished. The separation of colloids from crystalloids by dialysis is, in consequence, generally more complete than might be expected from the relative diffusibility of the two classes of substances.

Vegetable gum, which Frémy has shown to be a gummate of lime, can be purified by a dialytic method, which may be found applicable with advantage in other cases. Oxalic acid, it is known, precipitates lime from the gum very imperfectly. Hydrochloric acid may be used to separate that base from a solution of gum placed upon the dialyser, with more effect. It is only necessary to add to a strong solution of gum 4 or 5 per cent. of hydrochloric acid, and to dialyse till the gum solution gives no precipitate with nitrate of silver. In an experiment made upon a 20 per cent. solution of gum, the ash was reduced to 0·1 per cent. of the gum in five days. The gummic acid possesses a sensible acid reaction, about equal to that of carbonic acid. This acid reaction was neutralized in 100 parts of gummic acid by 2·85 parts of potash. This amount of potash is very nearly equivalent to the lime originally present in the gum (1·72 lime, or 3·07 carbonate of lime, being equivalent to 2·89 potash). When the gummate of potash itself was dialysed without addition, the potash gradually diffused away, possibly in the state of carbonate, and left the gum again possessed of an acid reaction. Gummic acid, well-dried at 100°, becomes insoluble in water, but swells up in that liquid, like gum-tragacanth. We appear to have here the pectous form of gummic acid.

It is worthy of inquiry whether such native gums as are insoluble in water are not the pectous form of soluble gum, rather than allotropic varieties of that substance. So also of the metagummic acid of FRÉMY, formed by the action of strong sulphuric acid on mucilage. This last substance is insoluble in water, but was found by FRÉMY to afford, when neutralized by lime and alkalies, a soluble gum undistinguishable from gum-arabic.

Gummic acid produces a remarkable compound with gelatine. When solutions of these two colloids are mixed, oily drops fall and form a nearly colourless jelly on standing. This jelly is very fusible, melting at 25°, or by the heat of the hand. The *gummate of gelatine* may be washed without decomposition, but is soluble to a certain extent in pure water, and still more so in a solution of gelatine. Prepared with gummic acid in excess, the compound, when dried at 100°, consisted of 100 gummic acid with 59 gelatine. The drops and the jelly contained 83·5 per cent. of water. Solution of gelatine is not precipitated by unpurified gum, nor by the gummate of potash.

Dextrin.—A two per cent. solution of dextrin, prepared from starch, was diffused in the same conditions as the preceding substances, but through a mucus septum. It gave in twenty-four hours ·034 gramme of diffusate from 2 grammes, or about three times more diffusate than was given by gum-arabic.

Caramel.—The dialytic examination of this substance adds to the accurate information on the subject lately supplied by M. A. GÉLIS *, and places caramel indisputably in the colloid class. The crude caramel obtained by heating cane-sugar at 210°—220°, when placed on the dialyser, allows certain intermediate coloured substances (Caramelane and Caramelene of GÉLIS) to diffuse out with considerable facility, while the compound containing the largest proportion of carbon remains behind. The latter substance, as obtained by me, possessed five times the colouring power of the original crude caramel, weight for weight. This highest soluble member of the caramel series may also be obtained, more quickly, by precipitation from its aqueous solution by means of alcohol. But I found it necessary to repeat the precipitation four times, or till the mass thrown down, from being plastic at first became pulverulent. A solution containing 10 per cent. of the caramel so purified is gummy; and on standing, it formed a tremulous jelly entirely soluble in hot or cold water. Evaporated *in vacuo*, the solution dries up into a black shining mass, which is tough and elastic, while it still possesses a certain proportion of water, like gum containing some water. Once thoroughly dried at a low temperature, this soluble caramel may be heated, afterwards, to 120° and retain complete solubility. But if a solution of the same caramel be directly evaporated to dryness by the heat of a water-bath, the whole matter is rendered insoluble in hot or cold water. The soluble and insoluble caramel have the same composition, and appear to illustrate the usual double form of colloids. The proportion of carbon in the fluid caramel was found as high as 54·59 per cent., which comes nearer to $C_{24}H_{15}O_{15}$ (requiring C 55·17) than any other formula in which the oxygen and hydrogen are assumed to be present in the pro-

* Annales de Chimie, &c., sér. 3. t. lii. p. 352.

portion of water. In the analysis by GÉLIS of his carameline, the proportion of carbon did not exceed 51·33 per cent., which does not apply to the present substance.

Fluid caramel is wholly tasteless, and appears to be neutral. It exhibits the same excessive sensibility to crystalloidal reagents which is witnessed in fluid silicic acid and alumina. The solution is precipitated or pectized by mere traces of any mineral acid, by alkaline sulphates, chloride of sodium, by most other salts, and by alcohol. The caramel then forms a brownish black pulverulent substance, insoluble in hot or cold water. The presence of sugar and of the intermediate brown substances protects fluid caramel in a remarkable way from the action of crystalloids, and accounts for the preceding properties not being observed in crude caramel. This colloid appears also to be precipitated by certain substances of its own class, such as peroxide of iron.

Pectous caramel may readily have its solubility restored. Placed in dilute potash, the caramel swells and appears gelatinous, and is dissolved on the application of heat. When this solution is dialysed, the potash is quickly reduced to the proportion of about 9 per cent., which forms a neutral compound. If an excess of acetic acid now be added, the whole potash is soon diffused away, and pure soluble caramel remains on the dialyser. Even carbonic acid will carry away the potash.

The extremely low diffusibility which has been assigned to caramel in former Tables, belongs to that substance as last described; the brown intermediate substances which accompany it in crude caramel being considerably more diffusive, although they again are much less diffusive than any variety of crystallizable or uncrystallizable sugar. When the molasses of the cane-sugar are diffused, much the greater portion of the colouring matters remains in the dialyser.

With the parchment-paper septum the fluid caramel appeared even less dialysable than gum, the diffusate in twenty-four hours from a 2 per cent. solution of the former being ·009 gramme only, while that of the latter was ·013. Caramel may be stated, approximately, to be 600 times less dialysable than chloride of sodium, and 200 times less so than sugar. Hence liquids coloured with caramel, such as porter and coffee, may be dialysed for a day with the passage of very little colouring matter.

Before leaving caramel, the analogy may be referred to which the insoluble form of that substance presents to *coal*. Caramelization appears the first step in that direction, —the beginning of a colloidal transformation to be consummated in the slow lapse of geological ages.

Albumen.—The purification of albumen is effected with much advantage upon the dialyser. The solution of egg-albumen is mixed freely with acetic acid and then dialysed. The earthy and alkaline salts are speedily got rid of, and in three or four days the albumen burns without leaving a trace of ash. Although the acetic acid used in the process appears to diffuse off entirely, albumen prepared in the manner described has a faint acid reaction. It also coagulates milk when mixed with the latter and heated. Albumen so prepared retains its constituent sulphur.

The passage through parchment-paper of pure albumen prepared by the unobjection-

able process of M. Wurtz is so slow, that several days are required to produce a sensible result. Thus the diffusate from a solution of 2 grammes of albumen in 50 grammes of water was 0·052 gramme in eleven days, which gives 0·005 gramme in a single day. Albumen, then, appears to be about 2½ times less dialysable than gum, and 1000 times less so than chloride of sodium.

Even combination with an alkali does not appear to enable albumen to pass through the colloid septum. To half a gramme of pure albuminic acid dissolved in 50 grammes of water, ·05 gramme of hydrate of soda was added (one-tenth of the weight of the albumen), and the liquid was placed upon parchment-paper. No albumen could be discovered in the diffusate of several days, but it gave ·069 gramme of carbonate of soda, equivalent to ·053 gramme of hydrate of soda; that is the whole soda originally added to the albumen. The separation of the soda from the albumen may possibly have been aided by the presence of carbonic acid in the water, but certainly the entire separation of the alkali from albumen by diffusion through a colloidal film is a remarkable fact. Hydrate of potash was found to diffuse away from albumen in the same manner.

A solution of *Emulsin* is precipitated by albuminic and gummic acids, but not by unpurified albumen or gum-arabic. The precipitates are white and opaque, pulverulent, and not gelatinous. They are soluble in acetic acid.

A thin stratum of pure albumen coagulated by heat appears to intercept completely the passage of liquid albumen of the egg. Forty grammes of undiluted egg-albumen, representing 5·6 grammes of dry albumen, were placed on a dialyser of the small size, composed of two sheets of calico well-impregnated with albumen and coagulated by heat of steam, as in the albumenized osmometer*. After twelve days the volume of liquid within the instrument had increased to 117 grammes by osmose, while a diffusate had passed through the dialyser of 0·243 gramme, or 4·34 per cent. of the original dry albumen. This diffusate consisted of salts chiefly, with some organic matter, but no portion of the latter was coagulable by heat.

Neither gelatinous starch, animal gelatine dissolved in water, nor extract of flesh appears to be capable of diffusing through a colloid septum in a sensible degree, although salts and other crystallizable substances, which are mixed with the former, diffuse through the septum readily, and may thus be separated from the former substances.

6. *Separation of Arsenious Acid from Colloidal Liquids.*

Dialysis may be advantageously applied to the separation of arsenious acid and metallic salts from organic solutions in medico-legal inquiries. The process has the advantage of introducing no metallic substance or chemical reagent of any kind into the organic fluid. The arrangement for operating is also of the simplest nature.

The organic fluid is placed, to the depth of half an inch, on a dialyser formed of a hoop of gutta percha 10 or 12 inches in diameter, covered with parchment-paper (fig. 1, page 186). The dialyser is then floated in a basin containing a volume of water about

* Philosophical Transactions, 1854, p. 189.

four times greater than the volume of organic fluid in the dialyser. The water of the basin is generally found to remain colourless after the lapse of twenty-four hours, and after being concentrated by evaporation, it admits of the application of the proper reagents to precipitate and remove a metal from solution. One-half to three-fourths of the crystalloidal and diffusible constituents of the organic fluid will generally be found in the water of the basin.

In the few illustrative experiments which follow, the 4-inch bulb dialyser, having an area of 16 square inches, or about $\frac{1}{100}$dth part of a square metre, was generally made use of (fig. 3, p. 201). The volume of liquid placed in the bulb was 50 cubic centimetres, and accordingly covered the dialyser to a depth of 5 millimetres, or about 0·2 inch. The outer volume of water (in the jar) was not less than 1 litre, or twenty times the volume of the solution on the dialyser.

1. A solution of arsenious acid, in pure water, was first placed on the dialyser, the water containing 0·5 per cent. of arsenious acid, or 0·25 gramme of that substance, for twenty-four hours. The dialyser being then removed, the outer fluid was concentrated by heat, and then precipitated by sulphuretted hydrogen. It gave 0·300 gramme of tersulphide of arsenic, equivalent to 0·241 gramme of arsenious acid. It appears, then, that about 95 per cent. of the arsenious acid had diffused from the dialyser into the water-jar in twenty-four hours.

2. Water, with one-fourth of its volume of fluid egg albumen and 0·25 gramme, or 0·5 per cent. of arsenious acid, was now placed on the dialyser as before. The diffusate gave, with sulphuretted hydrogen, after being acidulated with hydrochloric acid, 0·267 gramme of tersulphide of arsenic, equivalent to 0·214 gramme of arsenious acid.

3. The water contained 10 per cent. of gum-arabic and 1 per cent. arsenious acid, the latter amounting to 0·5 gramme. From the diffusate was derived 0·505 gramme of tersulphide of arsenic, equivalent to 0·406 gramme of arsenious acid. The dialyser still gave out arsenious acid when immersed for a second day in water. The outer fluid contained no gum.

It may be added that a similar 1 per cent. solution of arsenious acid, without the gum, gave a diffusate of 0·45 gramme arsenious acid in the same time, that is, nine-tenths of the whole acid.

4. A solution in hot water of 1 per cent. isinglass and 0·5 per cent. of arsenious acid (0·25 gramme), formed a jelly upon the dialyser on cooling. The diffusate from this jelly gave 0·260 tersulphide of arsenic, equivalent to 0·209 arsenious acid, with no gelatine. The escape of the arsenious acid appears then to have been slightly retarded by the fixing of the gelatinous solution. This is probably due to the arrest of mechanical movement within the gelatinous stratum, and not to any sensible impediment offered by the jelly to diffusion.

In another experiment, similar to the last, but continued for four days instead of twenty-four hours, the tersulphide of arsenic weighed 0·320 gramme, equivalent to 0·257 arsenious acid.

5. A quantity of white of egg, amounting to 50 grammes, to which 0·01 gramme of arsenious acid in solution had been added, was coagulated by heat. The solid mass was then cut up into small pieces and placed on the dialyser, mixed with 50 grammes of water; after the usual period of twenty-four hours, the diffusate gave 0·01 gramme of tersulphide of arsenic, equivalent to 0·008 gramme arsenious acid. Here, of the mass upon the dialyser, the arsenious acid formed only $\frac{1}{10,000}$dth part, yet four-fifths of it are recovered.

6. One hundred grammes of milk, charged with $\frac{1}{10,000}$dth part of arsenious acid (0·01 gramme), and forming a stratum on the dialyser of 10 millimetres, gave a diffusate which yielded 0·010 tersulphide of arsenic, equivalent to 0·008 gramme of arsenious acid. The outer liquid was colourless, and gave no indication of casein, but it contained of course the salts and the sugar of the milk.

7. The same experiment was repeated with sized writing-paper, as the septum, applied to the same bulb. The result was a slight increase in the quantity of arsenious acid recovered.

It appears, then, that arsenious acid separates on the dialyser from gum, from gelatine, albumen, fluid or coagulated, and from casein, and is obtained in a solution fit for the application of reagents.

8. Half a litre of dark-coloured porter, with 0·05 gramme of arsenious acid added ($\frac{1}{10,000}$dth part of arsenious acid) was placed on a hoop dialyser, 8 inches in diameter, and the whole floated in an earthenware basin containing 2 or 3 litres of water. After twenty-four hours the latter fluid had acquired a slight tinge of yellow. It yielded, when concentrated and precipitated by sulphuretted hydrogen, upwards of one-half of the original arsenious acid in a fit state for examination.

9. In a similar experiment on 200 grammes of defibrinated blood charged with $\frac{1}{40,000}$dth part of arsenious acid (0·05 gramme), and placed in a similar dialyser to the last for twenty-four hours, the diffusate of arsenious acid was recovered with the same facility, and appeared to be equally considerable.

10. Animal intestines, charged with the usual minute proportion of arsenious acid, were cut into small pieces and digested in water, about 32° C., for twenty-four hours. The whole was then thrown upon a dialyser for an equal time. Arsenious acid diffused out so free from colloidal matter that the action of reagents was not interfered with. A high temperature in digesting the intestines is quite unnecessary, and appeared indeed to increase the difficulty of diffusing out the arsenious acid afterwards.

The *tartrate of potash and antimony*, mixed in the small proportion of $\frac{1}{10,000}$dth, with defibrinated blood and with milk, was separated by dialysis quite as effectually as arsenious acid above.

Strychnine also was separated from organic fluids in the same manner, a small addition of hydrochloric acid being first made to the fluid on the dialyser.

Dialysis then appears of general application in the preparation of a liquid for examination by chemical tests, whether the poison looked for be mineral or organic. All

soluble poisonous substances, whatever their origin, appear to be crystalloids, and accordingly pass through colloidal septa.

7. *Colloidal Condition of Matter.*

I may be allowed to advert again to the radical distinction assumed in this paper to exist between colloids and crystalloids in their intimate molecular constitution. Every physical and chemical property is characteristically modified in each class. They appear like different worlds of matter, and give occasion to a corresponding division of chemical science. The distinction between these kinds of matter is that subsisting between the material of a mineral and the material of an organized mass.

The colloidal character is not obliterated by liquefaction, and is therefore more than a modification of the physical condition of solid. Some colloids are soluble in water, as gelatine and gum-arabic; and some are insoluble, like gum-tragacanth. Some colloids, again, form solid compounds with water, as gelatine and gum-tragacanth, while others, like tannin, do not. In such points the colloids exhibit as great a diversity of property as the crystalloids. A certain parallelism is maintained between the two classes, notwithstanding their differences.

The phenomena of the solution of a salt or crystalloid probably all appear in the solution of a colloid, but greatly reduced in degree. The process becomes slow; time, indeed, appearing essential to all colloidal changes. The change of temperature, usually occurring in the act of solution, becomes barely perceptible. The liquid is always sensibly gummy or viscous when concentrated. The colloid, although often dissolved in a large proportion by its solvent, is held in solution by a singularly feeble force. Hence colloids are generally displaced and precipitated by the addition to their solution of any substance from the other class. Of all the properties of liquid colloids, their slow diffusion in water, and their arrest by colloidal septa, are the most serviceable in distinguishing them from crystalloids. Colloids have feeble chemical reactions, but they exhibit at the same time a very general sensibility to liquid reagents, as has already been explained.

While soluble crystalloids are always highly sapid, soluble colloids are singularly insipid. It may be questioned whether a colloid, when tasted, ever reaches the sentient extremities of the nerves of the palate, as the latter are probably protected by a colloidal membrane, impermeable to soluble substances of the same physical constitution.

It has been observed that vegetable gum is not digested in the stomach. The coats of that organ dialyse the soluble food, absorbing crystalloids and rejecting all colloids. This action appears to be aided by the thick coating of mucus which usually lines the stomach.

The secretion of free hydrochloric acid during digestion—at times most abundant—appears to depend upon processes of which no distinct conception has been formed. But certain colloidal decompositions are equally inexplicable upon ordinary chemical views. To facilitate the separation of hydrochloric acid from the perchloride of iron,

for instance, that salt is first rendered basic by the addition of peroxide of iron. The comparatively stable perchloride of iron is transformed, by such treatment, into a feebly-constituted colloidal hydrochlorate. The latter compound breaks up under the purely physical agency of diffusion, and divides on the dialyser into colloidal peroxide of iron and free hydrochloric acid. The super-induction of the colloidal condition may possibly form a stage in many analogous organic decompositions.

A tendency to spontaneous change, which is observed occasionally in crystalloids, appears to be general in the other class. The fluid colloid becomes pectous and insoluble by contact with certain other substances, without combining with these substances, and often under the influence of time alone. The pectizing substance appears to hasten merely an impending change. Even while fluid a colloid may alter sensibly, from colourless becoming opalescent; and while pectous the degree of hydration may become reduced from internal change. The gradual progress of alteration in the colloid effected by the agency of time, is an investigation yet to be entered upon.

The equivalent of a colloid appears to be always high, although the ratio between the elements of the substance may be simple. Gummic acid, for instance, may be represented by $C_{12}H_{11}O_{11}$, but judging from the small proportions of lime and potash which suffice to neutralize this acid, the true numbers of its formula must be several times greater. It is difficult to avoid associating the inertness of colloids with their high equivalents, particularly where the high number appears to be attained by the repetition of a smaller number. The inquiry suggests itself whether the colloid molecule may not be constituted by the grouping together of a number of smaller crystalloid molecules, and whether the basis of colloidality may not really be this composite character of the molecule.

With silicic acid, which can exist in combination both as a crystalloid and colloid, we have two series of compounds, silicates and cosilicates, the acid of the latter appearing to have an equivalent much greater (thirty-six times greater in one salt) than the acid of the former. The apparently small proportion of acid in a variety of metallic salts, such as certain red salts of iron, is accounted for by the high colloidal equivalent of their bases. The effect of such an insoluble colloid as prussian blue in carrying down small proportions of the precipitating salts, may admit of a similar explanation.

Gelatine appears to hold an important place as a colloidal base. This base unites with colloidal acids, giving a class of stable compounds, of which tanno-gelatine only appears to be hitherto known. Gelatine is precipitated entirely by a solution of metaphosphoric acid added drop by drop, 100 parts of gelatine uniting with 3·6 parts of the acid. The compound formed is a semitransparent, soft, elastic, and stringy solid mass, presenting a startling resemblance to animal fibrin. It will be an interesting inquiry whether metaphosphoric acid is a colloid, and enters into the compound described in that character, or is a crystalloid, as the small proportion and low equivalent of the acid would suggest. Gelatine is also precipitated by carbolic acid.

The hardness of the crystalloid, with its crystalline planes and angles, is replaced in

the colloid by a degree of softness, with a more or less rounded outline. The water of crystallization is represented by the water of gelatination. The water in gelatinous hydrates is aptly described by M. Chevreul as retained by "capillary affinity," that is, by an attraction partaking both of the physical and chemical character. While it is here admitted that chemical affinity of the lowest degree may shade into capillary attraction, it is believed that the character of gelatinous hydration is as truly chemical as that of crystalline hydration. Combination of a colloid with water is feeble, it is true, but so is combination in general with the colloid. Notwithstanding this, anhydrous colloids can decompose certain crystalloid hydrates. The water in alcohol of greater strength than corresponds with the density 0·926, which represents the definite hydrate $C_4 H_6 O_2 + 6 HO$, is certainly in a state of chemical union. But alcohol so high as 0·906, contained in a close vessel, is concentrated in a notable degree by contact with dry mucus, gelatine, and gum, and sensibly even by dry parchment-paper. Dilute alcohol divided from the air of the atmosphere by a dry septum of mucus, gelatine, or gum, is also concentrated by evaporation, as in the well-known bladder experiment of Sömmering. The selective power is here apparent of the colloid for water, that fluid being separated from alcohol, and travelling through the colloidal septum by combination with successive molecules of the latter, till the outer surface is reached and evaporation takes place. The penetration in this manner of a colloid by a foreign substance may be taken as an illustration of the phenomena of cementation. Iron and other substances which soften under heat, may be supposed to assume at the same time a colloidal constitution. So it may be supposed does silica when fused into a glass by heat, and every other vitreous substance.

Gelatinous hydrates always exhibit a certain tendency to aggregation, as is seen in the jelly of hydrated silicic acid and of alumina. With some the jelly is also adhesive, as in glue and mucus. But unless they be soluble in water, gelatinous hydrates, when once formed, are not in general adhesive. Separated masses do not reunite when brought into contact. This want of adhesiveness is very remarkable in the gelose of Payen, which resembles gelatine so closely in other respects. Layers of a gelose solution, allowed to cool and gelatinize in succession in a diffusion-jar (p. 199), do not adhere together.

Ice itself presents colloidal characters at or near its melting-point, paradoxical although the statement may appear. When ice is formed at temperatures a few degrees under 0° C., it has a well-marked crystalline structure, as is seen in water frozen from a state of vapour, in the form of flakes of snow and hoar-frost, or in water frozen from dilute sulphuric acid, as observed by Mr. Faraday. But ice formed in contact with water at 0°, is a plain homogeneous mass with a vitreous fracture, exhibiting no facets or angles. This must appear singular when it is considered how favourable to crystallization are the circumstances in which a sheet of ice is slowly produced in the freezing of a lake or river. The continued extrication of latent heat by ice as it is cooled a few degrees below 0° C., observed by M. Person, appears also to indicate a molecular change subsequent to the first freezing. Further, ice, although exhibiting none of the viscous

softness of pitch, has the elasticity and tendency to rend seen in colloids. In the properties last mentioned, ice presents a distant analogy to gum incompletely dried, to glue, or any other firm jelly. Ice further appears to be of the class of adhesive colloids. The redintegration (regelation of FARADAY) of masses of melting ice, when placed in contact, has much of a colloid character. A colloidal view of the plasticity of ice demonstrated in the glacier movement will readily develope itself.

A similar extreme departure from its normal condition appears to be presented by a colloid holding so high a place in its class as albumen. In the so-called blood-crystals of FUNKE, a soft and gelatinous albuminoid body is seen to assume a crystalline contour. Can any facts more strikingly illustrate the maxim that in nature there are no abrupt transitions, and that distinctions of class are never absolute?

8. *Osmose.*

Little has been said in the present paper respecting osmose, a subject closely connected with colloidal septa. It now appears to me that the water movement in osmose is an affair of hydration and of dehydration in the substance of the membrane or other colloid septum, and that the diffusion of the saline solution placed within the osmometer has little or nothing to do with the osmotic result, otherwise than as it affects the state of hydration of the septum.

Osmose is generally considerable, through membranous and other highly hydrated septa, with the solution of any colloid (gum, for instance) contained in the osmometer. Yet the diffusion outwards of the colloid is always minute, and may sometimes amount to nothing. Indeed, an insoluble colloid, such as gum-tragacanth, placed in powder within the osmometer, was found to indicate the rapid entrance of water to convert the gum into a bulky gelatinous hydrate. Here no outward or double movement is possible.

The degree of hydration of any gelatinous body is much affected by the liquid medium in which it is placed. This is very obvious in fibrin and animal membrane. Placed in pure water, such colloids are hydrated to a higher degree than they are in neutral saline solutions. Hence the equilibrium of hydration is different on the two sides of the membrane of an osmometer. The outer surface of the membrane being in contact with pure water tends to hydrate itself in a higher degree than the inner surface does, the latter surface being supposed to be in contact with a saline solution. When the full hydration of the outer surface extends through the thickness of the membrane and reaches the inner surface, it there receives a check. The degree of hydration is lowered, and water must be given up by the inner layer of the membrane, and it forms the osmose. The contact of the saline fluid is thus attended by a continuous catalysis of the gelatinous hydrate, by which it is resolved into a lower gelatinous hydrate and free water. The inner surface of the membrane of the osmometer contracts by contact with the saline solution, while the outer surface dilates by contact with pure water. Far from promoting this separation of water, the diffusion of the salt throughout the substance of the membrane appears to impede osmose, by equalizing the condition as to

saline matter of the membrane through its whole thickness. The advantage which colloidal solutions have in inducing osmose, appears to depend in part upon the low diffusibility of such solutions, and their want of power to penetrate the colloidal septum.

The substances fibrin, albumen, and animal membrane swell greatly when immersed in water containing minute proportions of acid or of alkali, as is well known. On the other hand, when the proportion of acid or alkali is carried beyond a point peculiar to each substance, contraction of the colloid takes place. Such colloids as have been named acquire the power of combining with an increased proportion of water, and of forming superior gelatinous hydrates, in consequence of contact with dilute acid and alkaline reagents. Even parchment-paper is more elongated in an alkaline solution than in pure water. When so hydrated and dilated, the colloids present an extreme osmotic sensibility. Used as septa, they appear to assume or resign their water of gelatination under influences apparently the most feeble. It is not attempted to explain this varying hydration of colloids with the osmotic effects thence arising. Such phenomena belong to colloidal chemistry, where the prevailing changes in composition appear to be of the kind vaguely described as catalytic. To the future investigation of catalytic affinity, therefore, must we look for the further elucidation of osmose.

XI. *On the Porism of the in-and-circumscribed Polygon.*

By Arthur Cayley, *Esq.*, *F.R.S.*

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The Porism referred to is as follows, viz. that two conics may be so related to each other, that a polygon may be inscribed in the one, and circumscribed about the other conic, in such manner that any point whatever of the circumscribing conic may be taken for a vertex of the polygon. I gave in the year 1853, in the Philosophical Magazine*, a general formula for the relation between the two conics, viz. if $U=0$ is the equation of the inscribed conic, $V=0$ that of the circumscribed conic, and if disct. $(U+\xi V)$, where ξ is an arbitrary multiplier, denotes the discriminant of $U+\xi V$ in regard to the coordinates (x, y, z) (such discriminant being of course a cubic function in regard to ξ, and also in regard to the coefficients of the two conics U, V, jointly), then if we write

$$\sqrt{\text{disct.}}\,(U+\xi V)=A+B\xi+C\xi^2+D\xi^3+E\xi^4+F\xi^5+G\xi^6+\&c.,$$

the relations for the cases of the triangle, pentagon, heptagon, &c. are

$$C=0,\ \begin{vmatrix} C, & D \\ D, & E \end{vmatrix}=0,\ \begin{vmatrix} C, & D, & E \\ D, & E, & F \\ E, & F, & G \end{vmatrix}=0,\ \&c.$$

respectively, while those in the cases of the quadrangle, hexagon, octagon, &c. are

$$D=0,\ \begin{vmatrix} D, & E \\ E, & F \end{vmatrix}=0,\ \begin{vmatrix} D, & E, & F \\ E, & F, & G \\ F, & G, & H \end{vmatrix}=0,\ \&c.$$

respectively. The demonstration of this fundamental theorem is for greater completeness here reproduced; but the chief object of the memoir is to direct attention to a curious analytical theorem which is an easy *à priori* consequence of the Porism, and to obtain the relations for the several polygons up to the enneagon, in a new and simple form which puts in evidence *à posteriori* for these cases the analytical theorem just referred to. The analytical theorem rests upon the following considerations:—the relation for a hexagon ought to include that for a triangle; in fact a triangle with its

* See the papers—"On the Geometrical Representation of the Integral $\int dx \div \sqrt{(x+a)(x+b)(x+c)}$," Phil. Mag. April 1853.

"Note on the Porism of the in-and-circumscribed Polygon," Phil. Mag. August 1853.

"Correction of two Theorems relating to the Porism of the in-and-circumscribed Polygon," Phil. Mag. November 1853.

"Developments of the Porism of the in-and-circumscribed Polygon," Phil. Mag. May 1854.

sides in order twice over is a form of hexagon; the condition for an octagon should in like manner include that for a quadrangle; and so in other cases. Let the cubic function, disct. $(U+\xi V)$, be represented by $1+\beta\xi+\gamma\xi^2+\delta\xi^3$, the coefficients A, B, C, D, E, &c. are functions of β, γ, δ. Write

$$C \quad =(3),$$

$$D \quad =(4),$$

$$\begin{vmatrix} C, & D \\ D, & E \end{vmatrix} \quad =(5),$$

$$\begin{vmatrix} D, & E \\ E, & F \end{vmatrix} \quad =(6),$$

$$\begin{vmatrix} C, & D, & E \\ D, & E, & F \\ E, & F, & G \end{vmatrix} \quad =(7),$$

&c.

then (3), (4), (5) are respectively prime functions of β, γ, δ; that is they cannot be decomposed into factors, rational functions of these quantities; and it is convenient to denote this by writing (3)=[3], (4)=[4], (5)=[5]. But by what precedes, (6) contains the factor (3), that is [3]; and if the other factor, which is prime, is denoted by [6], then we have (6)=[6] [3]. The next term (7) is prime, that is we have (7)=[7]; but the term (8) gives (8)=[8] [4]; the term (9) gives (9)=[9] [3], and so on. Thus we have (12)=[12] [6] [4] [3], the numbers in [ ] being all the factors, the number itself included, and as well composite as prime, of the number in ( ), the factors 2 and 1 being however excluded. To make this clearer, it may be remarked that the last-mentioned equation has the geometrical signification that the relation for a dodecagon is the aggregate of the relations for a proper dodecagon, a proper hexagon, a quadrangle, and a triangle; that is, the relation for a dodecagon implies one or other of the last-mentioned relations. The relations for the several polygons up to the enneagon are in the memoir obtained in a form which puts in evidence the property in question, that is, the series of equations

$$(3)=[3],$$
$$(4)=[4],$$
$$(5)=[5],$$
$$(6)=[6]\,[3],$$
$$(7)=[7],$$
$$(8)=[8]\,[4]$$
$$(9)=[9]\,[3].$$

To do this, the discriminant is represented, not as above in terms of the constants β, γ, δ, but in a somewhat different form, by means of the constants δ, c, d, the last two

whereof are such that $c=0$ is the relation for the triangle, $d=0$ the relation for the quadrangle; thus $[3]=c$, $[4]=d$, and for the particular cases considered, the analytical theorem consists herein, that c is a factor of (6), and of (9), and that d is a factor of (8). I have, for the sake of homogeneity, introduced into the formulæ the quantity a $(=1)$, but this is a matter of form only.

The functions [3], [4], &c. have been spoken of as *prime*; they are so, in fact, as far as they are calculated; and that they are so in general rests on the assumption that for a polygon of a given number of sides, there is but one form of relation: if, for instance, in the equation $[12]=0$, which is the condition for a proper dodecagon, the function [12] could be decomposed into rational factors; then equating each of these factors to zero, we should have so many distinct forms of relation for a proper dodecagon. I believe that the assumption and reasoning are valid; but without entering further into this, I take it for granted that in the general case the functions [3], [4], &c. are in fact prime. But the coefficients β, γ, δ, or b, c, d, instead of being so many independent arbitrary quantities, may be given as rational functions of other quantities (if, for instance, the two conics are circles, radii R, r, and distance between the centres a, then β, γ, δ will be functions of R, r, a): and it is in a case of this kind quite conceivable that the functions [3], [4], &c., considered as functions of these new elements, should cease to be prime functions. In fact, in the case just referred to of the two circles (the original case of the Porism as considered by Fuss), the functions [4], [6], &c., which correspond to a polygon of an *even* number of sides, appear to be each of them decomposable into two factors: the memoir contains some remarks tending to show *à priori* that in the case in question this decomposition takes place. I was led to examine the point by the elegant formulæ obtained in an essentially different manner by M. Mention, Bull. de l'Acad. de St. Pét. t. i. pp. 15, 30, and 507 (1860), in reference to the case of the two circles (it thereby appears that the decomposition takes place for the quadrangle and the hexagon); and these formulæ are reproduced in the memoir.

I.

Demonstration of the general Formula of the Relation between the two Conics.

The equation of a conic passing through the points of intersection of the conics

$$U=0,\ V=0,$$

is of the form

$$U+mV=0,$$

where m is an arbitrary parameter. Suppose that the conic touches a given line, we have for the determination of m a quadratic equation; and conversely, if the roots of this quadratic equation are given, the line is also given; that is, the two roots may be considered as parameters which determine the particular line.

Let k be a given value of m; the parameters of any tangent of the conic $U+kV=0$ are k, p, but as k is common to all the tangents, we may consider the particular tangent

as determined by the single parameter p. And a point of the conic $U+kV=0$ may be considered as determined by the same parameter p which determines the tangent at that point.

As regards the conic $V=0$, the common parameter for all its tangents is ∞, and we may consider any other tangent of this conic as determined by the parameter θ, and a point of the conic as determined by the same parameter θ.

Suppose, in the first instance, that the two conics are

$$U = ax^2+by^2+cz^2=0,$$
$$V = x^2+y^2+z^2=0;$$

the equation of the tangent of $U+kV=0$, the parameter whereof is p (in fact a common tangent of the conics $U+kV=0$, $U+pV=0$), is easily found to be

$$x\sqrt{b-c}\sqrt{a+k}\sqrt{a+p}+y\sqrt{c-a}\sqrt{b+k}\sqrt{b+p}+z\sqrt{a-b}\sqrt{c+k}\sqrt{c+p}=0;$$

and if this meet the conic $V=0$ in the points P, P', the parameters whereof are ∞, θ, and ∞, θ', or say θ and θ' respectively, then the coordinates of the point P are given by

$$x:y:z=\sqrt{b-c}\sqrt{a+\theta}:\sqrt{c-a}\sqrt{b+\theta}:\sqrt{a-b}\sqrt{c+\theta};$$

and substituting these values in the foregoing equation, we have

$$(b-c)\sqrt{a+k}\sqrt{a+p}\sqrt{a+\theta}+(c-a)\sqrt{b+k}\sqrt{b+p}\sqrt{b+\theta}+(a-b)\sqrt{c+k}\sqrt{c+p}\sqrt{c+\theta}=0$$

as an equation connecting the parameters p and θ. This equation may be replaced by

$$\sqrt{(a+k)(a+p)(a+\theta)}=\lambda+\mu a,$$
$$\sqrt{(b+k)(b+p)(b+\theta)}=\lambda+\mu b,$$
$$\sqrt{(c+k)(c+p)(c+\theta)}=\lambda+\mu c,$$

from which λ, μ are to be eliminated; and squaring and reducing, we have

$$\lambda^2=abc+kp\theta,$$
$$-2\lambda\mu=bc+ca+ab-(p\theta+kp-k\theta),$$
$$\mu^2=a+b+c+k+p+\theta,$$

and thence

$$(bc+ca+ab-p\theta-kp-k\theta)^2-4(a+b+c+k+p+\theta)(abc+kp\theta)=0$$

as the rational form of the original equation. But the same rational equation would, it is clear, be obtained from the system

$$\sqrt{(k+a)(k+b)(k+c)}=L+Mk,$$
$$\sqrt{(p+a)(p+b)(p+c)}=L+Mp,$$
$$\sqrt{(\theta+a)(\theta+b)(\theta+c)}=L+M\theta,$$

by the elimination of L and M. And it follows from ABEL's theorem (but the result might be verified by means of EULER's fundamental theorem for the addition of elliptic functions), that if

$$\Pi\xi = \int_{\infty} \frac{d\xi}{\sqrt{(\xi+a)(\xi+b)(\xi+c)}},$$

then that the last-mentioned system is equivalent to the transcendental equation

$$\Pi\theta = \Pi p \pm \Pi k,$$

in which the arbitrary constant which should have been inserted, and the sign of $\Pi\theta$, are determined by the consideration that for $k=\infty$ (which gives $\Pi k=0$) we ought to have $\theta=p$, and therefore $\Pi\theta=\Pi p$.

There is of course a similar equation in θ', and the terms with Πk must be taken with opposite signs, and we have thence the theorem,

"If θ, θ' are the parameters of the points P, P' in which the conic $V=0$ is intersected by the tangent, the parameter whereof is p, of the conic $U+kV=0$, then the equations

$$\Pi\theta = \Pi p - \Pi k,$$
$$\Pi\theta' = \Pi p + \Pi k,$$

determine the parameters θ, θ' of the points in question." And again,

"If the two variable parameters θ, θ' are connected by the equation

$$\Pi\theta' - \Pi\theta = 2\Pi k,$$

then the line PP' will be a tangent of the conic $U+kV=0$."

The foregoing demonstration relates to the particular forms $U=ax^3+by^3+cz$, $V=x^3+y^3+z^3$; but observing that the function $\sqrt{(\xi+a)(\xi+b)(\xi+c)}$, which enters under the integral sign in the transcendental function $\Pi\xi$, is the square root of the discriminant of $U+\xi V$, the theory of covariants shows at once that the conclusions apply to any forms whatever of U, V, the expression for the transcendental function being

$$\Pi\xi = \int_{\infty} \frac{d\xi}{\sqrt{\text{disct. } (U+\xi V)}}.$$

Consider now a triangle inscribed in the conic $V=0$, and with its sides touching the conics

$$U+kV=0,$$
$$U+k'V=0,$$
$$U+k''V=0.$$

Then if θ, θ', θ'' are the parameters of the angles, we have

$$\Pi\theta'' - \Pi\theta' = 2\Pi k,$$
$$\Pi\theta - \Pi\theta'' = 2\Pi k',$$
$$\Pi\theta' - \Pi\theta = 2\Pi k'',$$

and thence

$$\Pi k + \Pi k' + \Pi k'' = 0$$

as the relation which must subsist between the parameters k, k', k'', of the conics touched by the sides; and similarly for a polygon of n sides, the relation between the parameters is

$$\Pi k_1 + \Pi k_2 + \ldots + \Pi k_n = 0.$$

But by ABEL's theorem, this transcendental equation is equivalent to an algebraical one.

In fact, calling the radical $\sqrt{\square x}$, then if ϕx, χx are rational and integral functions of x with arbitrary coefficients, and if

$$\phi^2 x - \chi^2 x \square x = A(x-k_1)(x-k_2)\ldots(x-k_n),$$

(this implies that $\phi^2 x$ is of a degree not exceeding n and $\chi^2 x$ of a degree not exceeding $n-3$; that is, for n even the degrees of ϕx, χx are $\frac{1}{2}n$, $\frac{1}{2}(n-4)$; and for n odd they are $\frac{1}{2}(n-1)$, $\frac{1}{2}(n-3)$), then the algebraical equation is that obtained by the elimination of the arbitrary coefficients from the system of equations

$$\phi k_1 + \chi k_1 \square k_1 = 0,$$
$$\phi k_2 + \chi k_2 \square k_2 = 0,$$
$$\ldots\ldots\ldots\ldots$$
$$\phi k_n + \chi k_n \square k_n = 0;$$

or, what is the same thing, for n odd $=2p-1$, it is

$$\{1, \theta, \ldots \theta^{p-1}, \sqrt{\square\theta}, \theta\sqrt{\square\theta}, \ldots \theta^{p-2}\sqrt{\square\theta}\} = 0,$$

and for n even, $=2p$, it is

$$\{1, \theta, \ldots \theta^{p}, \sqrt{\square\theta}, \theta\sqrt{\square\theta}, \ldots \theta^{p-2}\sqrt{\square\theta}\} = 0,$$

where the expressions in $\{\ \}$ denote respectively the determinants, of $2p-1$ lines, and $2p$ lines, formed by substituting for θ the values $k_1, k_2, \ldots k_n$ respectively. Thus for $n=3$, the equation is

$$\begin{vmatrix} 1, & k_1, & \sqrt{\square k_1} \\ 1, & k_2, & \sqrt{\square k_2} \\ 1, & k_3, & \sqrt{\square k_3} \end{vmatrix} = 0;$$

and for $n=4$, it is

$$\begin{vmatrix} 1, & k_1, & k_1^2, & \sqrt{\square k_1} \\ 1, & k_2, & k_2^2, & \sqrt{\square k_2} \\ 1, & k_3, & k_3^2, & \sqrt{\square k_3} \\ 1, & k_4, & k_4^2, & \sqrt{\square k_4} \end{vmatrix} = 0,$$

and so on.

Suppose

$$\sqrt{\square\xi} = A + B\xi + C\xi^2 + D\xi^3 + E\xi^4 + \ldots;$$

then substituting the corresponding expressions for $\sqrt{\square k_1}$, $\sqrt{\square k_2}$, &c., the determinant will divide by $\{1, \theta, \theta^2, \ldots \theta^{p-1}\}$, and it may be seen without difficulty that the resulting equation on putting therein $k_1 = k_2 \ldots = k_n = 0$, will, according as $n=3$, 4, 5, 6, &c., be

$$C=0,\ D=0,\ \begin{vmatrix} C, & D \\ D, & E \end{vmatrix} = 0,\ \begin{vmatrix} D, & E \\ E, & F \end{vmatrix} = 0,\ \begin{vmatrix} C, & D, & E \\ D, & E, & F \\ E, & F, & G \end{vmatrix} = 0, \text{ &c.},$$

which is the theorem above referred to.

II.

Application to the several Polygons up to the Enneagon.

If the equation of the inscribed conic is $U=0$, and that of the circumscribed conic is $V=0$, and if the discriminant $(U+\xi V)$ is in the first instance represented by $1+4\beta\xi+4\gamma\xi^2+4\delta\xi^3$, then the square root of the discriminant is

$$1+2\beta\xi+2(\gamma-\beta^2)\xi^2+2(\delta-2\beta\gamma+2\beta^3)\xi^3+\&c.,$$

so that the condition for the triangle is

$$\gamma-\beta^2=0,$$

and that for the quadrangle is

$$\delta-2\beta\gamma+2\beta^3=0.$$

It is obviously convenient to introduce into the formulæ, in the place of γ and δ, the quantities

$$c=\gamma-\beta^2,$$
$$d=\delta-2\beta\gamma+2\beta^3;$$

and writing also, for symmetry of notation, b in the place of β, we have

$$\beta=b,$$
$$\gamma=c+b^2,$$
$$\delta=d+2bc,$$

so that the discriminant will be

$$=1+4b\xi+4(c+b^2)\xi^2+4(d+2bc)\xi^3,$$

which is

$$=(1+2b\xi+2c\xi^2)^2+4(d\xi^3-c^2\xi^4).$$

But for homogeneity I introduce the quantity $a=1$, and put the discriminant

$$=(1+2b\xi+2ac\xi^2)^2+4a^3(d\xi^3-c^2\xi^4).$$

The square root, divided by a^2, is

$$=\tfrac{1}{a^2}(1+2b\xi+2ac\xi^2)\sqrt{1+4a^3(d\xi^3-c^2\xi^4)\div(1+2b\xi+2ac\xi^2)^2};$$

or developing, this is

$$\begin{aligned}
&\tfrac{1}{a^2}(1+2b\xi+2ac\xi^2)\\
&+\ 2\ (d\xi^3-c^2\xi^4)\div(1+2b\xi+2ac\xi^2)\\
&-\ 2a^3(d\xi^3-c^2\xi^4)^2\div(1+2b\xi+2ac\xi^2)^3\\
&+\ 4a^4(d\xi^3-c^2\xi^4)^3\div(1+2b\xi+2ac\xi^2)^4\\
&-10a^6(d\xi^3-c^2\xi^4)^4\div(1+2b\xi+2ac\xi^2)^5\\
&+\ \&c.;
\end{aligned}$$

or representing this by $1+2B\xi+2C\xi^2+2D\xi^3+$ &c., we have

$$C\xi^2+D\xi^3+E\xi^4+F\xi^5+G\xi^6+H\xi^7+I\xi^8+J\xi^9+K\xi^{10}+L\xi^{11}+M\xi^{12}+\cdots$$

$=a^{-1}c\xi^2$

$+(d\xi^3-c^2\xi^4)\left\{\begin{array}{l|l|l|

we have, up to 45, which is all that is required,—

$12=a^{-1}\times$	$13=a^{-1}\times$	$14=a^{-1}\times$	$15=a^{-1}\times$	$23=$	$24=$	$25=$	$34=$	$35=$	$45=$
$-1\ ad^2$	$+2\ abd^2$	$+1\ a^2cd^2$	$+1\ a^3d^3$	$-2\ acd^2$	$-1\ a^2d^2$	$+4\ a^2bd^2$	$+2\ a^2bd^2$	$-2\ a^3cd^2$	$-1\ a^4d^4$
$-2\ bcd$	$-1\ ac^2d$	$-4\ ab^2d^2$	$-2\ a^2bcd^2$	$-2\ bc^2d$	$+4\ abcd^2$	$+5\ a^2c^2d^2$	$-3\ a^2c^2d^2$	$-8\ a^2b^2d^2$	$+4\ a^4bcd^2$
$-1\ c^3$	$+4\ b^2cd$	$+6\ abc^2d$	$+4\ a^2c^2d$	$-1\ c^4$	$+4\ b^2c^2d$	$-8\ ab^2cd^2$	$-4\ abc^2d$	$-2\ a^2c^4d$	$-8\ a^3c^3d^2$
	$+2\ bc^3$	$+2\ ac^4$	$+8\ ab^2d^2$		$+2\ bc^4$	$+4\ abc^2d$	$-2\ ac^3$	$+8\ ab^2c^2d$	$+8\ a^2b^2d^2$
		$-8\ b^2cd$	$-20\ ab^2c^2d$			$+2\ ac^3$		$+4\ abc^3$	$+12\ a^2b^2c^2d^2$
		$-4\ b^2c^3$	$-8\ abc^4$			$-8\ b^2c^2d$			$-4\ a^2bc^4d$
			$+16\ b^2c^3$			$-4\ b^2c^4$			$-4\ a^2c^5$
			$+8\ b^2c^3$						
-4	-7	$+7$	-7	-5	$+9$	-5	-7	0	$+7$

Forming in like manner the determinants of the matrix

$$\begin{pmatrix} C, & D, & E, & F, & .. \\ D, & E, & F, & G, & . \\ E, & F, & G, & H, & \end{pmatrix},$$

and representing these by 123, 124, &c., viz.—

$$123=\begin{vmatrix} C, & D, & E \\ D, & E, & F \\ E, & F, & G \end{vmatrix}, \text{ &c.,}$$

we have, up to 234,

$123=a^{-1}\times$	$124=a^{-1}\times$	$134=a^{-1}\times$	$234=$
$+1\ a^3d^4$	$-4\ a^3bd^4$	$+1\ a^4cd^4$	$-1\ a^4d^5$
$+2\ a^3bcd^3$	$-3\ a^3c^2d^3$	$+4\ a^3b^2d^4$	$-4\ a^3bcd^4$
$-1\ a^3c^2d^2$	$-8\ a^2b^2cd^3$	$+8\ a^3bc^2d^3$	$-4\ a^3c^2d^3$
$-2\ abc^2d$	$-4\ a^3bc^2d^2$	$-3\ a^3c^4d^2$	$-4\ a^2b^2c^2d^3$
$-1\ ac^5$	$-2\ a^2c^4d$	$+8\ a^2b^2cd^3$	$-6\ a^2bc^4d^3$
	$+4\ ab^2c^4d$	$+12\ a^2b^2c^2d^2$	$-2\ a^3c^5d$
	$+2\ abc^5$	$-2\ a^2c^5$	
-1	-15	$+28$	-21

and further, the determinants of the matrix

$$\begin{pmatrix} C, & D, & E, & F, & .. \\ D, & E, & F, & G & \\ E, & F, & G, & H & \\ F, & G, & H, & I & \end{pmatrix},$$

in the present case, the single determinant

$$1234=\begin{vmatrix} C, & D, & E, & F \\ D, & E, & F, & G \\ E, & F, & G, & H \\ F, & G, & H, & I \end{vmatrix},$$

we have

$1234 = a^{-1} \times$
$-3\ a^3cd^3$
$-12\ a^3bc^2d^3$
$-4\ a^3c^3d^4$
$-16\ a^4b^2c^2d^4$
$-14\ a^4bc^3d^3$
$-3\ a^4c^3d^3$
$-8\ a^3b^2c^4d^3$
$-12\ a^3b^2c^3d^2$
$-6\ a^3bc^3d$
$-1\ a^3c^{10}$
-79

The conditions for the triangle and the quadrangle are $c=0$, $d=0$; those for the pentagon and hexagon are $12=0$, $23=0$; for the heptagon and octagon, $123=0$, $234=0$; and that for the enneagon is $1234=0$. The foregoing values show that 23 and 1234 (which belong to the hexagon and the enneagon) divide by c (which belongs to the triangle), and that 234 (which belongs to the octagon) divides by d (which belongs to the quadrangle). But I was not prepared for the destruction which will be observed in the several determinants, of the terms involving the lower powers of a (that is, the terms of the highest orders in b, c, d), and which renders these expressions so much more simple than they would otherwise have been.

Representing the reduced equations for the several polygons, as before, by

$$[3]=0,$$
$$[4]=0,$$
$$[5]=0,$$
$$[6]=0,$$
$$[7]=0,$$
$$[8]=0,$$
$$[9]=0,$$
$$\&c.$$

Then retaining the quantity $a(=1)$ for homogeneity, but rejecting the powers of a which divide out, and reversing in some cases the signs, the values of the functions [3], [4], &c. are

$[3]=$	$[4]=$	$[5]=$	$[6]=$	$[7]=$	$[8]=$	$[9]=$
$+1\ c$	$+1\ d$	$+1\ ad^2$	$+2\ ad^2$	$+1\ a^3d^4$	$+1\ a^3d^4$	$+3\ a^3d^6$
		$+2\ bcd$	$+2\ bcd$	$+2\ abcd^3$	$+4\ abcd^3$	$+12\ a^3bcd^3$
		$+1\ c^3$	$+1\ c^3$	$-1\ ac^3d^2$	$+4\ ac^3d^2$	$+4\ a^3c^3d^4$
				$-2\ bc^4d$	$+4\ b^2c^2d^3$	$+16\ ab^2c^2d^4$
				$-1\ c^5$	$+6\ bc^4d$	$-14\ abc^4d^3$
					$+2\ c^5$	$+3\ ac^6d^2$
						$+8\ b^3c^2d^3$
						$+12\ b^2c^3d^2$
						$+6\ bc^5d$
						$+1\ c^8$
$+1$	$+1$	$+4$	$+5$	-1	$+21$	$+79$

The similarity of form for the relations corresponding to the pentagon and the hexagon, and for those corresponding to the heptagon and the octagon, is, I am inclined to think, accidental; the functions are homogeneous as regards degree and weight; and the degrees and weights of the two consecutive functions being identical, the literal parts must be similarly constituted.

III.

M. MENTION'S *Formulæ for the Case of two Circles.*

In the case of two circles, if, as usual, the radii of the inscribed and circumscribed circles are put equal to r and R respectively, and the distance of their centres to a, then the equations of the inscribed and circumscribed circles respectively may be taken to be

$$x^2+y^2-r^2=0,$$
$$(x-a)^2+y^2-\mathrm{R}^2=0;$$

and if, in the notation of M. MENTION, we put

$$\frac{\mathrm{R}^2-r^2}{a^2}=i$$
$$\frac{1}{r^4}(r^4+\mathrm{R}^4+a^4-2r^2\mathrm{R}^2-2r^2a^2-2\mathrm{R}^2a^2)=-\nu,$$

then the quadratic radical is

$$\sqrt{(1+4\xi)\{[1+(2+2i)\xi]^2+4\nu\xi^2\}};$$

and comparing this with

$$\sqrt{1+4b\xi+4(c+b^2)\xi^2+4(d+2bc)\xi^3},$$

we have

$$\begin{aligned} b &= i+2,\\ c+b^2 &= i^2+6i+\nu+5,\\ d+2bc &= 4(i^2+2i+\nu+1), \end{aligned}$$

and thence

$$\begin{aligned} b &= i+2,\\ c &= 2i+\nu+1,\\ d &= -2i\nu-2i; \end{aligned}$$

and by means of these values, or by effecting the development in a different manner, we find

$$\mathrm{B}=\begin{cases} 1\\ +2, \end{cases}$$
$$\mathrm{C}=\begin{cases} i.2\\ +\nu+1, \end{cases}$$
$$\mathrm{D}=2\{i(-\nu-1)\},$$
$$\mathrm{E}=\begin{cases} i^2.4\nu\\ +i\,.4(\nu+1)\\ \quad -(\nu+1)^2, \end{cases}$$

$$F = 2\left\{\begin{array}{l} i^3.-4\nu \\ +i^2.-8\nu \\ +i.(\nu+1)(3\nu-5) \\ +2(\nu+1)^3, \end{array}\right.$$

$$G = 2\left\{\begin{array}{l} i^4.8\nu \\ +i^3.24\nu \\ +i^2.4\nu(-3\nu+5) \\ +i\ .(\nu+1)(-18\nu+14) \\ +\ (\nu+1)^2(\nu-7), \end{array}\right.$$

$$H = 4\left\{\begin{array}{l} i^5.-8\nu \\ +i^4.-32\nu \\ +i^3.\ 4\nu(5\nu-11) \\ +i^2.\ 16\nu(3\nu-1) \\ +i\ .\ (\nu+1)(\nu-7)(-5\nu+3) \\ \quad -4(\nu+1)^2(\nu-3). \end{array}\right.$$

These values give

$$C = \left\{\begin{array}{l} 2i \\ +\nu+1, \end{array}\right. \qquad = [3]$$

$$D = 2\ \{i(-\nu-1), \qquad = [4]$$

$$CE - D^2 = \left\{\begin{array}{l} i^3.8\nu \\ +i^2.4(\nu+1) \\ +i\ .2(\nu+1)^2 \\ \quad -(\nu+1)^3, \end{array}\right. \qquad = [5]$$

$$DF - E^2 = \begin{pmatrix} 2i \\ +\nu+1 \end{pmatrix}\begin{pmatrix} 2i \\ -\nu-1 \end{pmatrix}\left\{\begin{array}{l} i^2.4\nu \\ +(\nu+1)^2 \end{array}\right\} \qquad = [3]\,[6]$$

$$\begin{vmatrix} C, & D, & E \\ D, & E, & F \\ E, & F, & G \end{vmatrix} = \left\{\begin{array}{l} i^6.64\nu \\ +i^5.-32\nu(\nu+1)(\nu-1) \\ +i^4.-16\nu(\nu+1)^2 \\ +i^3.-8(3\nu-1)(\nu+1)^3 \\ +i^2.\ 4(\nu+1)^4 \\ +i\ .-4(\nu+1)^5 \\ \quad -\ (\nu+1)^6 \end{array}\right. \qquad = [7]$$

$$\begin{vmatrix} D, & E, & F \\ E, & F, & G \\ F, & G, & H \end{vmatrix} = i.(-\nu-1)\left\{\begin{array}{l} 16i^3\nu \\ +(\nu+1)^3. \end{array}\right. \qquad = [4]\,[8]$$

It will be remarked that $[4]=i(-\nu-1)$ breaks up into the factors i and $\nu+1$; and so $[6]=(2i-\nu-1)\{4\nu i^2+(\nu+1)^2\}$ breaks up into the factors $2i-\nu-1$ and $4\nu i^2+(\nu+1)^2$.

It may be added that the developed expression of $[6]$ is

$$= \begin{cases} i^2.8\nu \\ +i^2.-4\nu(\nu+1) \\ +i.2(\nu+1)^2 \\ -(\nu+1)^3, \end{cases}$$

so that the difference between this and $[5]$ is $i^2.4(\nu+1)^2$, which is $=d^2$; this agrees with a former result.

M. MENTION has also given, but not in a developed form, the formulæ for the enneagon and the endecagon, and the following formula for the decagon, viz.

$$[16i^2\nu+(\nu+1)^4]^2+16i^2\nu(\nu+1)^2.\{2i^2(1-\nu)-(\nu+1)^2\}^2=0.$$

IV.

Considerations as to the form of relation, in the case of two circles, for Polygons of an odd and even number of sides respectively.

The relation between the two conics, or condition for the existence of the polygon, is the same whatever point of the circumscribed conic is taken as an angle of the polygon. Take for an angle, a point of intersection of the two conics. Consider first the case of the triangle; if a point of intersection is taken as a vertex A of the triangle, then the sides AB, AC coincide in direction with the tangent at A to the inscribed conic U, hence B and C coincide together at the point where this tangent meets the circumscribed conic V, BC is therefore a tangent of V, but it is by hypothesis a tangent of U; hence for the triangle the relation between the inscribed conic U and the circumscribed conic V is as follows, viz. a tangent to U at a point of intersection with V meets V at a point of contact of a common tangent of U and V.

In like manner for the quadrangle, if A be taken at a point of intersection, the sides AB and AD will coincide in direction with the tangent to U at this point, consequently B and D must coincide at the point where this tangent meets V; hence also CB, CD, the two tangents to U from the point C, must coincide, or C must be a point of intersection of the conics U, V. In other words, the pole, with respect to the inscribed conic U, of a common chord AC of the two conics must lie on the circumscribed conic V; this is therefore the condition for the quadrilateral.

In the ordinary mode of drawing the figures, with two conics which do not intersect, the points and lines employed in the foregoing constructions are imaginary, but the conics may be so drawn that these points and lines are all real.

In general, for a polygon of an odd number, $2n+1$, of sides, then starting from a point of intersection, the sides will coincide in pairs, viz. the first and last, second and last but one, and so on, the middle or $(n+1)$th side being a common tangent of the two

conics. But for a figure of an even number, $2n$, of sides, then starting from a point of intersection of the two conics, the nth side will terminate at a second point of intersection, and then the same series of sides will be repeated in the reverse order, so that the sides will coincide in pairs, first and last, second and last but one, nth and $(n+1)$th. For a figure of an odd number of sides, the relation involves only a single point of intersection, but for a figure of an even number of sides, it involves two points of intersection.

Now in the case of two circles, for a polygon of an odd number of sides, the same relation is obtained, whether we take as the point of intersection one of the actual points of intersection, or a circular point at infinity, and the relation $[2n+1]=0$ does not break up into factors. And so for a polygon of an even number of sides, then taking for the two points of intersection, the two actual points of intersection, or the two circular points at infinity, we have one form of result; but taking for them an actual point of intersection and a circular point at infinity, we have a different form of result; and the equation $[2n]=0$ does break up into factors.

This is verified very simply in the case of the quadrangle. Taking for the two points of intersection the circular points at infinity, the line joining them is the line infinity, and its pole, with respect to the inscribed circle, is the centre of this circle; the relation therefore is that the centre of the inscribed circle lies on the circumscribed circle. But when this is the case, it is easy to see that the pole (with respect to the inscribed circle) of the radical axis, lies also on the circumscribed circle; this pole and the centre of the inscribed circle are in fact the extremities of a diameter of the circumscribed circle. The condition thus obtained is $R^2=a^2=0$ (which is M. MENTION'S condition $i=0$). We have next to find the analytical relation when the pole (with respect to the inscribed) circle, of the line joining one of the actual points of intersection with a circular point at infinity is a point on the circumscribed circle. This I effect as follows:—Taking $z=0$ as the equation of line infinity, if the origin be taken on the middle point of the radical axis, and if $x=0$ be the radical axis, then the equations of the two circles may be taken to be

$$\text{Inscribed circle,} \qquad x^2+y^2-2l\,xz-\nabla z^2=0.$$

$$\text{Circumscribed circle,} \qquad x^2+y^2-2\mathrm{L}xz-\nabla z^2=0.$$

A circular point at infinity is

$$x:y:z=1:i:0, \qquad (i=\sqrt{-1}).$$

An actual point of intersection is

$$x:y:z=0:\sqrt{\nabla}:1.$$

The line joining these is

$$xi-y+z\sqrt{\nabla}=0.$$

Its pole, with respect to the inscribed circle, is

$$x:y:z=-i\sqrt{\nabla}:\sqrt{\nabla}-il:1.$$

And if this be a point of the circumscribed circle

$$-\nabla+(\nabla-2il-l^2)+2Li\sqrt{\nabla}-\nabla=0,$$

that is,

$$2(L-l)i\sqrt{\nabla}=l^2+\nabla,$$

or

$$(l^2+\nabla)^2+4(L-l)^2\nabla=0,$$

which is the required relation: but to express it in terms of the ordinary data R, r, a, the equations of the circles, putting therein $z=1$, become

$$(x-l)^2+y^2=\nabla+l^2,$$
$$(x-L)^2+y^2=\nabla+L^2,$$

and therefore

$$a=L-l,$$
$$r^2=\nabla+l^2,$$
$$R^2=\nabla+L^2;$$

these equations give

$$\frac{R^2-r^2}{a}=L+l,$$
$$a=L-l;$$

and thence

$$L=\frac{R^2-r^2+a^2}{2a},$$

and

$$\nabla=R^2-\left(\frac{R^2-r^2-a^2}{2a}\right)^2=\frac{1}{4a^2}.(2a^2R^2+2a^2r^2+2r^2R^2-R^4-r^4-a^4)$$
$$=\frac{r^4}{4a^2}\nu,$$

if with M. Mention we write

$$\frac{1}{r^4}(r^4+R^4+a^4-2r^2R^2-2r^2a^2-2R^2a^2)\equiv-\nu.$$

The equation

$$(l^2+\nabla)^2+4(L-l)^2\nabla=0$$

thus becomes

$$r^4+4a^2.\frac{r^4}{4a^2}\nu=0,$$

that is, it becomes $\nu+1=0$, which is the other factor of the complete condition

$$i(\nu+1)=0.$$

XII. *Tables of the Weights of the Human Body and Internal Organs in the Sane and Insane of both Sexes at various Ages, arranged from 2614 post-mortem examinations.*

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These Tables have been compiled from notes of 2086 examinations made at St. Marylebone Infirmary, between 1839 and 1847; and of 528 examinations in cases of insanity made at the Somerset Lunatic Asylum, between 1848 and the end of December 1860, comprehending in all a period of twenty-one years. The calculations alone have been the labour of many months; a task, which, owing to the pressure of daily duties in a large establishment, I could not now have completed but for the able assistance of my relative Major Boyd.

The Tables are submitted with a hope that they may aid in forming a standard of the weight of the human organs from early infancy to old age. The cases are arranged at eighteen periods of life, under eighteen different heads, showing the average height and weight of the body (the measurement of the head, and weight of the spinal marrow in No. 2), the average weight of the encephalon and its several parts; also of each lung, of the heart, and of all the abdominal viscera. The assigned causes of death are given in the margin; also the variations in weight of the lungs, heart, and liver.

TABLE I.

Showing the maximum, minimum, and average height and weight of the human body, and weight compiled from 2086 cases, 1025 males and 1061 females, examined in the St. Marylebone and mean weight of the lungs in health and disease; of the hearts and livers above and also shown.

NOTE.

The weights are given in pounds, ounces, and decimal parts of an ounce avoirdupois.

The following method was pursued in examining the body:—It was measured from the vertex of the head to the inside of the foot behind the ball of the great toe; after being weighed, the spinal canal was opened, the cord with its membranes removed from below upwards, as high as possible, and weighed. The measurements of the head were next taken: the circumference around the occipital protuberance to the centre of the forehead; the antero-posterior from the former to the space between the eye-brows; and the transverse from the opening of one ear, over the vertex, to the opening of the other. The brain was then examined: the right and left cerebral hemispheres were each removed in slices to the tentorium, and weighed; the tentorium being divided on each side, the cerebellum, with pons and medulla, were raised; the crura cerebelli being divided close to the pons, each part was weighed, the pons and medulla together. The thoracic organs were next examined: the lungs having been taken out, the heart was removed by dividing the great vessels inside, and close to the pericardium; the cavities were opened, cleansed, and the organ weighed. The abdominal organs were weighed in the order in which they are set down in the Tables.

	Assigned causes of death.		
	Causes.	Males.	Females.
Still-born; full period.	Diseases of Digestive organs	1	1
	Respiratory organs	2	0
	(Enlarged thymus)	8	6
	Vascular system	4	0
	Nervous system	23	12
	Craniotomy	5	0
	Inquest cases	2	1
	Unknown causes	6	12
	Total	51	32
New born.	Diseases of Digestive organs	6	11
	Respiratory organs	0	0
	(Enlarged thymus)	1	1
	Vascular system	3	3
	Nervous system	12	7
	Inquest cases	9	8
	Unknown causes	14	15
	Total	45	45

TABLE I.

of the internal organs in both sexes, and in the fœtus, at eighteen periods from infancy to old age; parochial infirmary from 1839 to 1847. The assigned causes of death, the maximum, minimum, below the general average, and the mean weight of each kidney, where weighed separately, are

Age.	Sex.	Body.		Cerebral organs.				Thoracic organs.				Abdominal organs.						
		Weight.	Height.	Cerebrum.	Cerebellum.	Pons and medulla.	Encephalon.	Right lung.	Left lung.	Heart.	Thymus.	Stomach.	Liver.	Spleen.	Pancreas.	Kidneys.	Renal capsules.	Uterus.
										Maximum.							
Premature, still-born.	M.	oz. 85	in. 18	oz. 9	oz. ·75	oz. ·5	oz. 9·2	oz. 1·25	oz. 1·25	oz. ·75	oz. ·31	oz. ·5	oz. 7	oz. ·75	oz. ·25	oz. 1·13	oz. ·29	oz.
	F.	75	18	8·5	·5	·19	9·13	1	1	·62	·19	·2	5·5	1	·2	·75	·33	·06
										Minimum.							
	M.	12	10	1·25	·06	·04	1·31	·19	·16	·06	·01	·02	·5	·02	·01	·13	·02	
	F.	8·5	10	·8	·19	·03	1·29	·06	·06	·06	·01	·02	·25	·02	·01	·6	·02	·002
										Average.							
	M.	45·5	14	5·33	·28	·12	5·6	·57	·52	·31	·1	·12	2·42	·2	·03	·44	·13	
	F.	38	13·5	4·42	·36	·1	4·62	·5	·42	·26	·07	·05	2·1	·23	·05	·32	·1	·02
										Number examined.							
	M.	28	26	21	19	19	25	24	24	27	24	20	27	26	17	24	25	
	F.	20	19	10	6	6	18	19	19	20	18	18	19	18	18	19	18	14
										Maximum.							
Still born; full period.	M.	172	22	21	2	1	22	1·75	1·75	1·75	·75	·75	8·5	·75	·37	1·7	·75	
	F.	154	22	14·5	1·25	·37	15·12	1·75	·56	1	·75	·5	8·5	1·25	·25	1·62	·39	·13
										Minimum.							
	M.	60	16	8·75	·37	·04	9·37	·5	·31	·37	·06	·12	2·75	·12	·04	·5	·06	
	F.	67	17	7·5	·37	·03	8	·62	·5	·16	·08	·12	2·62	·12	·04	·37	·12	·01
										Average.							
	M.	105·7	18·5	12·87	·8	·02	13·87	1·03	·83	·72	·31	·22	5·33	·29	·11	·89	·25	
	F.	98·5	19	11·25	·75	·16	12·25	1·03	·83	·65	·3	·25	5·5	·37	·11	·82	·23	·06
										Number examined.							
	M.	51	48	42	43	43	43	47	47	50	50	42	49	47	46	48	45	
	F.	32	32	30	29	29	31	29	29	30	30	31	32	30	28	31	30	27
										Maximum.							
New born.	M.	140	21	14	1·25	·37	15·37	1·75	1·25	1·25	1·02	·5	8·5	1	·37	2·25	·5	
	F.	128	21	15	1·25	·37	16	1·75	1·5	1·12	·56	·75	8·75	1·25	·25	1·5	·28	·08
										Minimum.							
	M.	42·5	14	5·62	·25	·06	6	·37	·29	·18	·06	·06	1·75	·06	·03	·25	·06	
	F.	25	14	1·62	·12	·04	1·75	·25	·21	·16	·02	·04	1·25	·04	·03	·25	·06	·01
										Average.							
	M.	81	18·2	10·84	·69	·2	11·67	1·04	·85	·54	·25	·23	4	·33	·11	·87	·17	
	F.	67·7	17·5	9·26	·62	·16	10	1·19	·86	·59	·21	·22	3·82	·34	·11	·68	·16	·04
										Number examined.							
	M.	45	45	44	40	40	42	44	44	44	44	42	45	42	39	28	42	
	F.	45	40	38	35	35	39	38	38	42	34	40	44	40	34	38	33	27

TABLE I.

Age.	Assigned causes of death.			Lungs.	No.		Average.		Maximum.		Minimum.	
	Causes.	Males.	Females.		Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
Under 3 months.	Diseases of Digestive organs	0	4	Male. Healthy	7	8	1·3	·95	1·5	1·25	·75	·5
	Genito-urinary organs	0	1	Phthisis	...	...	...	...	...	...	...	...
	Respiratory organs	6	8	Pneumonia	4	3	1·67	1·75	2·5	2·25	1	·75
	Vascular system	3	4	Bronchitis	...	...	...	...	...	...	...	...
	Nervous system	5	2	Female. Healthy	11	11	1·04	·98	1·5	1·5	·37	·37
	Locomotive organs	0	1	Phthisis	...	...	...	...	...	...	...	...
	Fevers	0	2	Pneumonia	4	4	1·75	1·62	2·25	2	1·25	1·25
	Inquest cases	2	0	Bronchitis	...	...	...	...	...	...	...	...
	Total	16	22									
From 3 to 6 months.	Diseases of Digestive organs	2	2	Male. Healthy	9	9	1·42	1·38	2	1·75	·75	·75
	Respiratory organs	5	16	Phthisis	2	2	2·25	2	2·5	2·25	2	1·75
	Vascular system	4	1	Pneumonia	4	4	2·28	1·93	2·5	2	2	1·75
	Nervous system	2	3	Bronchitis	...	...	...	...	...	...	...	...
	Locomotive organs	1	2	Female. Healthy	8	9	1·32	1·31	1·5	1·5	1·13	1
	Fevers	0	1	Phthisis	1	1	...	...	3	2·75	...	...
	Inquest cases	1	0	Pneumonia	9	8	2·86	2·19	5·5	2·25	1·5	1·25
	Total	15	25	Bronchitis	...	...	...	...	...	...	...	...
From 6 to 12 months.	Diseases of Digestive organs	3	0	Male. Healthy	15	14	1·87	1·77	2·75	2·37	1·25	1·13
	Genito-urinary organs	0	1	Phthisis	4	4	4·47	4·25	9·25	9·25	2	2
	Respiratory organs	29	23	Pneumonia	18	19	4·09	3·37	5·5	5	2·5	2·25
	Vascular system	5	4	Bronchitis	1	1	...	...	1·5	1·5	...	...
	Nervous system	6	4	Female. Healthy	14	16	1·87	1·86	2·75	2·5	1	1·13
	Fevers	3	8	Phthisis	1	1	...	...	3·25	3	...	...
	Total	46	40	Pneumonia	19	17	3·37	2·85	5·5	4·5	2·12	2
				Bronchitis	3	3	2·12	2·56	2·75	3	2·13	1·75
From 1 to 2 years.	Diseases of Digestive organs	1	2	Male. Healthy	6	6	3·38	3·06	3·5	3·5	3	2·5
	Respiratory organs	19	15	Phthisis	3	3	4	4·87	5	6	3	2·75
	Vascular system	2	2	Pneumonia	19	19	4·68	4·47	7·25	6·5	3·75	3·25
	Nervous system	3	5	Bronchitis	2	2	2·5	2·5	2·25	2·5	2·75	2·5
	Fevers	9	9	Female. Healthy	14	15	2·69	2·17	4·5	3·25	1·5	1·25
	Total	34	33	Phthisis	2	2	5	4·75	5·5	6·5	4·5	3
				Pneumonia	10	9	5·31	4·04	7	6	3	3
				Bronchitis	4	4	2·5	2·25	3	3	1·75	1·5

continued).

Age.	Sex.	Body.		Cerebral organs.				Thoracic organs.				Abdominal organs.						
		Weight.	Height.	Cerebrum.	Cerebellum.	Pons and medulla.	Encephalon.	Right lung.	Left lung.	Heart.	Thymus.	Stomach.	Liver.	Spleen.	Pancreas.	Kidneys.	Renal capsules.	Uterus.
								Maximum.										
Under 3 months.	M.	lbs. oz. 12 0	in. 24	oz. 31·5	oz. 1·62	oz. ·5	oz. 32·75	oz. 2·5	oz. 2·25	oz. 1·5	oz. ·37	oz. ·77	oz. 8	oz. ·75	oz. ·75	oz. 2·25	oz. ·19	oz.
	F.	12 0	22	31	1·75	·5	32·5	2·25	2	1	·75	·62	7	·75	·5	1·5	·25	·25
								Minimum.										
	M.	4 0	18	9·25	·5	·13	10·5	·75	·5	·5	·04	·25	2·5	·13	·06	·25	·06	
	F.	3 6	16	11	·5	·13	11	·37	·37	·25	·06	·20	3	·17	·06	·37	·06	·03
								Average.										
	M.	7 3	22	16·2	1·07	·24	17·42	1·51	1·16	·88	·16	·37	5	·34	·27	·99	·13	
	F.	6 2	20·7	14·81	·9	·25	15·94	1·23	1·15	·64	·17	·4	4·32	·33	·19	·94	·11	·06
								Number examined.										
	M.	16	16	15	15	15	16	11	11	16	11	15	16	16	15	14	13	
	F.	22	17	19	18	18	20	15	15	21	13	19	21	19	18	22	15	14
								Maximum.										
From 3 to 6 months.	M.	12 8	32	27·5	3	·5	30·75	2·5	2·25	1	·62	1·25	9	1·5	·5	2	·29	
	F.	11 8	25	33	2·75	·62	34·75	5·5	2·75	1·5	·5	·75	8·5	1·5	·5	2·5	·5	·06
								Minimum.										
	M.	4 8	19·5	15·5	·75	·25	10·75	·75	·75	·62	·06	·25	3	·25	·06	·75	·06	
	F.	4 0	19	13·5	·25	·13	13	1·13	1	·5	·03	·25	2·75	·19	·1	·75	·06	·02
								Average.										
	M.	8 5	22·7	20	1·72	·29	21·29	1·77	1·6	·88	·22	·49	6·22	·71	·21	1·3	·13	
	F.	7 0	22·5	18·05	1·69	·30	19·76	2·19	1·76	·81	·15	·5	5·44	·46	·22	1·09	·14	·04
								Number examined.										
	M.	15	15	14	14	14	15	15	15	15	9	15	15	15	14	14	13	
	F.	25	22	23	21	21	25	18	18	24	13	21	25	24	20	23	18	12
								Maximum.										
From 6 to 12 months.	M.	18 0	30	32	3·75	·75	36·13	9·25	9·25	2·25	·75	1·25	18·5	1·75	1·04	3·75	·5	
	F.	18 0	33	34·75	4	·62	39·13	5·5	4·5	2·25	·5	1·25	13	1·5	1·25	3	·37	·13
								Minimum.										
	M.	5 12	18	14·5	·75	·13	17·75	1·25	1·13	·5	·04	·25	4	·25	·08	1·25	·06	
	F.	6 0	19	13·5	1·75	·25	16·37	1	1·13	·5	·06	·25	3·5	·25	·13	·62	·06	·02
								Average.										
	M.	12 1·5	26	24·29	2·74	·39	27·42	3·20	2·86	1·18	·18	·64	9·41	·79	·34	2·43	·21	
	F.	10 11·5	25·7	22·68	2·32	·35	25·7	2·73	2·39	1·08	·15	·66	8·35	·72	·32	1·85	·18	·05
								Number examined.										
	M.	45	45	46	46	46	46	38	38	46	29	42	44	43	41	45	33	
	F.	40	39	38	38	38	40	37	37	40	25	29	40	40	36	39	32	27
								Maximum.										
From 1 to 3 years.	M.	26 0	34	36	4·5	·75	41·25	7·25	6·5	3·5	·5	1·75	17	8	·75	4	·25	
	F.	25 0	35	33·25	4·25	1	37	7	6·5	2·25	·5	1·37	17·25	2·25	1	4	·25	·13
								Minimum.										
	M.	10 8	24	20·5	2·75	·25	23·25	2·75	2·5	1	·06	·5	7·5	·5	·06	1	·06	
	F.	5 8	22	14·75	1·75	·25	18	1·5	1·25	·75	·04	·37	7	·5	·06	1·5	·06	·03
								Average.										
	M.	14 6	28·5	29·21	3·54	·5	33·25	4·12	3·99	1·66	·17	1	11·7	1·34	·44	2·55	·16	
	F.	13 2	27·7	26·19	3·13	·46	29·8	3·58	2·95	1·47	·13	·91	11·17	1·01	·49	2·4	·14	·05
								Number examined.										
	M.	34	32	34	34	34	34	30	30	34	19	32	34	33	30	33	30	
	F.	33	33	33	33	33	33	30	30	32	18	30	33	32	32	29	28	25

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TABLE I.

Age.	Assigned causes of death.				No.		Average.		Maximum.		Minimum.	
	Causes.	Males.	Females.	Lungs.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
From 2 to 4 years.	Diseases of: Digestive organs	2	3	Male. Healthy	8	7	3·8	3·35	4·75	4·75	3·25	2·75
	Genito-urinary organs	0	1	Phthisis	6	6	6·16	5·87	10·75	10·75	3	4·5
	Respiratory organs	16	16	Pneumonia	10	11	6·85	6·76	8·75	11	5·5	3·13
	Vascular system	1	2	Bronchitis	2	2	4	3	4·5	4	3·5	2
	Nervous system	3	2	Female. Healthy	13	14	3·76	3·5	4·75	4·5	1·75	1·42
	Fevers	7	5	Phthisis	3	3	6·37	5·25	8·25	6·5	5·5	5
	Total	29	29	Pneumonia	11	10	7·01	5·91	9·5	8	4	3·5
				Bronchitis	...	...	...	...	...	...	...	...

From 4 to 7 years.	Diseases of: Digestive organs	1	1	Male. Healthy	6	5	3·79	3·58	6·5	5·5	2	1·75
	Respiratory organs	15	10	Phthisis	9	9	7·36	6·25	10	9·75	4·5	3·5
	Vascular system	3	3	Pneumonia	6	7	6·96	8·46	8·5	14·5	5	5
	Nervous system	4	1	Bronchitis	3	3	6·58	5·25	7	5·5	6	5
	Locomotive organs	0	1	Female. Healthy	8	7	3·13	3·23	4·5	4·75	2·13	2
	Fevers	4	5	Phthisis	8	8	7·15	7·06	11	11	3·5	3
	Total	27	21	Pneumonia	3	3	7·5	6·33	8	7·25	7·25	6
				Bronchitis	...	...	...	...	...	...	...	...

		Male.				Female.			
		No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
Kidneys.									
Right		2	2·75	2·25	2·5	5	5·25	1·75	2·7
Left		2	3	2·5	2·75	5	3·25	1·75	2·34

From 7 to 14 years.	Diseases of: Digestive organs	3	2	Lungs. Male. Healthy	3	4	7·42	6·06	9·25	6·75	4·5	4·5
	Respiratory organs	11	6	Phthisis	7	7	9·5	10·29	15·25	16	5	5
	Vascular system	4	2	Pneumonia	6	5	12·46	14·64	16·5	23·5	7·5	8
	Nervous system	2	4	Bronchitis	1	1	...	...	9·5	7·5	...	...
	Locomotive organs	1	0	Female. Healthy	8	7	6·19	6·07	8	9	4·5	4
	Fevers	1	4	Phthisis	2	2	8·5	8·25	12·5	11	4·5	5·5
	Total	22	18	Pneumonia	1	2	...	14	29·5	16·5	...	11·5
				Bronchitis	2	2	9·5	9·5	10	11·25	9	7·75

		Male.				Female.			
		No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
Livers.									
Under general average		11	22·25	18·25	20·79	11	23·5	15·5	20·8
Above general average		8	71	35·25	47·17	6	44	28	35·16
Kidneys.									
Right		10	6	1·87	3·51	1	2·25	...	...
Left		10	4·25	1·74	3·27	1	2·75	...	...

(continued).

Age.	Sex.	Body.		Cerebral organs.				Thoracic organs.				Abdominal organs.						
		Weight.	Height.	Cerebrum.	Cerebellum.	Pons and medulla.	Encephalon.	Right lung.	Left lung.	Heart.	Thymus.	Stomach.	Liver.	Spleen.	Pancreas.	Kidneys.	Renal capsules.	Uterus.
		lbs. oz.	in.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.
From 2 to 4 years.																		

TABLE I.

Age.	Assigned causes of death.				No.		Average.		Maximum.		Minimum.	
	Causes.	Males.	Females.	Lungs.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
From 14 to 20 years.	Diseases of: Digestive organs	1	2	Male. Healthy	6	5	11·71	9·89	18·5	17·5	7·75	6·5
	Respirstory organs	7	8	Phthisis	5	5	28·1	32	37·5	42	10·5	15·25
	Vascular system	0	1	Pneumonia	3	4	27·42	18·5	47	20·5	13·5	17
	Nervous system	5	1	Bronchitis	2	2	16·75	15·5	19·5	19·75	14	11·25
	Locomotive organs	4	1	Female. Healthy	5	7	11·1	10·85	16·5	16	7	7
	Fevers	2	3	Phthisis	2	2	21	20·25	27·25	24	14·75	16·5
	Total	19	16	Pneumonia	4	2	23·75	24·25	30	33	19·5	15·5
				Bronchitis	1	1	...	...	19	16·5	...	..
					Male.				Female.			
				Hearts.	No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
				Under general average	11	7·5	3·5	5·71	7	8·25	4·5	7·4
				Above general average	7	14	8·5	9·32	7	16·5	9·25	10·68
				Livers.								
				Under general average	8	56	32	39·41	5	53	23	35·64
				Above general average	9	96	58	67·58	9	73	55	58·83
				Kidneys.								
				Right	10	6·5	3·5	4·52	2	7	3·5	5·25
				Left	10	7·5	3	4·84	2	6·5	3	4·75
From 20 to 30 years.	Diseases of: Digestive organs	4	10		No.		Average.		Maximum.		Minimum.	
	Genito-urinary organs	1	9		Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
	Respiratory organs	42	25	Lungs. Male. Healthy	9	8	17·36	17·31	20·75	19·5	11	9
	Vascular system	1	6	Phthisis	28	28	35·6	35	64·5	58	23	20·75
	Nervous system	3	13	Pneumonia	3	4	23·67	20·27	24·5	21	23	19·5
	Locomotive organs	0	3	Bronchitis	...	...	...	...	...	...	...	...
	Fevers	6	9	Female. Healthy	32	33	14·65	13·5	21	20	6·25	6·25
	Inquest cases	2	0	Phthisis	16	16	25·78	20·16	36·5	36·5	16	14
	Total	59	75	Pneumonia	11	10	30·26	21·27	31·5	30·25	22	18·5
				Bronchitis	2	2	20·75	19·75	22	21·5	19·5	18
					Male.				Female.			
				Hearts.	No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
				Under general average	36	10	5·5	8·54	44	9	3·5	7·3
				Above general average	22	17	11	14	30	26	9·25	11·69
				Livers.								
				Under general average	39	60	32	51·53	38	52·25	28	45·52
				Above general average	17	114	60·5	79·16	36	83·5	53	6·.81
				Kidneys.								
				Right	14	7·25	1·5	5·82	25	8	2·25	5·3
				Left	14	9	4	6·48	25	6·75	2·25	5·3
From 30 to 40 years.	Diseases of: Digestive organs	3	9		No.		Average.		Maximum.		Minimum.	
	Genito-urinary organs	3	3		Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
	Respiratory organs	70	44	Lungs. Male. Healthy	19	20	19·08	17·16	24·5	23·5	10·75	10·5
	Vascular system	4	9	Phthisis	49	49	36·71	31·24	63·5	52	14	10·5
	Nervous system	26	15	Pneumonia	16	15	39·89	25·98	87	47	21·5	22·5
	Locomotive organs	0	1	Bronchitis	12	12	24·14	20·27	32·5	28·5	20	13·75
	Fevers	5	5	Female. Healthy	35	33	14·53	13·11	20·25	18·75	9·5	6·25
	Inquest cases	2	3	Phthisis	25	25	24·86	22·7	37	37	9	13
	Total	113	89	Pneumonia	10	12	25·6	24·51	33	34	19	17
				Bronchitis	6	6	20·16	16·25	20·5	19	14·25	12·5
					Male.				Female.			
				Hearts.	No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
				Under general average	68	11·25	3·5	8·91	52	9·25	5·5	7·83
				Above general average	40	30·25	11·5	15·51	35	18·75	9·5	12·11
				Livers.								
				Under general average	62	58	24·5	48·58	52	53·5	22·5	43·84
				Above general average	48	101	58·5	70·83	35	103	54	66·84
				Kidneys.								
				Right	35	8	3·25	5·58	32	6·25	3	4·61
				Left	35	9	3·37	6·3	32	8·75	2·75	4·94

(continued).

Age.	Sex.	Body.			Cerebral organs.				Thoracic organs.				Abdominal organs.						
		Weight.		Height.	Cerebrum.	Cerebellum.	Pons and medulla.	Encephalon.	Right lung.	Left lung.	Heart.	Thymus.	Stomach.	Liver.	Spleen.	Pancreas.	Kidneys.	Renal capsules.	Uterus.
		Maximum.																	
		lbs.	oz.	ft. in.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.
From 14 to 30 years.	M.	104	0	0 71	50·75	6·5	1·25	58·5	47	49	14	...	7	96	10·75	3·5	14	1	
	F.	107	0	0 67	45·5	5·5	1	52	30	33	16·5	...	6·5	73	7·25	4	13·5	1·25	2·5
		Minimum.																	
	M.	40	0	0 54	34·5	4·25	·75	36·5	7·75	6·5	3·5	...	2·5	32	2·75	1	5·5	·25	
	F.	47	0	0 42	34	2	·5	37·5	7	7	4·5	·19	2·5	23	3·5	1·5	6·5	·25	·6
		Average.																	
	M.	68	0	0 60·5	41·77	5·32	1	48·54	20·4	19·67	7·61	...	4·51	57·76	5·19	2·19	9·34	·52	
	F.	63	14	0 57·7	38·83	4·65	·85	43·94	17·52	15·08	9·46	...	4·6	54·33	4·68	2·64	9·09	·63	1·04
		Number examined.																	
	M.	17		17	18	17	17	19	16	16	18	...	18	17	17	17	18	15	
	F.	14		12	15	15	15	16	12	12	15	...	14	15	14	14	14	10	11
		Maximum.																	
	M.	132	0	6 0	48	6·25	1·25	57	64·5	58	17	...	8	114	19	5	16·5	1·25	
From 30 to 30 years.	F.	145	0	5 7	48·75	5·5	1·25	55·25	36·5	36	26	...	8·5	83·5	15	5	17	1·	4·25
		Minimum.																	
	M.	60	0	5 0	33	4·5	·5	39·25	11	9	5·5	...	4	32	2·75	1·75	6	·12	
	F.	40	0	4 7	31·5	4	·5	35·75	6·25	6·25	·5	...	2·5	28	1·75	1·25	3·5	·25	·75
		Average.																	
	M.	92	14·5	5 6·75	41·98	5·19	·93	47·9	32·34	30·09	10·06	...	5·51	60·29	7·19	3·54	11·57	·82	
	F.	86	13	5 2	38	4·82	·88	43·7	21·89	19·71	9·08	...	5·32	53·74	6·53	2·95	10·17	·63	1·73
		Number examined.																	
	M.	55		57	52	52	52	59	50	50	58	...	58	56	59	55	57	47	
	F.	70		71	69	69	69	72	61	61	74	...	72	74	73	68	73	57	47
		Maximum.																	
	M.	154	0	6 2	50	6·75	1·5	60·75	87	52	30·25	...	9·5	101	36	7	25	1·25	
From 30 to 40 years.	F.	147	0	5 9	46	6	1·5	53	37	37	18·75	...	8·5	103	20·5	6	20	1	4·5
		Minimum.																	
	M.	60	0	4 2	28·25	2·75	·37	33·75	10·75	10·5	3·5	...	3·5	24·5	1·75	1	6·6	·25	
	F.	57	0	4 6	28·25	3·75	·5	33·25	9	6·25	5·5	...	3·25	22·5	1·25	1·25	6	·25	·5
		Average.																	
	M.	98	3·5	5 6·5	42·06	5·15	·98	48·2	28·47	24·29	11·36	...	5·72	58·11	7·12	3·47	11·35	·61	
	F.	87	0	5 2	37·92	4·74	·91	43·09	18·74	17·64	9·45	...	5·34	53·61	6·13	3·05	10·34	·47	1·98
		Number examined.																	
	M.	103		106	99	99	99	110	96	96	118	...	105	110	103	105	108	89	
	F.	85		84	79	80	80	89	76	76	87	...	83	87	85	85	80	67	79

TABLE I.

From 40 to 60 years.

Assigned causes of death.		
Causes.	Males.	Females.
Diseases of Digestive organs	2	15
Diseases of Genito-urinary organs	4	4
Diseases of Respiratory organs	90	50
Diseases of Vascular system	14	13
Diseases of Nervous system	20	16
Diseases of Locomotive organs	4	3
Fevers	4	4
Inquest cases	3	3
Total	141	98

Lungs.	No. Right.	No. Left.	Average. Right.	Average. Left.	Maximum. Right.	Maximum. Left.	Minimum. Right.	Minimum. Left.
Male. Healthy	28	32	18·86	17·5	28	27	5	5
Male. Phthisis	54	54	35·49	31·53	57	58	13·5	17·5
Male. Pneumonia	33	30	32·14	30·53	53·5	66	22	20
Male. Bronchitis	12	11	25·54	23·82	46	37·5	15·5	13·5
Female. Healthy	40	42	15·06	13·21	20·5	21	7·5	8·25
Female. Phthisis	27	27	25·62	23·01	42·5	43·5	16·5	16
Female. Pneumonia	13	11	25	25·24	38·5	41·5	20	18·25
Female. Bronchitis	7	7	22·51	18·96	24	23·25	20	12·5

Hearts.	Male. No.	Male. Max.	Male. Min.	Male. Aver.	Female. No.	Female. Max.	Female. Min.	Female. Aver.
Under general average	88	11·5	5·75	9·32	60	9·5	5	7·58
Above general average	49	27	11·75	16·29	46	17·25	9·75	12·36
Livers.								
Under general average	88	58	30	53·41	65	49	25·5	41·08
Above general average	51	192	58·25	74·12	41	130	49·25	62·1
Kidneys.								
Right	50	9	2·5	5·07	32	7	2·25	4·25
Left	50	9	2·5	5·57	32	7	2·5	4·42

From 60 to 60 years.

Assigned causes of death.		
Causes.	Males.	Females.
Diseases of Digestive organs	1	9
Diseases of Genito-urinary organs	3	2
Diseases of Respiratory organs	80	54
Diseases of Vascular system	10	17
Diseases of Nervous system	18	23
Diseases of Locomotive organs	2	0
Fevers	3	3
Inquest cases	4	0
Total	121	108

Lungs.	No. Right.	No. Left.	Average. Right.	Average. Left.	Maximum. Right.	Maximum. Left.	Minimum. Right.	Minimum. Left.
Male. Healthy	21	22	19·28	16·04	25·5	25·5	10	9·25
Male. Phthisis	42	42	34·43	29·54	57·5	45	14	13·5
Male. Pneumonia	35	34	17·89	29·6	57	56·5	19	15·5
Male. Bronchitis	10	10	26·7	23·2	36	27·25	15·5	17·5
Female. Healthy	55	57	15·21	13·25	26	26	7·5	7
Female. Phthisis	15	15	26·27	21·89	38·75	32	21	11·5
Female. Pneumonia	16	14	25·75	25·25	35·5	55	19·25	13
Female. Bronchitis	11	11	20·68	15·65	24·5	20·5	16	10·5

Hearts.	Male. No.	Male. Max.	Male. Min.	Male. Aver.	Females. No.	Females. Max.	Females. Min.	Females. Aver.
Under general average	71	11·75	6	9·62	63	10·25	5	8·33
Above general average	48	30	12	15·51	43	24	10·5	13·24
Livers.								
Under general average	63	55·25	30	46·06	63	43·75	28	37·07
Above general average	55	88	55·5	65·42	44	90	44	54·37
Kidneys.								
Right	36	7·25	3·25	5·21	37	7·75	1·5	4·27
Left	36	9·25	3·5	5·62	38	8	2·25	4·58

From 60 to 70 years.

Assigned causes of death.		
Causes.	Males.	Females.
Diseases of Digestive organs	7	17
Diseases of Genito-urinary organs	1	6
Diseases of Respiratory organs	88	78
Diseases of Vascular system	13	15
Diseases of Nervous system	13	24
Diseases of Locomotive organs	2	2
Fevers	5	6
Inquest cases	2	0
Total	131	148

Lungs.	No. Right.	No. Left.	Average. Right.	Average. Left.	Maximum. Right.	Maximum. Left.	Minimum. Right.	Minimum. Left.
Male. Healthy	24	36	17·84	16·92	30	27	13	10
Male. Phthisis	28	28	31·75	29·83	45	52	14·5	14
Male. Pneumonia	52	40	27·59	23·81	59·25	49·5	20·5	19
Male. Bronchitis	10	10	23·79	20·06	30	27	18	15
Female. Healthy	57	74	14·34	13·52	18	19·5	7	6·5
Female. Phthisis	21	21	24·62	22	34	34·5	13	7·5
Female. Pneumonia	36	19	27·06	22·15	49·5	32	17·75	15·75
Female. Bronchitis	22	22	19·83	17·08	28	24	12·75	10·5

Hearts.	Male. No.	Male. Max.	Male. Min.	Male. Aver.	Female. No.	Female. Max.	Female. Min.	Female. Aver.
Under general average	79	12·75	7	10·52	90	10·5	5·25	8·72
Above general average	47	24·75	13	17·02	59	20	10·75	13·5
Livers.								
Under general average	69	48	23	38·32	91	49·75	25	37·54
Above general average	58	173	48·25	57·31	63	79	43	51·03
Kidneys.								
Right	35	7·5	2·75	4·54	50	12·5	1·5	4·38
Left	35	7·25	·5	4·68	50	7	1·5	4·46

(continued).

Age.	Sex.	Body.				Cerebral organs.				Thoracic organs.				Abdominal organs.						
		Weight.		Height.		Cerebrum.	Cerebellum.	Pons and medulla.	Encephalon.	Right lung.	Left lung.	Heart.	Thymus.	Stomach.	Liver.	Spleen.	Pancreas.	Kidneys.	Renal capsules.	Uterus.
		Maximum.																		
		lbs.	oz.	ft.	in.	oz.	oz.	oz.	oz.	oz.	oz.	oz.		oz.	oz.	oz.	oz.	oz.	oz.	oz.
From 40 to 50 years.	M.	155	0	6	1	53·5	6·75	1·5	60	83·5	66	27	...	11·5	192	33	6	20·5	1·5	
	F.	133	0	5	7	46	6	1·25	52·5	42·5	43·5	17·25	...	9·5	130	15	5·25	21·5	1·25	
		Minimum.																		
	M.	54	0	4	6	27·75	4·5	·75	33·75	5	5	5·75	...	3·75	30	1·75	2	5·5	·27	
	F.	44	0	4	10	31·25	4	·5	37·5	7·5	8·25	5	...	3	25·6	1	1·75	3·6	·25	·5
		Average.																		
	M.	102	0	5	6·8	41·48	5·22	1·06	47·75	31·21	28·63	11·53	...	5·83	58·06	6·19	3·48	10·89	·74	
	F.	84	9·5	5	2	37·12	4·69	·89	42·81	19·73	17·47	9·6	...	5·2	49·03	5·04	2·73	8·8	·62	1·95
		Number examined.																		
	M.	135		137		121	120	120	137	127	127	137	...	138	139	138	132	140	106	
	F.	97		97		94	94	94	106	87	87	106	...	103	106	104	99	106	77	88
		Maximum.																		
	M.	188	0	6	2	52	7	1·75	59	57·5	56·5	30	...	10	88	22	7	16·5	1·25	
	F.	171	0	5	7	46	5·75	1·5	52·5	38·75	55	24	...	8·5	90	17	6	14·25	1·25	4·5
From 50 to 60 years.		Minimum.																		
	M.	66	0	4	10	34·5	4	·5	30·5	10	9·25	6	...	3·5	30	·75	1	5·5	·25	
	F.	50	0	4	6	29·5	3	·5	36·25	7·5	7	5	...	2·5	28	·75	1·25	4	·25	·75
		Average.																		
	M.	102	·5	5	6	41·09	5·13	·98	47·44	30·32	26·29	11·83	...	5·58	53·37	6·23	3·46	9·1	·84	
	F.	86	0	5	2	37·38	4·62	·86	43·12	19·48	17·08	10·44	...	5·42	44	4·67	2·83	8·35	·65	1·75
		Number examined.																		
	M.	110		114		111	111	111	119	108	108	119	...	113	118	112	116	116	101	
	F.	100		104		91	91	91	103	97	97	106	...	103	107	105	89	108	89	93
		Maximum.																		
	M.	256	0	6	2	52·5	7	1·5	59·5	59·25	52	24·75	...	9	173	20	5	14·5	1·25	
	F.	147	0	6	0	48	6·25	1·25	54	49·5	34·5	20	...	9·25	79	15·5	6	14	1·25	3·5
From 60 to 70 years.		Minimum.																		
	M.	70	0	5	0	31	3·5	·5	36·25	13	10	7	...	3	23	1·5	1·5	3·25	·12	
	F.	50	0	4	6	27·5	3·75	·5	32·5	7	6·5	5·25	...	3	25	·5	1·25	4	·25	·75
		Average.																		
	M.	103	13	5	5·7	40·21	4·98	·97	46·4	28·52	24·16	12·94	...	5·58	48·24	4·82	3·1	8·83	·67	
	F.	86	14	5	1·5	37·13	4·68	·83	42·69	20·32	16·59	10·64	...	5·3	43·98	4·07	2·8	8·28	·64	1·53
		Number examined.																		
	M.	123		125		119	119	119	127	114	114	126	...	125	127	126	122	123	102	
	F.	142		148		136	134	134	149	136	136	149	...	144	154	150	145	148	121	140

TABLE I.

Age.	Assigned causes of death.				No.		Average.		Maximum.		Minimum.	
	Causes.	Males.	Females.	Lungs.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
From 70 to 80 years.	Diseases of: Digestive organs	8	8	Male. Healthy	13	21	18·08	16·13	22	23·5	15	9
	Genito-urinary organs	2	9	Male. Phthisis	13	12	29·88	30·8	47	42·5	19·5	19
	Respiratory organs	58	69	Male. Pneumonia	40	30	32·78	27·65	85	43·5	20	18·25
	Vascular system	13	26	Male. Bronchitis	21	23	23·42	19·36	31·75	35	15·5	13
	Nervous system	20	37	Female. Healthy	48	56	13·32	12·34	19	18	6·5	5·5
	Locomotive organs	1	3	Female. Phthisis	11	11	23·67	18·26	31	27	14	10·5
	Fevers	2	1	Female. Pneumonia	35	37	25·11	25·38	45	49·5	15	13
	Total	104	153	Female. Bronchitis	33	33	19·04	16·08	30	22·75	13·5	11
					Male.				Female.			
				Hearts.	No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
				Under general average	57	13	5·5	9·96	86	10	5·25	8·09
				Above general average	43	37·5	13·25	17·04	64	21	10·25	13·5
				Livers.								
				Under general average	56	46	27	37·29	79	38·25	19·5	32·49
				Above general average	48	99	46·5	57·81	69	70	38·5	44·97
				Kidneys.								
				Right	19	6·75	2	4·46	31	5·75	1·5	3·8
				Left	19	9	3·25	5·19	31	7	2·25	3·96
Upwards of 80 years.	Diseases of: Digestive organs	0	2		No.		Average.		Maximum.		Minimum.	
	Genito-urinary organs	1	0		Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
	Respiratory organs	19	37	Lungs.								
	Vascular system	1	12	Male. Healthy	1	4	...	16·37	18·5	...	19·5	13
	Nervous system	2	18	Male. Phthisis	4	4	25·75	28·81	38·5	38	21	19
	Locomotive organs	1	3	Male. Pneumonia	13	10	34·13	26·65	61·5	39·5	29	22·5
	Fevers	0	2	Male. Bronchitis	4	4	23·69	21·37	25	28	22	16·5
	Inquest cases	0	1	Female. Healthy	26	34	13·31	12	19	17·5	7·75	6
	Unknown causes	0	4	Female. Phthisis	6	6	21·29	19·25	24·25	24	16·5	13·5
	Total	24	79	Female. Pneumonia	19	11	24·53	25·59	50	38·5	17	17·5
				Female. Bronchitis	20	20	17·63	13·7	33·5	21·5	10·5	8·5
					Male.				Female.			
				Hearts.	No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
				Under general average	12	12	8	10·1	50	10·25	5·5	8·71
				Above general average	12	16·75	13·5	14·1	26	26·5	10·5	13·27
				Livers.								
				Under general average	12	39·5	25·5	33·69	43	34·5	22	29·37
				Above general average	12	57	42	48·14	31	57	35	41·96
				Kidneys.								
				Right	2	6	5·75	5·87	6	3·75	2·5	3·46
				Left	2	6·25	6	6·12	6	4	2·5	3·58

(continued).

Age.	Sex.	Body.			Cerebral organs.				Thoracic organs.				Abdominal organs.						
		Weight.		Height.	Cerebrum.	Cerebellum.	Pons and medulla.	Encephalon.	Right lung.	Left lung.	Heart.	Thymus.	Stomach.	Liver.	Spleen.	Pancreas.	Kidneys.	Renal capsules.	Uterus.
From 70 to 80 years.																			

TABLE II.

Showing the maximum, minimum, and average height and weight of the human body, and female patients, examined in the Somerset County Lunatic Asylum. The assigned

Forms of Insanity.		Under 30 years.		From 30 to 40 years.	
		Male.	Female.	Male.	Female.
Mania		13	11	16	21
Dementia		3	4	6	4
Melancholia		2	2	6	7
Monomania		1	1	3	...
Delirium tremens		...	...	1	...
Idiocy		4	1	...	...
Epilepsy with	Mania	9	6	3	9
	Dementia	1	1	1	1
	Melancholia	1	...	1	1
	Idiocy	11	3	3	2
General paralysis with	Mania	...	1	8	1
	Dementia	1	...	6	3
	Melancholia	...	...	1	...
	Monomania	...	...	6	...
Total		46	30	61	49

Age.	Assigned causes of death.				No.		Average.		Maximum.		Minimum.	
	Causes.	Males.	Females.	Lungs.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
	Diseases of: Digestive organs	5	3	Male. Healthy	17	20	15·35	13·28	20·5	19	8	6
	Genito-urinary organs	0	0	Phthisis	14	13	35·37	34·44	72	49·5	20·5	19
	Respiratory organs	27	17	Pneumonia	15	12	27·59	23·84	54	42	19·75	18·25
	Vascular system	1	0	Bronchitis	...	...	...	...	...	...	...	...
	Nervous system	11	10	Female. Healthy	13	16	14·03	13·74	20	16·5	10	8
	Locomotive organs	1	0	Phthisis	9	9	25·52	23·54	36·5	31	22·25	20
	Inquest cases	1	0	Pneumonia	8	5	22·9	23·67	37·5	26	17·75	19·5
	Total	46	30	Bronchitis	...	...	...	...	...	...	...	...
					Male.				Female.			
				Hearts.	No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
				Under general average	24	8·5	5	7·11	14	7·5	5	6
				Above general average	22	12·5	9	10·51	15	9·75	8	8·6
				Livers.								
				Under general average	23	50	28	40·88	18	44	24·5	33·23
				Above general average	23	84	51	60·84	11	75	45·75	61·87
From 30 to 40 years.	Diseases of: Digestive organs	2	10	Male. Healthy	15	23	15·94	14·82	19·75	19·75	10	9·5
	Genito-urinary organs	0	0	Phthisis	10	10	37·53	48·99	74	52	23·5	16·75
	Respiratory organs	32	25	Pneumonia	34	26	31·02	28·52	45·5	47	20	20
	Vascular system	0	3	Bronchitis	...	...	...	...	...	...	...	...
	Nervous system	26	11	Female. Healthy	23	26	13·59	13·05	17·5	19·5	7	6
	Locomotive organs	0	0	Phthisis	11	12	24·23	24·48	40	33	13·25	18·25
	Fevers	1	0	Pneumonia	13	9	23·85	22·96	46·5	29	19·5	16·25
	Total	61	49	Bronchitis	1	1	...	...	23	14·75	...	...
					Male.				Female.			
				Hearts.	No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
				Under general average	35	9·9	5·25	8·54	29	7·5	4·5	6·49
				Above general average	21	16·5	10·5	12·03	20	16	8·25	9·38
				Livers.								
				Under general average	30	55·25	37	47·25	29	43·25	26·5	37·68
				Above general average	29	74·75	53·25	64·07	20	114	44·75	51·49

TABLE II.

weight of the internal organs and measurement of the head, in 528 cases, 295 male and 233 causes of death, &c., as in Table I., and the forms of insanity, are also shown.

From 40 to 50 years.		From 50 to 60 years.		From 60 to 70 years.		From 70 to 80 years.		Upwards of 80 years.	
Male.	Female.	Male.	Female.	Male.	Female.	Male.	Female.	Male.	Female.
25	14	14	10	13	10	5	4	1	1
13	7	4	8	14	14	5	12	7	4
4	10	9	9	7	12	4	2	...	...
2	1	4	5	...	2	3	1	...	...
1	...	...	...	...	...	...	...	...	...
...	1	1	2	...	2	...	...	...	...
6	3	2	3	2	...	1	1	...	...
...	4	2	...	1	...	1	...	...	...
1	1	...	...	...	...	...	...	...	...
1	...	...	1	...	...	...	...	...	...
10	3	3	...	...	1	2	...	...	...
12	5	3	1	2	...	...	...	...	...
2	...	1	...	...	...	...	...	...	...
...	...	...	...	...	...	...	...	...	...
77	49	43	39	39	41	21	20	8	5

Age.	Sex.	Body.		Measurement of head.			Cerebral organs.					Spinal cord.	Thoracic organs.			Abdominal organs.							
							Cerebrum.																
		Weight.	Height.	Circumference.	Antero-posterior.	Transverse.	Right hemisphere.	Left hemisphere.	Cerebellum.	Pons and medulla.	Encephalon.		Right lung.	Left lung.	Heart.	Stomach.	Liver.	Spleen.	Pancreas.	Right kidney.	Left kidney.	Renal capsules.	Uterus.
												Maximum.											
Under 30 years of age.	M.	lbs. oz. 185 0	ft. in. 6 0	in. 23·5	in. 15·25	in. 15·5	oz. 25·5	oz. 25·5	oz. 6·5	oz. 1·5	oz. 58	oz. 1·5	oz. 72	oz. 49·5	oz. 12·5	oz. 8	oz. 84	oz. 7·5	oz. 6	oz. 8·5	oz. 8	oz. 1·25	oz.
	F.	106 0	5 8	23	14	16·5	24·5	24·75	6·25	1·5	55·75	1·5	36·5	31	9·75	6·5	75	16·5	4	6·5	7·75	1	2
												Minimum.											
	M.	55 0	5 2	20·25	10·5	10	13·75	8	3·25	·75	30	·75	8	6	5	3·5	28	2·25	2·25	1·75	1·5	·5	
	F.	47 0	4 11	19·5	11	12	13	13	3·5	·75	31	·75	10	8	5	9·75	24·5	2·5	2	2·75	9·75	·25	·75
												Average.											
	M.	91 8	5 6·7	22·03	13·32	13·29	20·89	21·05	5·23	1·04	48·17	1·1	25·32	22·55	8·75	5·44	50·18	4·86	3·52	4·65	4·82	·79	
	F.	77 10	5 2·5	21·22	13·07	13·32	19·21	19·51	4·82	1·01	44·55	1·05	19·82	18·57	7·57	5·06	44·84	5·13	2·94	4·14	4·32	·74	1·24
												Number examined.											
	M.	40	39	38	38	38	44	44	43	43	45	18	46	45	46	45	46	45	45	46	46	44	
	F.	27	23	24	24	23	30	30	30	30	30	18	30	30	29	30	29	30	30	30	30	27	28
												Maximum.											
From 30 to 40 years.	M.	158 0	6 0	24	14·75	15	24·5	25	7	1·5	57·5	1·75	74	52	16·5	9	74·75	12·75	5·75	10	10·5	1·25	
	F.	123 0	5 6·5	23	14	14	24	24·5	6	1·75	55·75	1·5	46·5	33	16	7	114	20·25	5	7·5	8	1·	4·5
												Minimum.											
	M.	66 0	5 2	20·5	10	10	13·5	14·5	4	·75	35·25	1	10	9·5	5·25	4	37	2·5	2	2·5	3·5	·5	
	F.	50 0	4 10	19	9	9	14·5	14·5	3·75	·75	34·5	·75	7	6	4·5	3·25	26·5	1·75	1·75	2	2·75	·25	·75
												Average.											
	M.	103 1·5	5 7·2	22·16	13·2	13·25	19·82	19·94	5·23	1·05	46·14	1·1	28·32	24·04	9·94	5·93	55·45	5·64	3·54	5·01	5·33	·83	
	F.	77 14	5 3·5	21·31	13·77	13·77	18·63	18·84	4·23	1·05	43·29	1·08	19·44	17·84	7·73	5·09	43·36	5·47	3·49	4·17	4·45	·81	1·84
												Number examined.											
	M.	55	53	46	45	46	61	61	61	61	61	22	59	59	59	58	59	59	59	58	58	57	
	F.	49	48	42	42	42	46	46	48	48	49	18	48	48	49	48	49	46	47	49	48	46	44

TABLE II.

From 40 to 50 years.

Assigned causes of death.			Lungs.		No.		Average.		Maximum.		Minimum.	
Causes.	Males.	Females.			Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
Diseases of: Digestive organs	4	9	Male.	Healthy	14	21	16·43	15·64	19·5	19·25	10·25	10·5
Genito-urinary organs	0	1		Phthisis	9	7	29·84	27·1	48·5	33	20	17·5
Respiratory organs	43	18		Pneumonia	50	42	33·54	29·79	73·75	60	20	19
Vascular system	1	3		Bronchitis	3	3	26·5	23·12	34	29	26	19·5
Nervous system	29	15	Female.	Healthy	31	35	14·1	13·6	19·5	20	6·5	6
Locomotive organs	0	1		Phthisis	3	3	23·98	23	26	31·5	20	18·5
Fevers	0	2		Pneumonia	14	10	23·58	22·02	35	27	20	20
Total	77	49		Bronchitis	1	1	...	...	22	23	...	...

Hearts.	Male. No.	Male. Max.	Male. Min.	Male. Aver.	Female. No.	Female. Max.	Female. Min.	Female. Aver.
Under general average	40	10·5	6·5	9·31	25	8·25	4·75	6·82
Above general average	36	16·5	10·75	12·33	24	12·5	8·5	9·87
Livers.								
Under general average	42	56·5	36	48·53	26	43·25	25	35·02
Above general average	34	89	57·1	67·58	22	98·75	44	53

From 50 to 60 years.

Assigned causes of death.			Lungs.		No.		Average.		Maximum.		Minimum.	
Causes.	Males.	Females.			Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
Diseases of: Digestive organs	4	5	Male.	Healthy	8	13	17·83	15·84	19·5	19	13	8
Genito-urinary organs	0	0		Phthisis	9	9	28·87	26·3	57	42	16·5	11
Respiratory organs	23	15		Pneumonia	24	19	29·27	29·58	59	57	18	17·5
Vascular system	0	5		Bronchitis	1	1	...	...	25·75	23·5	...	...
Nervous system	16	13	Female.	Healthy	24	24	14·61	12·14	19	16·75	7	6·75
Locomotive organs	0	1		Phthisis	5	5	26·58	19·37	30·25	23·75	21·5	16
Total	43	39		Pneumonia	10	10	27·41	26·82	39	40	20	16
				Bronchitis	...	...	...	...	...	...	...	...

Hearts.	Male. No.	Male. Max.	Male. Min.	Male. Aver.	Female. No.	Female. Max.	Female. Min.	Female. Aver.
Under general average	26	10·75	5	9·48	22	9	5·5	7·35
Above general average	16	24	11·25	14·5	17	16·75	9·25	13·31
Livers.								
Under general average	19	53·5	13	40·43	25	45·5	24·5	38·05
Above general average	23	95	53·5	63·47	14	119·5	48	58·42

From 60 to 70 years.

Assigned causes of death.			Lungs.		No.		Average.		Maximum.		Minimum.	
Causes.	Males.	Females.			Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
Diseases of: Digestive organs	2	9	Male.	Healthy	10	16	15·7	14·85	18·5	18·5	8·5	7
Genito-urinary organs	1	0		Phthisis	3	3	42·87	35·87	48·75	42	37	29·75
Respiratory organs	13	14		Pneumonia	23	17	37·33	32·89	74·5	47	23·5	20
Vascular system	4	4		Bronchitis	3	3	19·5	16·5	21·75	19	16·5	16
Nervous system	16	13	Female.	Healthy	26	30	13·59	12·21	18·75	18	9	7·5
Locomotive organs	0	1		Phthisis	7	7	21·54	24·87	30	26	19·5	15
Fevers	1	0		Pneumonia	7	3	22·36	30·16	28	50	19	19·5
Inquest cases	2	0		Bronchitis	1	1	...	...	20	19·5	...	...
Total	39	41										

Hearts.	Male. No.	Male. Max.	Male. Min.	Male. Aver.	Female. No.	Female. Max.	Female. Min.	Female. Aver.
Under general average	22	12	6·5	9·13	26	9	6	7·62
Above general average	17	27·5	12·5	15·58	15	15·5	9·5	11·77
Livers.								
Under general average	25	50	36	45·13	21	41	27	33·63
Above general average	14	73	51	61·6	20	58	41·5	49·7

(continued).

Age.	Sex.	Body.		Measurement of head.			Cerebral organs.					Spinal cord.	Thoracic organs.			Abdominal organs.							
							Cerebrum.																
		Weight.	Height.	Circumference.	Antero-posterior.	Transverse.	Right hemisphere.	Left hemisphere.	Cerebellum.	Pons and medulla.	Encephalon.		Right lung.	Left lung.	Heart.	Stomach.	Liver.	Spleen.	Pancreas.	Right kidney.	Left kidney.	Renal capsules.	Uterus.
Maximum.																							
From 40 to 60 years.	M.	lbs. oz. 257 0	ft. in. 6 0	in. 24·5	in. 16	in. 15	oz. 25·5	oz. 25·5	oz. 6·5	oz. 1·5	oz. 57·75	oz. 1·5	oz. 73·75	oz. 60	oz. 16·5	oz. 10	oz. 89	oz. 14·25	oz. 7·5	oz. 7·5	oz. 9	oz. 2	oz.
	F.	132 0	5 7	23	14·5	15·5	23	21·5	6·5	1·5	51·25	1·37	35	31·5	12·5	8·25	98·75	11·5	5·25	7·5	7	1	3
Minimum.																							
	M.	61 0	5 0	20·5	10·5	10·25	15	15·25	4	·75	36·25	·75	10·25	10·5	6·5	3·75	36	1·75	2	3	2·5	·5	
	F.	47 0	4 9	19	10	10·5	11·75	11·75	3	·75	27·25	·75	6·5	6	4·75	3	25	1·5	1·25	1·75	2	·5	1
Average.																							
	M.	109 15·5	5 7·7	22·46	13·27	13·29	19·49	19·67	5·42	1·08	45·66	1·13	29·42	24·95	10·75	6·01	57·02	5·12	3·77	5·11	5·54	·91	
	F.	76 14·2	5 1·6	21·45	12·91	12·84	18·05	18·24	4·83	1·03	42·23	·97	17·55	16·05	8·31	5·19	43·51	4·33	2·88	4·2	4·42	·76	1·83
Number examined.																							
	M.	57	57	60	59	59	76	76	76	76	77	28	76	76	76	76	76	76	75	75	75	71	
	F.	47	44	43	43	43	48	48	48	48	49	27	49	49	49	48	48	48	48	48	48	48	43
Maximum.																							
From 50 to 60 years.	M.	204 0	6 3	23·5	14·5	14·5	24	24·5	7	1·5	56	1·5	59	57	24	10	95	15	6·5	7	8·25	1·5	
	F.	128 0	5 7	22·5	14·5	14·25	22·5	23·25	6	1·25	51·75	1	39	40	16·75	7	119·5	14	5·5	7·75	5·5	1·5	2·75
Minimum.																							
	M.	51 0	5 2	20	10·5	11	14·5	14·75	4·25	·75	34·5	·75	13	8	5	4	13	2	1·75	3·5	4	·5	
	F.	52 0	4 7	20·25	11	10	15·25	14·25	3·5	·5	34·25	·75	7	6·75	5·5	3	24·5	1·75	1·75	2·5	1·5	·5	·5
Average.																							
	M.	110 4·25	7·8	22·15	12·9	13·31	20·44	20·73	5·35	1·06	47·68	1·06	26·45	23·82	11·22	6·19	52·8	5·06	3·64	4·89	5·41	·89	
	F.	79 14	5 2·5	21·48	12·95	12·73	18·66	18·75	4·74	1·03	43·18	·99	19·42	16·52	9·15	5·38	45·53	4·43	3·38	4·21	4·13	·75	1·77
Number examined.																							
	M.	38	37	37	37	37	42	42	42	43	43	16	42	42	42	42	42	41	42	41	41	36	
	F.	37	33	34	34	34	39	39	39	39	39	19	39	39	39	39	39	39	37	39	39	38	37
Maximum.																							
From 60 to 70 years.	M.	188 0	6 0	23·5	14·5	15	25·5	26	6	1·96	58·75	1·25	74·5	47	27·5	10·5	73	10	5·75	6·75	6·75	1·5	
	F.	132 0	5 7	22·5	14	14	21·5	21·5	6·5	1·25	48·25	1·25	30	50	15·5	7·5	58	8·25	5	6·5	6·75	1	2·52
Minimum.																							
	M.	67 0	5 3	21	10·5	11·5	15	14·75	4·25	·75	35·75	·75	8·5	7	6·5	4	36	2·25	2·5	3·5	3·5	·5	
	F.	46 0	5 0	20	11	10·5	14	14	4	·75	33·25	·91	9·5	7·5	6	2·75	27	1·5	1·75	2·5	2·5	·5	1
Average.																							
	M.	106 15·25	5·8	22·34	13·28	13·4	20·66	20·86	5·22	1·08	47·82	1·03	29·02	22·91	12·42	5·87	50·77	4·72	3·7	4·53	5·09	·89	
	F.	79 9·35	5 2·5	21·19	12·73	12·81	18·37	18·53	4·85	·95	42·7	·96	16·42	15·05	9·12	4·89	41·31	3·59	2·87	3·84	3·98	·82	1·65
Number examined.																							
	M.	35	36	29	29	29	39	39	39	39	39	15	39	39	39	39	39	39	39	39	39	33	
	F.	39	38	34	34	34	41	41	41	41	41	15	41	41	41	39	41	41	41	41	41	41	35

TABLE II.

Age.	Assigned causes of death.				No.		Average.		Maximum.		Minimum.	
	Causes.	Males.	Females.	Lungs.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
From 70 to 80 years.	Diseases of: Digestive organs	1	5	Male. Healthy	9	6	15·58	12·25	19·5	16	12·5	10·5
	Genito-urinary organs	1	0	Phthisis	2	2	26·25	24·75	29·5	28·5	23	21
	Respiratory organs	5	7	Pneumonia	5	8	30·75	32·56	45	37	21	21
	Vascular system	4	1	Bronchitis	5	5	29·75	17·30	28	27·5	18	12
	Nervous system	10	7	Female. Healthy	8	6	12·53	10·98	16	16	9	8
	Locomotive organs	0	0	Phthisis	1	1	...	...	20·5	20	...	...
	Total	21	20	Pneumonia	5	7	21·8	19·22	34	24·5	18·5	15·75
				Bronchitis	6	6	18·22	16·31	28	20·5	14	11
					Male.				Female.			
				Hearts.	No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
				Under general average	9	11·46	6	11·37	14	9·2	6·5	8·03
				Above general average	12	15·5	11	14·56	6	15	10·5	12·57
				Livers.								
				Under general average	10	41	32	37·67	12	38	28·25	34·91
				Above general average	11	63	46	49·98	8	58	41	44·79
Upwards of 80 years.	Diseases of: Digestive organs	0	0		No.		Average.		Maximum.		Minimum.	
	Genito-urinary organs	0	0		Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
	Respiratory organs	4	2	Lungs.								
	Vascular system	1	1	Male. Healthy	1	3	...	11·67	...	13·5	10·5	8·5
	Nervous system	3	2	Phthisis	...	...	...	...	...	...	...	...
	Locomotive organs	0	0	Pneumonia	4	2	22·75	31·5	26·5	36	20	27
	Total	8	5	Bronchitis	3	3	21·75	15·66	29	16	11·25	15·5
				Female. Healthy	2	2	10·75	9·75	13	10·5	8·5	9
				Phthisis	...	...	...	...	...	...	...	...
				Pneumonia	2	2	18·5	17·75	23	19·5	14	16
				Bronchitis	1	1	...	...	9	12	...	...
					Male.				Female.			
				Hearts.	No.	Max.	Min.	Aver.	No.	Max.	Min.	Aver.
				Under general average	4	13	12	12·25	4	9·25	9	9·06
				Above general average	3	16	14	14·83	1	11	...	...
				Livers.								
				Under general average	5	41	32	38·58	2	38	36	37
				Above general average	3	60	43	50	3	45	41	43·67

(continued).

Age.	Sex.	Body.		Measurement of head.			Cerebral organs.					Spinal cord.	Thoracic organs.			Abdominal organs.							
							Cerebrum.																
		Weight.	Height.	Circumference.	Antero-posterior.	Transverse.	Right hemisphere.	Left hemisphere.	Cerebellum.	Pons and medulla.	Encephalon.		Right lung.	Left lung.	Heart.	Stomach.	Liver.	Spleen.	Pancreas.	Right kidney.	Left kidney.	Renal capsules.	Uterus.
												Maximum.											
From 70 to 80 years.	M.	lbs. oz. 135 0	ft. in. 5 11	in. 23·5	in. 14	in. 15	oz. 24·5	oz. 25	oz. 6	oz. 1·25	oz. 55	oz. 1·37	oz. 45	oz. 37	oz. 15·5	oz. 7·5	oz. 63	oz. 7	oz. 4	oz. 5	oz. 7·25	oz. 1	oz.
	F.	130 0	5 8	22	14	13·5	20	21	6	1·25	48	1·25	34	24·5	15	7	58	5	4·5	5	4·5	1·5	2·5
												Minimum.											
	M.	63 0	5 5	21	11	11·5	18	17·5	4	·75	40·5	·75	12·5	10·5	6	3·25	32	1·25	1·5	2	3	·5	
	F.	68 0	4 10	20	10·5	11·5	16	15·5	3·25	·75	36·75	·75	9	8	6·5	3·75	28·25	2	2	2·5	2·75	·5	1
												Average.											
	M.	103 10·7	5 7·2	22·3	13·34	13·1	20·25	20·47	5·06	1·02	46·85	1	21·53	20·71	11·52	5·44	43·87	3·37	3·03	3·94	4·44	·77	
	F.	95 3·4	5 1·5	21·18	12·38	12·76	17·97	18·09	4·76	1·05	41·87	1·02	17·17	15·87	9·36	5·37	38·33	3·38	3·12	3·7	3·37	·81	1·58
												Number examined.											
	M.	18	18	19	19	19	20	20	21	21	21	13	21	21	21	21	21	21	20	21	21	20	
	F.	15	18	15	15	15	20	20	20	20	20	9	20	20	20	20	20	20	20	20	20	20	20
												Maximum.											
Upwards of 80 years.	M.	124 0	5 10	23	14·25	14	21·5	22	6	1·5	49·75	1·5	29	36	16	6·5	60	3·75	4	6	7·5	1	
	F.	103 0	5 6	22·5	14·5	14	19·25	19·5	6	1	44·5	1	23	19·5	11	5·5	45	4	3·5	4	4	1·75	2
												Minimum.											
	M.	85 0	5 1	21·5	11	11·5	17	16	4·5	1	39·5	1	10·5	8·5	12	4	32	1·25	2	2·75	2·75	·75	
	F.	72 0	5 2	20	13	13	14·5	15·5	4·5	·75	15·25	·75	8·5	9	9	4	36	1·75	2·5	2	2·5	·75	1·25
												Average.											
	M.	112 2	5 5	22·25	13·28	13·31	18·97	18·62	5·13	1·15	43·87	1·14	20·84	18·12	13·35	5	42·87	2·39	2·87	3·84	4·5	·89	
	F.	90 3	5 3·2	21·6	13·4	13·5	17·20	17·39	5	·96	40·55	·87	13·5	13·39	9·46	4·69	40·39	2·6	2·87	3·14	3·3	1·06	1·69
												Number examined.											
	M.	7	7	8	8	8	8	8	8	8	8	6	8	8	7	7	8	8	8	8	8	5	
	F.	5	5	5	5	5	5	5	5	5	5	4	5	5	5	5	5	5	4	5	5	4	4

TABLE No. I.—*Assigned causes of death.*

In diseases of the digestive organs the disproportion between the sexes was great, 98 females to 45 males; there were 13 cases of infantile jaundice, 2 cases of imperforate anus, and 1 of malposition of the viscera in a new-born male. Diseases of the genito-urinary organs were more frequent amongst females than males, 33 to 15. Above half the mortality was owing to diseases of the respiratory organs; the per-centage amongst the male adults was 64·5, and amongst the females 47·6. In infants from 3 months to 4 years, pneumonia, frequently combined with eruptive fevers, prevailed; from 4 to 60 years pulmonary phthisis was most prevalent; and after 60 years pneumonia again prevailed. Of phthisis, there were 253 males and 140 females. Under diseases of the vascular system have been included those of the heart and blood-vessels, diseased state of the blood, purpura, hæmorrhage, dropsy, cachexy, scrofula, and cancer, which in the advanced periods of life, after 50 years, fell most heavily on females; the numbers were 85 males, and 120 females. Diseases of the nervous system were most fatal in infancy and old age, especially amongst male infants and aged females; the total numbers of all ages were males 167, females 187. Under the head "locomotive organs" are included accidents, diseases of bone, skin, and cellular tissue, of which there were 36 cases in both sexes. Of fevers, about 50 cases of the eruptive form occurred in infancy, and of ordinary fevers about 50 in adults. There were 43 "inquest cases," of which 23 were infants, mostly new-born, found exposed, and 20 were adults. There were 51 "causes unknown," viz. 47 still-born and new-born infants, and 4 females upwards of 80 years.

Results.

The general results obtained from a review of the foregoing cases among the poor of the parish of St. Marylebone, appear to be that, with few exceptions, the body and internal organs arrived at their full size in both sexes between 20 and 30 years. In children especially, the body was attenuated from disease: for example, one scrofulous female child, aged 3 years, weighed only $8\frac{1}{4}$ lbs., and the numbers about that age were insufficient to counterbalance the effect of such cases on the mean result, and form a standard of comparison for children of the same age under more favourable circumstances. The average weight of the males was greatest at from 70 to 80 years, which is to be accounted for by the large proportion cut off at earlier periods by pulmonary phthisis. The mean weight of the male brain was, at all periods, above that of the female, which was the probable cause of the large number of still-born male infants as compared with females, 51 to 32, and the necessity of resorting to craniotomy in five instances in the males only. The highest average weight of the brain in both sexes was from 14 to 20 years; the next highest was in the males from 30 to 40, and in the females from 20 to 30 years; but it will be observed that the number of cases were much fewer in these than in other later periods. The weight of the lungs was so much, and so frequently increased by disease, that healthy lungs were exceptions: it

therefore appeared advisable to introduce in the margin their weights in various states; also the weights of the heart and liver, which were subject to great variations. The "Thymus gland," an organ which disappears with infancy, was so large in the fœtus in fourteen cases, that it appeared to have formed a fatal impediment to respiration. The abdominal organs were generally heavier in the male than in the female; the spleen in both was subject to considerable variations in size, and the mean weight of the left kidney was generally found greater than that of the right.

TABLE No. II.—*Assigned causes of death.*

Diseases of the digestive organs, principally peritonitis, enteritis, and dysentery, were found in 18 males and 41 females, and most frequently in females from 30 years of age and upwards. In 1 male and 2 females there was disease of the kidneys; diseases of the respiratory organs in 147, one-half of the males, and in 98 females; 44 were cases of pulmonary consumption in males and 37 in females; 91 were cases of pneumonia in males and 55 in females. Of diseases of the vascular system, 11 occurred in males and 17 in females; 5 were cases of pericarditis, 4 in males; 13 of dropsy, 8 in females; and 11 of cachexy, of which 7 were females. Diseases of the nervous system were found in 109 males and 66 females; viz. cerebral apoplexy in 9 males and 8 females; meningitis in 22 males and 11 females; disease of brain in 55 males and 36 females; myelitis in 23 males and 11 females; so that 37 per cent. of males and 28·3 per cent. of females had organic disease of the cerebral organs. In Table I. the proportion is only 12·3 per cent. in the males and 17·6 per cent. in the females. There were 3 deaths from lumbar abscess, 1 from cancer, 1 from erysipelas, 4 from fever, and 3 were inquest cases.

Results.

The average *height* of the adult male varied from 67·8 to 65 inches, of the female from 63·2 to 61·6 inches; while the mean weight of the former varied from 112·12 to 91·5 lbs., and of the latter from 95·2 to 76·9, showing a preponderance in the insane male of 6 lbs., and in the insane female of 8 lbs., as compared with the sane adults dying at the same period of life.

The average weight of the right cerebral hemisphere varied in the males from 20·89 oz. to 18·97 oz., and in the females from 19·21 oz. to 17·20 oz.; the left varied in the males from 21·05 oz. to 18·62 oz., and from 19·51 oz. to 17·39 oz. in the females. It is a singular fact, confirmed by the examination of nearly 200 cases at St. Marylebone, in which the hemispheres were weighed separately, that almost invariably the weight of the left exceeded that of the right by at least the eighth of an ounce. In the Med. Chir. Trans. vol. xxxix., several cases of inequalities of the cerebral hemispheres which came under my notice are given. The average weight of the cerebellum varied in the males from 5·42 oz. to 5·06 oz., and from 5 to 4·74 oz. in the females; that of the pons Varolii and medulla in the males from 1·15 oz. to 1·02 oz., and from 1·05 to ·95 oz.

in the females; and that of the encephalon in males from 48·17 oz. to 43·87 oz., and in females from 44·55 oz. to 40·55 oz.; in the sane, at the same period of life, the average varied in the male from 48·2 to 45·34, and in the female from 43·7 to 39·77.

The general average weight of the lungs is shown in the Table, and the exceptions in the margin. The average weight of the heart did not reach its maximum until an advanced period of life.

In the abdominal organs nothing was observed differing essentially from those in Table No. I.

XIII. *On a New Auxiliary Equation in the Theory of Equations of the Fifth Order.*

By ARTHUR CAYLEY, *Esq.*, *F.R.S.*

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CONSIDERING the equation of the fifth order, or quintic equation,

$$(*)(v, 1)^5=(v-x_1)(v-x_2)(v-x_3)(v-x_4)(v-x_5)=0,$$

and putting as usual

$$f\omega=x_1+\omega x_2+\omega^2 x_3+\omega^3 x_4+\omega^4 x_5,$$

where ω is an imaginary fifth root of unity, then, according to LAGRANGE'S general theory for the solution of equations, $f\omega$ is the root of an equation of the order 24, called the Resolvent Equation, but the solution whereof depends ultimately on an equation of the sixth order, viz.

$$(f\omega)^5,\ (f\omega^2)^5,\ (f\omega^3)^5,\ (f\omega^4)^5$$

are the roots of an equation of the fourth order, each coefficient whereof is determined by an equation of the sixth order; and moreover the other coefficients can be all of them rationally expressed in terms of any one coefficient assumed to be known; the solution thus depends on a single equation of the sixth order. In particular the last coefficient, or

$$(f\omega . f\omega^2 . f\omega^3 . f\omega^4)^5,$$

is determined by an equation of the sixth order; and not only so, but its fifth root, or

$$f\omega . f\omega^2 . f\omega^3 . f\omega^4,$$

(which is a rational function of the roots, and is the function called by Mr. COCKLE the Resolvent Product), is also determined by an equation of the sixth order: this equation may be called the Resolvent-Product Equation. But the recent researches of Mr. COCKLE and Mr. HARLEY* show that the solution of an equation of the fifth order may be made to depend on an equation of the sixth order, originating indeed in, and closely connected with, the resolvent-product equation, but of a far more simple form; this is the auxiliary equation referred to in the title of the present memoir. The connexion of the two equations, and the considerations which led to the new one, will be pointed out in the sequel; but I will here state synthetically the construction of the auxiliary equation. Representing for shortness the roots $(x_1, x_2, x_3, x_4, x_5)$ of the given quintic equation by 1, 2, 3, 4, 5, and putting moreover

$$12345=12+23+34+45+51, \text{ \&c.}$$

* COCKLE, "Researches in the Higher Algebra," Manchester Memoirs, t. xv. pp. 131–142 (1858).

HARLEY, "On the Method of Symmetric Products, and its Application to the Finite Algebraic Solution of Equations," Manchester Memoirs, t. xv. pp. 172–219 (1859).

HARLEY, "On the Theory of Quintics," Quart. Math. Journ. t. iii. pp. 343–359 (1859).

(where on the right-hand side 12, 23, &c. stand for x_1x_2, x_2x_3, &c.), then the auxiliary equation, say

$$(*\chi\varphi, 1)^6=0,$$

has for its roots

$$\varphi_1=12345-24135, \quad \varphi_4=21435-13245,$$
$$\varphi_2=13425-32145, \quad \varphi_5=31245-14325,$$
$$\varphi_3=14235-43125, \quad \varphi_6=41325-12435,$$

and, it follows therefrom, is of the form

$$(1, 0, C, 0, E, F, G\chi\varphi, 1)^6=0,$$

where C, E, G are rational and integral functions of the coefficients of the given equation, being in fact seminvariants, and F is a mere numerical multiple of the square root of the discriminant.

The roots of the given quintic equation are each of them rational functions of the roots of the auxiliary equation, so that the theory of the solution of an equation of the fifth order appears to be now carried to its extreme limit. We have in fact

$$\varphi_1\varphi_6+\varphi_2\varphi_4+\varphi_3\varphi_5=(*\chi x_1, 1)^4,$$
$$\varphi_1\varphi_2+\varphi_3\varphi_4+\varphi_5\varphi_6=(*\chi x_2, 1)^4,$$
$$\varphi_1\varphi_3+\varphi_2\varphi_5+\varphi_4\varphi_6=(*\chi x_3, 1)^4,$$
$$\varphi_1\varphi_5+\varphi_2\varphi_6+\varphi_3\varphi_4=(*\chi x_4, 1)^4,$$
$$\varphi_1\varphi_4+\varphi_2\varphi_5+\varphi_3\varphi_6=(*\chi x_5, 1)^4,$$

where $(*\chi x_1, 1)^4$, &c. are the values, corresponding to the roots x_1, &c. of the given equation, of a given quartic function. And combining these equations respectively with the quintic equations satisfied by the roots x_1, &c. respectively, it follows that, conversely, the roots x_1, x_2, &c. are rational functions of the combinations $\varphi_1\varphi_6+\varphi_2\varphi_4+\varphi_3\varphi_5$, $\varphi_1\varphi_2+\varphi_3\varphi_4+\varphi_5\varphi_6$, &c. respectively, of the roots of the auxiliary equation.

It is proper to notice that, combining together in every possible manner the six roots of the auxiliary equation, there are in all fifteen combinations of the form $\varphi_1\varphi_2+\varphi_3\varphi_4+\varphi_5\varphi_6$. But the combinations occurring in the above-mentioned equations are a completely determinate set of five combinations: the equation of the order 15, whereon depend the combinations $\varphi_1\varphi_2+\varphi_3\varphi_4+\varphi_5\varphi_6$, is not rationally decomposable into three quintic equations, but only into a quintic equation having for its roots the above-mentioned five combinations, and into an equation of the tenth order, having for its roots the other ten combinations, and being an irreducible equation. Suppose that the auxiliary equation and its roots are known; the method of ascertaining what combinations of roots correspond to the roots of the quintic equation would be to find the rational quintic factor of the equation of the fifth order, and observe what combinations of the roots of the auxiliary equation are also roots of this quintic factor. The direct calculation of the auxiliary equation by the method of symmetric functions would, I imagine, be very laborious. But the coefficients are seminvariants, and the process explained in my

memoir on the Equation of Differences was therefore applicable, and by means of it, the equation, it will be seen, is readily obtained. The auxiliary equation gives rise to a corresponding covariant equation, which is given at the conclusion of the memoir.

1. I will commence by referring to some of the results obtained by Mr. COCKLE and Mr. HARLEY.

In the paper "Researches on the Higher Algebra," Mr. COCKLE, dealing with the quintic equation

$$v^5 - 5Qv + E = 0,$$

obtains for the Resolvent Product $\theta(=f\omega f\omega^2 f\omega^3 f\omega^4)$ the equation

$$\theta^6 + 2QE5^2\theta^4 + 2Q^4 5^7\theta^3 + Q^2E^2 5^{10}\theta^2 - (58Q^5 - E^3)E\theta + 5^{14}Q^6 = 0;$$

and he remarks that this equation may be written

$$(\theta^3 + 5^5QE\theta + 5^7Q^4)^2 = 5^{15}(108Q^5E - E^4)\theta,$$

so that $\sqrt{-\theta}$ is determined by an equation of the sixth order, involving the quadratic radical $\sqrt{E(E^5 - 108Q^5)}$, which is in fact the square root of the discriminant of the quintic equation.

2. Mr. HARLEY, in his paper "On the Symmetric Product, &c.," makes use of the functions

$$\tau = x_1x_2 + x_2x_3 + x_3x_4 + x_4x_5 + x_5x_1 (= 12345),$$
$$\tau' = x_1x_3 + x_3x_5 + x_5x_2 + x_2x_4 + x_4x_1 (= 24135),$$

and he obtains for the form $v^5 - 5Qv^3 + E = 0$, the relation $\theta = 5\tau\tau'$, which, since here $\tau + \tau' = 0$, gives $\theta = -5\tau^2$.

Hence $\tau\left(=\sqrt{-\frac{1}{5}\theta}\right)$ is the root of an equation of the sixth order involving the radical $\sqrt{E(E^5 - 108Q^5)}$, and which is in fact $\left(t = \frac{\tau}{\sqrt{5}} = \frac{1}{5}\sqrt{-\theta}\right)$, the equation

$$t^6 + 5QEt^2 + \sqrt{E(E^5 - 108Q^5)}t - 5Q^4 = 0,$$

given in Mr. HARLEY'S paper "On the Theory of Quintics."

3. And in the same paper there is given a system of equations

$$t_1t_2 + t_2t_3 + t_4t_5 = x_1(3^2Q - x_1^2), \text{ &c.},$$

connecting the five roots of the given quintic equation with the combinations

$$t_1t_3 + t_2t_4 + t_4t_5, \text{ &c.}$$

of the roots of the equation in t.

4. I quote also, with a slight change of notation, the following results from the paper "On the Symmetric Product, &c.," viz. considering the quintic equation under the form

$$(a, b, c, c, d, e, f)(v, 1)^5 = 0,$$

we have

$$f\omega f\omega^4 = \Sigma x^3 + \tau(\omega + \omega^4) + \tau'(\omega^2 + \omega^3)$$
$$f\omega^2 f\omega^3 = \Sigma x^3 + \tau(\omega^2 + \omega^3) + \tau'(\omega + \omega^4),$$

where

$$\Sigma x^2 = x_1^2 + x_2^2 + x_3^2 + x_4^2 + x_5^2 = \frac{1}{a^2}(b^2 - 2ac),$$

and thence, observing also that $\tau + \tau' = \frac{c}{a}$,

$$a^4\theta\,(= a^4 f\omega f\omega^2 f\omega^3 f\omega^4) = 5a^3c^3 - 5ab^3c + b^4 + 5a^4\tau\tau',$$

or as this equation may also be written,

$$4a^4\theta = (5ac - 2b^3)^2 - 5a^4(\tau - \tau')^2;$$

and hence the Resolvent Product $\theta\,(= f\omega f\omega^2 f\omega^3 f\omega^4)$ being determined by an equation of the sixth order, this is also the case with the function $(\tau - \tau')^2$.

5. But the twelve functions $\pm(\tau - \tau')$ can be divided into two sets of six functions each, so that each set is determined by an equation of the sixth order involving a single quadratic radical. This was in fact suggested to me by Mr. HARLEY'S equation in t; for in the case considered $t + t'$ was $= 0$, or $2t = t - t'$, and the equation in t was presumably the particular form of the equation for $\frac{1}{4}(t - t')$ in the general case. But it will presently appear in what manner the conclusion should have been arrived at *à priori*.

6. The preceding remarks show the connexion between the function $\varphi\,(= \tau - \tau')$ to which belongs the new auxiliary equation, and the Resolvent Product $\theta\,(= f\omega f\omega^2 f\omega^3 f\omega^4)$. The relation was given for the denumerate form of the quintic; but taking, instead, the standard form $(a, b, c, d, e, f)(x, 1)^5 = 0$, it becomes

$$4a^4\theta = 2500(ac - b^2)^3 - 5a^4\varphi^2.$$

7. The foregoing equation shows that φ is a seminvariantive function of the roots. In fact

$$f\omega, = x_1 - x_2 + \omega(x_2 - x_3) + \omega^2(x_3 - x_4) + \omega^3(x_4 - x_5),$$

is seminvariantive, and $f\omega^2, f\omega^3, f\omega^4$, being in like manner seminvariantive, the product $\theta\,(= f\omega f\omega^2 f\omega^3 f\omega^4)$ is also seminvariantive; $ac - b^2$ and a are seminvariants, and therefore φ is a seminvariantive function.

8. But it is easy to show this directly. For representing, as before, the roots by 1, 2, 3, 4, 5, we have

$$(1-5)(2-5) + (2-5)(3-5) + (3-5)(4-5) = 12 + 23 + 34 - 5(1 + 2 + 2 + 3 + 4) + 3.5^2,$$
$$(2-5)(4-5) + (4-5)(1-5) + (1-5)(3-5) = 24 + 41 + 13 - 5(2 + 4 + 1 + 3) + 3.5^2;$$

and the difference of the right-hand sides is

$$12 + 23 + 34 - 5(2 + 3)$$
$$-24 - 41 - 13 + 5(4 + 1),$$

which is $= 12345 - 24135$. So that φ,

$$= (1-5)(2-5) + (2-5)(3-5) + (3-5)(4-5) - [(2-5)(4-5) + (4-5)(1-5) + (1-5)(3-5)],$$

is a function of the differences of the roots, that is, it is a seminvariantive function.

9. To account for the division of the twelve values of $\pm(\tau - \tau')$ into two sets as above,

and to explain the formation of a set, consider the symbols 1, 2, 3, 4, 5 as belonging to five points. We may with these five points form in all $(\frac{1}{2}.1.2.3.4=)12$ pentagons, and the symbol 12345 of any pentagon may of course be read backwards or forwards from any point (12345=23451=&c.=15432=&c.) without alteration of its meaning. Now attaching to each arrangement of the five numbers a sign, + or −, according to the ordinary rule of signs, 12345 being as usual positive, the arrangements 12345, 23451, &c. . . 15432, &c., which belong to the same pentagon, have all of them the same sign; and we may consequently connect with each pentagon the sign + or −; there are, in fact, six pentagons with the sign + and six with the sign −; and to each positive pentagon there corresponds a negative pentagon, which is derived from it by *stellation*, viz. to the positive pentagon 12345 there corresponds the negative one 24135, and so for the other positive pentagons. The above-mentioned system of equations

$$\varphi_1=12345-24135, \quad \varphi_4=21435-13245,$$
$$\varphi_2=13425-32145, \quad \varphi_5=31245-14325,$$
$$\varphi_3=14235-43125, \quad \varphi_6=41325-12435,$$

in fact exhibits the six positive pentagons, each accompanied by its stellated negative pentagon, and the formation of the system of equations is thus completely explained; the order of arrangement of the pairs *inter se* (or, what comes to the same thing, the order of arrangement of the suffixes of the φ's) is wholly immaterial.

10. The six pairs of pentagons, or, what is the same thing, the φ's, correspond to each other in pairs in a fivefold manner, *quoad* the numbers 1, 2, 3, 4, 5 respectively; thus, *quoad* 5, the pairs are φ_1 and φ_4, φ_2 and φ_5, φ_3 and φ_6, or say 1 and 4, 2 and 5, 3 and 6. The relation is best seen by means of the positive pentagons; thus, *quoad* 5, in the pentagons 12345 and 21435, the points adjacent to 5 in the one of them are the points 2, 3, and in the other of them the complementary points 1, 4; and so in the other cases. The fivefold correspondence is shown by the symbolical equations

$$1=16,\ 24,\ 35,$$
$$2=12,\ 34,\ 56,$$
$$3=15,\ 23,\ 46,$$
$$4=13,\ 26,\ 35,$$
$$5=14,\ 25,\ 36,$$

which, in fact, indicate the combinations of the φ's which correspond to the several roots of the quintic.

11. It is proper to notice that the right-hand sides of the last-mentioned equations contain all the duads formed with the six numbers 1, 2, 3, 4, 5, 6, each duad once, and once only. There are in all six such synthemes of duads, viz.

12.34.56	12.35.46	12.36.45
13.25.46	13.24.56	13.25.46
14.26.35	14.25.36	14.23.56
15.24.36	15.26.34	15.26.34
16.23.45	16.23.45	16.24.35
12.34.56	12.35.46	12.36.45
13.26.45	13.26.45	13.24.56
14.25.36	14.23.56	14.26.35
15.23.46	15.24.36	15.23.46
16.24.35	16.25.34	16.25.34

which is in fact the theorem whereon depends the existence, for six letters, of a 6-valued function not symmetrical in respect of five letters. There is not any peculiarity in the syntheme of duads which above presented itself; the occurrence of this particular syntheme, instead of any other, arises merely from the arbitrary selection of the suffixes of the φ's.

12. It is hardly necessary to remark that if the pentagon 12345 had been assumed negative instead of positive, the only difference would be that the φ's would have their signs reversed.

13. I proceed now to the calculation of the Auxiliary Equation. As the working is rather easier for that form, I shall in the first instance take for the given quintic the denumerate form

$$(a, b, c, d, e, f\text{Y}v, 1)^5=0.$$

Representing, as before, the roots x_1, x_2, x_3, x_4, x_5 of this equation by 1, 2, 3, 4, 5, and writing

$$12345=12+23+34+45+51, \text{ \&c.}$$

(where on the right-hand side 12, 23, &c. stand for x_1x_2, x_2x_3, &c.), we have to find the equation

$$(\bullet\text{Y}\varphi, 1)^6=0,$$

the roots whereof are

$$\varphi_1=12345-24135, \quad \varphi_4=21435-13245,$$
$$\varphi_2=13425-32145, \quad \varphi_5=31245-14325,$$
$$\varphi_3=14235-43125, \quad \varphi_6=41325-12435.$$

As already remarked, the coefficients are seminvariants, and if the equation is in the first instance calculated for the particular case $f=0$, the terms in f can be separately determined. But putting $f=0$, one of the roots, say 5, becomes $=0$, and the remaining roots 1, 2, 3, 4 are the roots of the quartic equation $(a, b, c, d, e\text{Y}v, 1)^4=0$.

14. Writing for shortness

$$1234=12+23+34, \text{ \&c.},$$

and putting also

$$A=12+34,$$
$$B=13+42,$$
$$C=14+23,$$

then we have

$$\varphi_1 = 1234 - 2413 = 12 + 23 + 34 - 24 - 41 - 13 = A - B + 23 - 14,$$
$$\varphi_2 = 1342 - 3214 = 13 + 34 + 42 - 32 - 21 - 14 = B - C + 34 - 12,$$
$$\varphi_3 = 1423 - 4312 = 14 + 42 + 23 - 43 - 31 - 12 = C - A + 42 - 13,$$

$$\varphi_4 = 2143 - 1324 = 21 + 14 + 43 - 13 - 32 - 24 = A - B - 23 + 14,$$
$$\varphi_5 = 3124 - 1423 = 31 + 12 + 24 - 14 - 42 - 23 = B - C - 34 + 12,$$
$$\varphi_6 = 4132 - 1243 = 41 + 13 + 32 - 12 - 24 - 43 = C - A - 42 + 13.$$

15. We have then

$$(\varphi - \varphi_1)(\varphi - \varphi_4) = (\varphi - A + B)^2 - (14 - 23)^2$$
$$= (\varphi - A + B)^2 - C^2 + 4.1234,$$

where 1234 denotes the product of the four roots; the functions A, B, C, and the product 1234, are each of the degree zero in the coefficients (a, b, c, d, e); and if we put

$$b = -c,$$
$$c = -4ae + bd,$$
$$d = 4ace - ad^2 - b^2e,$$

then we in fact have

$$a\,\Sigma A = -b,$$
$$a^2\Sigma AB = c,$$
$$a^3ABC = -d,$$
$$a.1234 = e.$$

But on the understanding that φ is ultimately to be changed into $a\varphi$, it is allowable, and it will be convenient to write

$$\Sigma A = -b,$$
$$\Sigma AB = c,$$
$$ABC = -d,$$
$$1234 = ae.$$

16. I assume also

$$B + C - A = \alpha,$$
$$C + A - B = \beta,$$
$$A + B - C = \gamma.$$

And we have thus

$$(\varphi - \varphi_1)(\varphi - \varphi_4) = (\varphi + \alpha)(\varphi - \beta) + 4ae, \text{ and } \therefore \text{ also}$$
$$(\varphi - \varphi_2)(\varphi - \varphi_5) = (\varphi + \beta)(\varphi - \gamma) + 4ae,$$
$$(\varphi - \varphi_3)(\varphi - \varphi_6) = (\varphi + \gamma)(\varphi - \alpha) + 4ae,$$

so that the equation in φ is

$$[(\varphi + \beta)(\varphi - \gamma) + 4ae][(\varphi + \gamma)(\varphi - \alpha) + 4ae][(\varphi + \alpha)(\varphi - \beta) + 4ae] = 0.$$

17. To obtain the symmetrical functions of α, β, γ, it is only necessary to remark, that if in the identical equation

$$(1, b, c, d)(\theta, 1)^3 = (\theta - A)(\theta - B)(\theta - C),$$

we put $\frac{1}{3}(\chi + A + B + C)$, $= \frac{1}{3}(\chi - b)$, in the place of θ, the equation becomes

$$(1, b, c, d)(\chi - b, 2)^3 = (\chi + \alpha)(\chi + \beta)(\chi + \gamma),$$

so that we have

$$\begin{aligned} \Sigma\alpha &= -b &&= c, \\ \Sigma\alpha\beta &= -b^2 + 4c &&= -16ae + 4bd - c^2, \\ \alpha\beta\gamma &= b^3 - 4bc + 8d &&= 16ace - 8ad^2 - 8b^2e + 4bcd - c^3. \end{aligned}$$

18. The developed expression for the equation in φ is easily found to be

$$\left.\begin{aligned} &\varphi^0 \\ &+\varphi^1 . -\Sigma\alpha^2 + 12ae \\ &+\varphi^2 . \quad \Sigma\alpha^2\beta^2 - 4ae(\Sigma\alpha^2 + \Sigma\alpha\beta) + 48a^2e^2 \\ &+\varphi \, . -4ae(\alpha - \beta)(\beta - \gamma)(\gamma - \alpha) \\ &+ \quad -\alpha^2\beta^2\gamma^2 + 4ae\alpha\beta\gamma\Sigma\alpha - 16a^2e^2\Sigma\alpha\beta + 64a^3e^3 \end{aligned}\right\} = 0.$$

19. In this equation the coefficient of φ is

$$\begin{aligned} &-4 \; ae . 8(B - A)(C - B)(A - C) \\ &= \quad 32ae \quad (A - B)(B - C)(C - A); \end{aligned}$$

or, neglecting the multiplier a, it is

$$-32 . 1 . 2 . 3 . 4 (1-2)(1-3)(1-4)(2-3)(2-4)(3-4),$$

which is the value for $5 = 0$, of

$$-32(1-2)(1-3)(1-4)(1-5)(2-3)(2-4)(2-5)(3-4)(3-5)(4-5),$$

i. e. the coefficient in question is

$$-32 . 25\sqrt{5}\sqrt{a^4f^4 + \&c.} = -800\sqrt{5}\sqrt{a^4f^4 + \&c.},$$

where $a^4f^4 + \&c.$ denotes the discriminant of the denumerate form

$$(a, b, c, d, e, f)(v, 1)^5.$$

20. The remaining coefficients are rational functions of a, b, c, d, e, which have to be completed by the introduction of the terms in f. We have

Coeff. φ^1

$$\begin{aligned} &= - \quad (\Sigma\alpha)^2 && = -c^2 \\ &\quad + \; 2\Sigma\alpha\beta && \quad + 2(-16ae + 4bd - c^2) \\ &\quad + 12ae && \quad + 12ae. \end{aligned}$$

Coeff. φ^2

$$\begin{aligned} &= \quad (\Sigma\alpha\beta)^2 && = \quad (-16ae + 4bd - c^2)^2 \\ &\quad - \; 2\alpha\beta\gamma\Sigma\alpha && \quad - 2c(16ace - 8ad^2 - 8b^2e + 4bcd - c^3) \\ &\quad - \; 4ae(\Sigma\alpha)^2 && \quad - 4ac^2e \\ &\quad + \; 4ae(\Sigma\alpha\beta) && \quad + 4ae(-16ae + 4bd - c^2) \\ &\quad + 48a^2e^2 && \quad + 48a^2e^2. \end{aligned}$$

Coeff. φ^5

$$\begin{aligned} &= - \quad \alpha^2\beta^2\gamma^2 && = -(16ace - 8ad^2 - 8b^2e + 4bcd - c^3)^2 \\ &+ \ 4ae\alpha\beta\gamma\Sigma\alpha && + 4ace(16ace - 8ad^2 - 8b^2e + 4bcd - c^3) \\ &- 16a^2e^2\Sigma\alpha\beta && - 16a^2e^2(-16ae + 4bd - c^2) \\ &+ 64a^3e^2. && + 64a^3e^3. \end{aligned}$$

21. Effecting the developments, these are

$-32\ ae$	$+240\ a^2e^2$	$+320\ a^3e^3$
$+\ 8\ bd$	$-112\ abde$	$-\ 64\ a^2bde^2$
$-\ 3\ c^2$	$-\ 8\ ac^2e$	$-176\ a^2c^2e^2$
	$+\ 16\ acd^2$	$+224\ a^2cd^2e$
	$+\ 16\ b^2ce$	$-\ 64\ a^2d^4$
	$+\ 16\ b^2d^2$	$+224\ ab^2ce^2$
	$-\ 16\ bc^2d$	$-128\ ab^2d^2e$
	$+\ 3\ c^4$	$-112\ abc^2de$
		$+\ 64\ abcd^3$
		$+\ 28\ ac^4e$
		$-\ 16\ ac^3d^2$
		$-\ 64\ b^4e^2$
		$+\ 64\ b^2cde$
		$-\ 16\ b^2c^2e$
		$-\ 16\ b^2c^2d^2$
		$+\ 8\ bc^4d$
		$-\ 1\ c^6$

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the first of which is in fact complete; the others being completed, the equation in φ is found to be

22. For the denumerate form $(a, b, c, d, e, f)(x, 1)^5 = 0$, the equation in φ is

$a^6\times$		$a^4\times$		$a^2\times$	$-800\sqrt{5a^2}\sqrt{\square}\times$	
$+1$	0	$-32\ ae$	0	$-400\ a^2df$	$+1$	$+4000\ a^3cf^2$
		$+\ 8\ bd$		$+240\ a^2e^2$		$-1600\ a^3def$
		$-\ 3\ c^2$		$+240\ abcf$		$+\ 320\ a^3e^3$
				$-112\ abde$		$-1600\ a^2b^2f^2$
				$-\ 8\ ac^2e$		$-\ 640\ a^2bcef$
				$+\ 16\ acd^2$		$+\ 640\ a^2bd^2f$
				$-\ 64\ b^2f$		$-\ 64\ a^2bde^2$
				$+\ 16\ b^2ce$		$-\ 80\ a^2c^2df$
				$+\ 16\ b^2d^2$		$-176\ a^2c^2e^2$
				$-\ 16\ bc^2d$		$+224\ a^2cd^2e$
				$+\ 3\ c^4$		$-\ 64\ a^2d^4$
(						$+384\ ab^2ef$ $\quad (x, 1)^5 = 0,$
						$-192\ ab^2cdf$
						$+224\ ab^2ce^2$
						$-128\ ab^2d^2e$
						$+\ 48\ abc^2f$
						$-112\ abc^2de$
						$+\ 64\ abcd^3$
						$+\ 28\ ac^4e$
						$-\ 16\ ac^3d^2$
						$-\ 64\ b^4e^2$
						$+\ 64\ b^3cde$
						$-\ 16\ b^2c^2e$
						$-\ 16\ b^2c^2d^2$
						$+\ 8\ bc^4d$
						$-\ 1\ c^6$

where $\square$, $= a^4f^4 +$ &c., denotes the discriminant for the denumerate form.

I proceed now to form the expression for $\varphi_1\varphi_4+\varphi_2\varphi_3+\varphi_5\varphi_6$.

23. Writing for convenience x in the place of the root 5, we have

$$\varphi_1=A-B+\{23-14+x(1+4-2-3)\}$$
$$\varphi_4=A-B-\{23-14+x(1+4-2-3)\},$$

or

$$\varphi_1\varphi_4=(A-B)^2-\{23-14+x(1+4-2-3)\}^2.$$

The terms without x are, as before, $(A-B)^2-C^2+4.1234$, or $-\alpha\beta+4.1234$, and we have

$$\varphi_1\varphi_4=-\alpha\beta+4.1234$$
$$+2x(1+4-2-3)(14-23)$$
$$-x^2(1+4-2-3)^2;$$

and in like manner

$$\varphi_2\varphi_3=-\beta\gamma+4.1234$$
$$+2x(1+2-3-4)(12-34)$$
$$-x^2(1+2-3-4)^2,$$

and

$$\varphi_3\varphi_6=-\gamma\alpha+4.1234$$
$$+2x(1+3-4-2)(13-42)$$
$$-x^2(1+3-4-2)^2.$$

24. The roots 1, 2, 3, 4, contained in these expressions explicitly, and in α, β, γ, are the roots of the equation $\frac{1}{v-x}(a, b, c, d, e, f)(v, 1)^4=0$, or, what is the same thing,

$$(a', b', c', d', e')(v, 1)^4=0,$$

where

$$a'=a,$$
$$b'=ax+b,$$
$$c'=ax^2+bx+c,$$
$$d'=ax^3+bx^2+cx+d,$$
$$e'=ax^4+bx^3+cx^2+dx+e.$$

Omitting, as before, a power of a, which is ultimately restored, we have

$$\varphi_1\varphi_4+\varphi_2\varphi_3+\varphi_5\varphi_6=-\Sigma\alpha\beta+12a'e'$$
$$+2x\Sigma(1+4-2-3)(14-23)$$
$$-x^2\Sigma(1+4-2-3)^2,$$

where the Σ's in the second and third lines denote each of them the sum of the three terms obtained by the cyclical permutations of 2, 3, 4.

The first line is

$$(16a'e'-4b'd'+c'^2)+12a'e'$$
$$=28a'e'-4b'd'+1c'^2.$$

The second line is $2x$ into $\Sigma 1^2 2-3\Sigma 123$,

$$=(-b'c'+3a'd')+3a'd';$$

or it is

$$=2x(6a'd'-1b'c');$$

and the third line is $-x^2$ into $3\Sigma 1^2-2\Sigma 12$,

$$=3(b'^2-2a'c')-2a'c';$$

or it is

$$=x^2(8a'c'-3b'^2).$$

Hence, combining the three terms,

$$\begin{aligned}\varphi_1\varphi_4+\varphi_2\varphi_5+\varphi_3\varphi_6=&28a'e'-4b'd'+1c'^2\\&+x(12a'd'-2b'c')\\&+x^2(8a'c'-3b'^2),\end{aligned}$$

or substituting for (a', b', c', d', e') their values, the right-hand side is

$$=(40a^2,\ 32ab,\ 28ac-8b^2,\ 44ad-8bc,\ 28ae-4bd+1c^2\nabla(x, 1)^4,$$

where x stands for x_3, and on the left-hand side the factor a^2 is to be restored.

25. Writing for shortness

$$(*\nabla(x,\ 1)^4=(40a^2,\ 32ab,\ 28ac-8b^2,\ 44ad-8bc,\ 28ae-4bd+1c^2\nabla(x,\ 1)^4,$$

the equation is

$$a^2(\varphi_1\varphi_4+\varphi_2\varphi_5+\varphi_3\varphi_6)=(*\nabla(x_3,\ 1)^4;$$

and the system of equations to which this belongs is

$$\begin{aligned}a^2(\varphi_1\varphi_6+\varphi_2\varphi_4+\varphi_3\varphi_5)&=(*\nabla(x_1,\ 1)^4,\\a^2(\varphi_1\varphi_2+\varphi_3\varphi_4+\varphi_5\varphi_6)&=(*\nabla(x_2,\ 1)^4,\\a^2(\varphi_1\varphi_3+\varphi_2\varphi_5+\varphi_4\varphi_6)&=(*\nabla(x_3,\ 1)^4,\\a^2(\varphi_1\varphi_5+\varphi_2\varphi_6+\varphi_3\varphi_4)&=(*\nabla(x_4,\ 1)^4,\\a^2(\varphi_1\varphi_4+\varphi_2\varphi_4+\varphi_3\varphi_6)&=(*\nabla(x_5,\ 1)^4;\end{aligned}$$

so that the roots x_1, x_2, x_3, x_4, x_5 will be rational functions of the combinations $\varphi_1\varphi_6+\varphi_2\varphi_4+\varphi_3\varphi_5$, &c. respectively, of the equation in φ.

26. Passing now to the standard form $(a, b, c, d, e, f)(v, 1)^5=0$, the equation in φ is

$a^4\times$		$-100\,a^4\times$		$2000\,a^3\times$	$-800\,a^3\sqrt{5}\sqrt{\square}\times$	$40000\times$
$+1$	0	$+1\ ae$	0	$-2\ a^2df$	$+1$	$+1\ a^3cf^2$
		$-4\ bd$		$+3\ a^2e^2$		$-2\ a^3def$
		$+3\ c^2$		$+6\ abcf$		$+1\ a^3e^3$
				$-14\ abde$		$-1\ a^2b^2f^2$
				$-2\ ac^2e$		$-4\ a^2bcef$
				$+8\ acd^2$		$+8\ a^2bd^2f$
				$-4\ b^2f$		$-2\ a^2bde^2$
				$+10\ b^2ce$		$-2\ a^2c^2df$
				$+20\ b^2d^2$		$-11\ a^2c^2e^2$
				$-40\ bc^2d$		$+28\ a^2cd^2e$
				$+15\ c^4$		$-16\ a^2d^4$
						$+6\ ab^2ef$
						$-12\ ab^2cdf$
						$+35\ ab^2ce^2$
						$-40\ ab^2d^2e$
						$+6\ abc^2f$
						$-70\ abc^2de$
						$+80\ abcd^2$
						$+35\ ac^4e$
						$-40\ ac^3d^2$
						$-25\ b^4e^2$
						$+100\ b^3cde$
						$-50\ b^2c^2e$
						$-100\ b^2c^2d^2$
						$+100\ bc^4d$
						$-25\ c^6$

$(\varphi, 1)^6=0,$

where $\square$, $= a^4f^4+$ &c., denotes the Discriminant for the Standard form.

27. And if we put

$$(*)(x, 1)^4=20(2a^2,\ 8ab,\ 22ac-10b^2,\ 18ad-10bc,\ 7ae-10bd+5c^2)(x, 1)^4,$$

then we have

$$a^2(\varphi_1\varphi_6+\varphi_2\varphi_5+\varphi_3\varphi_4)=(*)(x_1, 1)^4,$$
$$a^2(\varphi_1\varphi_5+\varphi_2\varphi_4+\varphi_3\varphi_6)=(*)(x_2, 1)^4,$$
$$a^2(\varphi_1\varphi_4+\varphi_2\varphi_3+\varphi_5\varphi_6)=(*)(x_3, 1)^4,$$
$$a^2(\varphi_1\varphi_3+\varphi_2\varphi_6+\varphi_4\varphi_5)=(*)(x_4, 1)^4,$$
$$a^2(\varphi_1\varphi_2+\varphi_3\varphi_5+\varphi_4\varphi_6)=(*)(x_5, 1)^4,$$

which lead to rational expressions for the roots x_1, x_2, x_3, x_4, x_5 in terms of the combinations $\varphi_1\varphi_6+\varphi_2\varphi_5+\varphi_3\varphi_4$, &c. respectively.

28. Consider now the quintic function

$$U=(a, b, c, d, e, f)(x, y)^5=a(x-\alpha y)(x-\beta y)(x-\gamma y)(x-\delta y)(x-\epsilon y);$$

and treating the numbers 1, 2, 3, 4, 5 as corresponding to α, β, γ, δ, ϵ respectively, write

$$\Phi=\widehat{12345}\,\widehat{24135};$$

where

$$\widehat{12345}=\widehat{12}+\widehat{23}+\widehat{34}+\widehat{45}+\widehat{51},\ \text{&c.},$$

in which $\widehat{12}$, &c. denote respectively

$$\frac{1}{y^2}\,\frac{1}{x-\alpha y}\cdot\frac{1}{x-\beta y},\ \text{&c.}$$

Then we have

$$\Phi = \frac{a}{U}[\varphi x - \chi y],$$

where

$$\varphi = 12345 - 24135,$$

and

$$12345 = 12+23+34+45+51 = \alpha\beta+\beta\gamma+\gamma\delta+\delta\varepsilon+\varepsilon\alpha,$$
$$24135 = 24+41+13+35+52 = \beta\delta+\delta\alpha+\alpha\gamma+\gamma\varepsilon+\varepsilon\beta,$$

and where

$$\chi = (12345) - (24135),$$

and

$$(12345) = 123+234+345+451+512 = \alpha\beta\gamma+\beta\gamma\delta+\gamma\delta\varepsilon+\delta\varepsilon\alpha+\varepsilon\alpha\beta,$$
$$(24135) = 241+413+135+352+524 = \beta\delta\alpha+\delta\alpha\gamma+\alpha\gamma\varepsilon+\gamma\varepsilon\beta+\varepsilon\beta\delta.$$

29. In fact,

$$\Phi = \frac{a}{U}\left\{(\widehat{12345}) - (\widehat{24135})\right\},$$

where

$$(\widehat{12345}) = \widehat{123}+\widehat{234}+\widehat{345}+\widehat{451}+\widehat{512},$$
$$(\widehat{24135}) = \widehat{241}+\widehat{413}+\widehat{135}+\widehat{352}+\widehat{524},$$

where $\widehat{123}$, &c. denote respectively

$$\frac{1}{y^3}(x-\alpha y)(x-\beta y)(x-\gamma y), \text{ &c.},$$

and $(\widehat{12345})-(\widehat{24135})$ thus presents itself as a cubic function divided by y^5. But in this cubic function the coefficients of x^3, x^2y vanish. For the coefficient of any power of x will be

$$123+234+345+451+512-241-413-135-352-524,$$

where, first, for x^3, 123, &c. denote respectively unity; the coefficient of x^3 therefore vanishes. Next, for x^2y, 123, &c. denote respectively $-(1+2+3)$, &c. $(=\alpha+\beta+\gamma)$, and the coefficient of x^2y also vanishes. But for xy^3, 123, &c. denote respectively $12+23+31 (=\alpha\beta+\beta\gamma+\gamma\alpha)$, &c. respectively; the positive terms are

$$(12+23+31)+(23+34+42)+(34+45+53)+(45+51+14)+(51+12+25),$$

which are

$$= 2(12+23+34+45+51)+(24+41+13+35+52)$$
$$= 2.12345+24153;$$

and the negative terms, taken positively, are

$$(24+41+12)+(41+13+34)+(13+35+51)+(35+52+23)+(52+24+45),$$

which are

$$= (12+23+34+45+51)+2(24+41+13+35+52)$$
$$= 12345+2.24135;$$

so that the difference, or coefficient of xy^3, is

$$= 12345-24135,$$

which is $=\varphi$.

And for y^5, 123, &c. denote respectively $-123 (=-\alpha\beta\gamma)$, &c., so that the coefficient of y^5 is $=\chi$.

30. The cubic function is therefore $=\phi xy^3-\chi y^4$; and dividing by y^4, we have

$$\Phi=\frac{a}{U}(\phi x-\chi y).$$

Φ is thus a fractional covariantive function, the leading coefficient whereof is ϕ, and the equation for the determination of Φ is consequently that deduced from the equation for ϕ, by replacing therein the seminvariants by the corresponding covariants. The equation is

$$\left\{\begin{array}{l} U^6, \\ 0, \\ -100\,U^4\,\text{Tab. No. 14}, \\ 0, \\ +2000\,U^3[6(\text{Tab. No. 14})^2-4\,\text{Tab. No. 20}], \\ -800\,U^2\sqrt{5}\sqrt{\text{discr.}}=\text{Tab. No. 26}, \\ (\mathfrak{A}, \mathfrak{B}, \mathfrak{C}, \mathfrak{D}, \mathfrak{E}, \mathfrak{F}, \mathfrak{G})(x, y)^6, \end{array}\right\}(\Phi, 1)^6=0,$$

where the Tables referred to are those of my Second Memoir on Quantics; the coefficients are in regard to (x, y) of the orders 30, —, 22, —, 14, 10, 6 respectively. The last coefficient, being of the degree 6 in the coefficients (a, b, c, d, e, f), is not given in the Tables; it is therefore merely indicated by $(\mathfrak{A}, \mathfrak{B}, \mathfrak{C}, \mathfrak{D}, \mathfrak{E}, \mathfrak{F}, \mathfrak{G})(x, y)^6$, the leading coefficient $\mathfrak{A}$ being of course the last coefficient in the equation for ϕ, to the standard form.

I refrain from at present entering into the consideration of the values of the expressions $\Phi_1\Phi_4+\Phi_2\Phi_5+\Phi_3\Phi_6$, &c.

XIV. *A Seventh Memoir on Quantics.* By ARTHUR CAYLEY, *Esq.*, *F.R.S.*

Received February 28,—Read March 14, 1861.

THE present memoir relates chiefly to the theory of ternary cubics. Since the date of my Third Memoir on Quantics, M. ARONHOLD has published the continuation of his researches on ternary cubics, in the memoir "Theorie der homogenen Functionen dritten Grades von drei Veränderlichen," Crelle, t. lv. pp. 97–191 (1858). He there considers two derived contravariants, linear functions of the fundamental ones, and which occupy therein the position which the fundamental contravariants PU, QU do in my Third Memoir; in the notation of the present memoir these derived contravariants are

$$\begin{aligned} YU &= \quad 3T.PU - 4S.QU, \\ ZU &= -48S^2.PU + T.\ QU; \end{aligned}$$

and for the canonical form $x^3+y^3+z^3+6lxyz$, they acquire respectively the factor $(1+8l^3)^2$, viz. in this case

$$\begin{aligned} YU &= (1+8l^3)^2\{ \quad l\,(\xi^3+\eta^3+\zeta^3) - 3\xi\eta\zeta\} \\ ZU &= (1+8l^3)^2\{(1+2l^3)(\xi^3+\eta^3+\zeta^3)+18l^2\xi\eta\zeta\}. \end{aligned}$$

The derived contravariants have with the covariants U, HU, even a more intimate connexion than have the contravariants PU, QU; and the advantage of the employment of YU, ZU fully appears by M. ARONHOLD'S memoir.

But the conclusion is, not that the contravariants PU, QU are to be rejected, but that the system is to be completed by the addition thereto of two derived covariants, linear functions of U, HU; these derived covariants, suggested to me by M. ARONHOLD'S memoir, are in the present memoir called CU, DU; their values are

$$\begin{aligned} CU &= \quad -T.U + 24S.HU \\ DU &= \quad 8S^2.U - 3T.\ HU: \end{aligned}$$

and for the canonical form $x^3+y^3+z^3+6lxyz$, they acquire respectively, not indeed $(1+8l^3)^2$, but the simple power $(1+8l^3)$, as a factor, viz. in this case

$$\begin{aligned} CU &= (1+8l^3)\{ \quad (-1+4l^3)(x^3+y^3+z^3) + \quad 18lxyz\} \\ DU &= (1+8l^3)\{l^2(\quad 5+4l^3)(x^2+y^2+z^2) + 3(1-10l^3)xyz\}; \end{aligned}$$

it was in fact by means of this condition as to the factor $(1+8l^3)$, that the foregoing expressions for CU, DU were obtained*.

* M. ARONHOLD, in a letter dated Berlin, 17 June 1861, has pointed out to me that the covariants CU, DU are in his notation P_{S_T}, P_{T_T}, and that they belong to the forms called Conjugate Forms, § 27 of his memoir. But the explicit development of the properties of these covariants is not on this account the less interesting. Added 20 Sept. 1861.—A. C.

The formulæ of my Third Memoir and those of M. ARONHOLD are by this means brought into harmony and made parts of a whole; instead of the two intermediates

$$\alpha U+6\beta HU, \quad 6\alpha PU+\beta QU,$$

in Tables 68 and 69 of my Third Memoir, or of the intermediates

$$\alpha U+6\beta HU, \quad -2\alpha YU+2\beta ZU,$$

of M. ARONHOLD'S theory, we have the four intermediates

$$\alpha U+6\beta HU, \quad -2\alpha YU+2\beta ZU, \quad 2\alpha CU-2\beta DU, \quad 6\alpha PU+\beta QU,$$

in Tables 74, 75, 76, and 77 of the present memoir. These four Tables embrace the former results, and the new ones which relate to the covariants CU, DU; and they are what is most important in the present memoir. I have, however, excluded from the Tables, and I do not in the memoir consider (otherwise than incidentally) the covariant of the sixth order ΘU, or the contravariant (reciprocant) FU.

I have given in the memoir a comparison of my notation with that of M. ARONHOLD. A short part of the memoir relates to the binary cubic and the binary quartic, viz. each of these quantics has a covariant of its own order, forming with it an intermediate $\alpha U+\beta W$, the covariants whereof contain quantics in (α, β), the coefficients of which are invariants of the original quantic. The formulæ which relate to these cases are in fact given in my Fifth Memoir, but they are reproduced here in order to show the relations between the quantics in (α, β) contained in the formulæ. As regards the binary quartic, these results are required for the discussion of the like question in regard to the ternary cubic, viz. that of finding the relations between the different quantics in (α, β) contained in the formulæ relating to the ternary cubic. Some of these relations have been obtained by M. HERMITE in the memoir "Sur les formes cubiques à trois indéterminées," (Liouville, t. iii. pp. 37–40 (1858), and in that "Sur la Résolution des équations du quatrième degré," Comptes Rendus, xlvi. p. 715 (1858), and by M. ARONHOLD in his memoir already referred to; and in particular I reproduce and demonstrate some of the results in the last-mentioned memoir of M. HERMITE. But the relations in question are in the present memoir exhibited in a more complete and systematic form.

The paragraphs and Tables of the present memoir are numbered consecutively with those of my former memoirs on Quantics.

231. For the binary cubic $(a, b, c, d)(x, y)^3$, if U be the cubic itself, HU the Hessian, ΦU the cubicovariant, and $\square$ the discriminant (see Fifth Memoir, Nos. 115, 118), then

Covariant and other Tables, No. 71.

$$\begin{aligned} H(\alpha U+\beta\Phi U) &= (\alpha^3-\beta^2\square)HU, \\ \Phi(\alpha U+\beta\Phi U) &= -\tfrac{1}{3}\partial_\beta(\alpha^3-\beta^2\square).U \\ &\quad +\tfrac{1}{3}\partial_\alpha(\alpha^3-\beta^2\square).\Phi U, \\ \square(\alpha U+\beta\Phi U) &= (\alpha^3-\beta^2\square)^2\square, \end{aligned}$$

so that the quantics in (α, β) all of them depend on $\alpha^3-\beta^2\square$.

232. For the binary quartic $(a, b, c, d, e)(x, y)^4$, if U be the quartic itself, HU the Hessian, ΦU the cubicovariant, I, J, the quadrinvariant and the cubinvariant, and $\square(=I^3-27J^2)$ the discriminant (see Fifth Memoir, Nos. 128, 134), then

Table No. 72.

$$\Phi(\alpha U+6\beta HU)= (1, 0, -9I, -54J)(\alpha, \beta)^2\Phi U,$$

$$H(\alpha U+6\beta HU)=-\tfrac{1}{18}\partial_x(1, 0, -9I, -54J)(\alpha, \beta)^3.U + \tfrac{1}{3}\partial_x(1, 0, -9I, -54J)(\alpha, \beta)^3.HU,$$

$$I(\alpha U+6\beta HU)= (I, 18J, 3I^2)(\alpha, \beta)^2,$$

$$J(\alpha U+6\beta HU)= (J, I^2, 9IJ, -I^3+54J^2)(\alpha, \beta)^3,$$

$$\square(\alpha U+6\beta HU)= \bullet(1, 0, -18I, 108J, 81I^2, 9721J, 2916J^2)(\alpha, \beta)^6\square$$
$$=[(1, 0, -9I, -54J)(\alpha, \beta)^2]^3\square.$$

233. Writing for the moment

$$G=(1, 0, -9I, -54J)(\alpha, \beta)^2,$$

then the Hessian, cubicovariant, and discriminant of this cubic function of (α, β) are respectively

$$HG=- 3(I, 18J, 3I^2)(\alpha, \beta)^2,$$

$$\Phi G= 54(J, I^2, 9IJ, -I^3+54J^2)(\alpha, \beta)^3,$$

$$\square G= -108\square;$$

so that the covariants of the intermediate $\alpha U+6\beta HU$ are all of them expressible by means of the cubic function G.

It may be noticed that G is what the left-hand side of the equation

$$4(HU)^3-4I.HU.U^3+JU^3=-(\Phi U)^3$$

(see Fifth Memoir, No. 128) becomes on writing therein $\alpha, -6\beta$, for U, HU respectively, and throwing out the factor 4.

234. I take the opportunity of remarking with respect to a binary quartic $U=(a, b, c, d)(x, y)^4$, that the Hessian of the cubicovariant, to fix the numerical factor, say $-\frac{1}{3}\{\partial_x^2\Phi U.\partial_y^2\Phi U-(\partial_x\partial_y\Phi U)^2\}$, is

$$=I^2U^2-36J.U.HU+12I(HU)^2,$$

which is

$$=\left(IU-\frac{18J}{I}HU\right)^2+\frac{12}{I^2}(I^3-27J^2)(HU)^2;$$

or if $I^3-27J^2\neq 0$, that is if the quartic has a pair of equal factors, the Hessian of the cubicovariant is a perfect square,

235. Coming now to the ternary cubic $U=(a, b, c, f, g, h, i, j, k, l)(x, y, z)^3$, I give in the first place the following comparison of my notation with that of M. Aronhold.

• The coefficient $2916J^2$ is in the Fifth Memoir erroneously given as $-2916J^2$.

ARONHOLD.	CAYLEY.
f	U
Δf	$-6HU$
S	4S
T	$-T$
R	$-R$
S_f	6PU
T_f	$-2QU$
J	$64S^3 \div T^2$
P_f	$-2YU$
Q_f	2ZU
Θ	CU
Θ_1	DU
Θ	Θ
H	Θ
F	$-FU$
$\downarrow$	$2(\Theta_{11}U - TU^3 + 4SU.HU)$,

where the notations YU, ZU (to correspond to M. ARONHOLD'S P_f, Q_f) and the notations CU, DU are first employed in the present memoir. I remark in regard to $P_f(=-2YU)$, where, as already mentioned,

$$YU = (1+8l^2)^3\{l(\xi^3+\eta^3+\zeta^3) - 3\xi\eta\zeta\},$$

that in my Memoir on Curves of the Third Order*, I was led incidentally to the curve

$$l(\xi^3+\eta^3+\zeta^3) - 3\xi\eta\zeta = 0,$$

and that I there gave the equation

$$3T.PU - 4S.QU = (1+8l^2)^3\{l(\xi^3+\eta^3+\zeta^3) - 3\xi\eta\zeta\}.$$

But the curve

$$(1+2l^3)(\xi^3+\eta^3+\zeta^3) + 18l^2\xi\eta\zeta = 0,$$

which corresponds to $(Q_f = 2ZU)$, does not occur in that memoir.

236. I remark, further, in regard to M. ARONHOLD'S Θ, H, that these are what he calls "Zwischenformen," viz. they are covariants of the cubic and of the adjoint linear form $\xi x+\eta y+\zeta z$, or as they might be termed *Contracovariants*. For the canonical form $U = x^3+y^3+z^3+6lxyz$, the value of $\frac{1}{2}\Theta$ is

$$(yz - l^2x^2,\ zx - l^2y^2,\ xy - l^2z^2,\ l^2yz - lx^2,\ l^2zx - ly^2,\ l^2xy - lz^2)(\xi, \eta, \zeta)^2,$$

which is a form which occurs incidentally in my memoir last referred to (see p. 427). The value of H in the same case is

$$(-2l(1+2l^3)x^2 - 6lyz, \ldots, -(1+4l^3)x^2 + 2l(1+2l^3)yz, \ldots)(\xi, \eta, \zeta)^2,$$

which does not occur in that memoir. In my Third Memoir on Quantics I purposely abstained from the consideration of any such forms.

237. My covariants ΘU and $\Theta_1 U$ involved unsymmetrically the cubic and its Hessian,

* Philosophical Transactions, t. cxlvii. (1857) see p. 427.

and it did not occur to me how a similar covariant, such as M. ARONHOLD's ψ, which involves the two functions symmetrically, was to be formed. Let (A, B, C) be the first derived functions, (a, b, c, f, g, h) the second derived functions of the cubic U, and (A', B', C') the first derived functions, (a', b', c', f', g', h') the second derived functions of the Hessian HU, then disregarding numerical factors, we have

$$\Theta U = (bc - f^2, \ldots gh - af, \ldots \}A', B', C')^2,$$
$$\Theta_1 U = (b'c' - f'^2, \ldots g'h' - a'f', \ldots \}A, B, C)^2,$$

and

$$\psi = (bc' + b'c - 2ff', \ldots gh' + g'h - af' - a'f, \ldots \}A, B, C\}A', B', C');$$

and considering $U=0$ as the equation of a curve of the third order, the equations $\Theta U=0$, $\Theta_1 U=0$, $\psi=0$ have the following significations, viz. $\Theta U=0$ is the locus of a point, such that its second or line polar with respect to the Hessian touches its first or conic polar with respect to the cubic: $\Theta_1 U$ is the locus of a point such that its second or line polar with respect to the cubic touches its first or conic polar with respect to the Hessian: and $\psi=0$ is the locus of a point such that its second or line polar with respect to the cubic, and its second or line polar with respect to the Hessian are reciprocals (that is, each passes through the pole of the other of them) with respect to the conic which is the envelope of a line cutting the first or conic polar of the point with respect to the cubic, and the first or conic polar of the point with respect to the Hessian in two pairs of points which are harmonically related to each other: such being in fact the immediate interpretation of the analytical formula. But this in passing.

238. The formulæ (Tables 68 and 69 of my Third Memoir) for the discriminants of the intermediates $\alpha U + 6\beta HU$ and $6\alpha PU + \beta QU$ respectively are

$$R(\alpha U + 6\beta HU) = [(1, 0, -24S, -8T, -48S^2 \}(\alpha, \beta)^4]^3 R,$$
$$R(6\alpha PU + \beta QU) = [(48S, 8T, -96S^2, -24TS, -T^3 - 16S^3 \}(\alpha, \beta)^4]^3 R.$$

In M. HERMITE's paper in the 'Comptes Rendus,' already referred to, there are given between these quantics in (α, β) certain relations which (although less simple than the relations that will afterwards be obtained) I now proceed to investigate. Putting in the first formula $\alpha \div \beta = p$, and in the second formula $\alpha \div \beta = \theta$, we have

$$R(pU + 6HU) = 0, \text{ if } (1, 0, -24S, -8T, -48S^2 \}(p, 1)^4 = 0,$$
$$R(6\theta PU + QU) = 0, \text{ if } (48S, 8T, -96S^2, -24TS, -T^3 - 16S^3 \}(\theta, 1)^4 = 0,$$

which equations in p, θ, are about to be considered in place of the quantics from which they respectively arise.

239. It is convenient to write*

$$A = 4S,$$
$$B = \sqrt[3]{T^2 - 64S^3}$$

* A is (ARONHOLD's and) HERMITE's S, B is HERMITE's S_1, and p, q, θ, Λ are HERMITE's δ, δ_1, Δ, d: there is a slight inaccuracy in three of his formulæ, which should be

$$\Delta = -\tfrac{1}{4}\frac{1}{S}\left(T + \frac{S^2}{\delta_1}\right), \quad \delta_1 = \frac{24S^2}{f'\delta}, \quad \delta = \frac{24S^2}{f_1\delta},$$

corresponding to formulæ in the present memoir.

(so that $T^3=A^3+B^3$ and, for the canonical form,

$$A=-4l+4l^4, \quad B=1+8l^3).$$

Making this change, and joining to the equation in p that derived from it by writing q for p, and interchanging A, B, we have the three equations

$$(1, \quad 0, -6A, -8T, -3A^2 \quad \text{X}(p, 1)^4=0,$$
$$(1, \quad 0, -6B, -8T, -3B^2 \quad \text{X}(q, 1)^4=0,$$
$$(12A, 8T, -6A^2, -6TA, -T^2-\tfrac{1}{4}A^3\text{X}(\theta, 1)^4=0.$$

240. The signification of the equation in q is as follows, viz. if the quantic

$$U=(*\text{X}(x, y, z)^3$$

is transformed into the canonical form

$$X^3+Y^3+Z^3+6lXYZ$$

by means of the linear equations

$$(x, y, z)=(\Lambda\text{X}(X, Y, Z),$$

where Λ is a matrix, then using the same letter Λ to represent the determinant formed out of this matrix, or determinant of substitution, we have

$$q=\frac{3}{\Lambda^2},$$

so that the equation in q is one that presents itself in the question of the reduction of the cubic to its canonical form.

In fact the linear transformation gives

$$S\Lambda^4=-l+l^4,$$
$$T\Lambda^6=1-20l^3-8l^6,$$

and thence

$$(T^2-64S^3)\Lambda^{12}=(1+8l^3)^3,$$

which, writing B^2 in the place of T^2-64S^3, becomes

$$B^2\Lambda^{12}=(1+8l^3)^3, \text{ or}$$
$$B\,\Lambda^4=1+8l^3, \text{ or } 8l^3=B\Lambda^4-1,$$

whence also

$$8T\Lambda^6=8-20(B\Lambda^4-1)-(B\Lambda^4-1)^2$$
$$=27-18\,B\Lambda^4-B^2\Lambda^8,$$

or, as this may be written,

$$\frac{81}{\Lambda^8}-\frac{54B}{\Lambda^4}-\frac{24T}{\Lambda^2}-3B^2=0,$$

which, putting therein $q=\frac{3}{\Lambda^2}$, becomes

$$(1, 0, -6B, -8T, -3B^2\text{X}(q, 1)^4=0,$$

the above-mentioned equation in q.

241. The relation between θ and q is

$$\theta=-\tfrac{1}{2}\frac{T}{A}+\frac{B^2}{2Aq},$$

as may be verified without difficulty. That between θ and p is

$$\theta = \tfrac{1}{4}\left(p + \frac{A}{p}\right),$$

as appears by the identical equation

$$(12A, 8T, -6A^2, -6TA, -T^3 - \tfrac{1}{4}A^3)(\tfrac{1}{4}(p + \tfrac{A}{p}), 1)^4$$
$$= \frac{1}{64p^4}(3A, 8T, -12A^2, -72TA, -46A^3 - 64T^2, -72TA^2, -12A^4, 8TA^2, 3A^3)(p, 1)^6$$
$$= \frac{1}{64p^4}(1, 0, -6A, -8T, -3A^2)(p, 1)^4 . (3A, 8T, 6A^2, 0, -A^3)(p, 1)^4,$$

where the second factor of the product on the right-hand side is

$$-\frac{p^4}{A}(1, 0, -6A, -8T, -3A^2)(\frac{A}{p}, 1)^4.$$

The relation between p and q is then at once found to be

$$q = \frac{\frac{2B^2}{A}}{p + \frac{A}{p} + \frac{2T}{A}},$$

or (since p, q and A, B may be simultaneously interchanged)

$$p = \frac{\frac{2A^2}{B}}{q + \frac{B}{q} + \frac{2T}{B}}.$$

242. Let the equations in p, q be represented by $\varphi p = 0$, $\psi q = 0$ respectively; then we have

$$\varphi p = p^4 - 6Ap^2 - 8p - 3A^2,$$

and therefore

$$\tfrac{1}{4}\varphi' p = p^3 - 3Ap - 2T,$$

whence

$$\tfrac{1}{4}p\varphi' p = p^4 - 3Ap^2 - 2Tp$$
$$= 3Ap^2 + 6Tp + 3A^2,$$

and therefore

$$q = \frac{2B^2 p}{\frac{1}{12}p\varphi' p} = \frac{24B^2}{\varphi' p},$$

with a like formula for p, that is we have

$$q = \frac{24B^2}{\varphi' p}, \quad p = \frac{24A^2}{\psi' q},$$

which with the equation

$$\theta = \tfrac{1}{4}\left(p + \frac{A}{p}\right),$$

are the system of equations connecting θ, p, q.

243. As already remarked, we have to consider the two derived covariants,

$$CU = -T.U + 24S.HU,$$
$$DU = 8S^2.U - 3T.HU,$$

and the two derived contravariants,

$$YU = \quad 3T\,.\,PU - 4S\,.\,QU,$$
$$ZU = -48S^3\,.\,PU + \;T\,.\,QU,$$

which for the canonical form $x^3+y^3+z^3+6lxyz$ are as follows:—

Table No. 70 (addition to).

$$CU = (1+8l^3)\,[(-1+4l^3)(x^3+y^3+z^3) + \quad 18l\,xyz],$$
$$DU = (1+8l^3)\,[l^3\,(5+4l^3)(x^3+y^3+z^3) + 3(1-10l^3)xyz],$$
$$YU = (1+8l^3)^2[\quad l(\xi^3+\eta^3+\zeta^3) \quad -3\xi\eta\zeta],$$
$$ZU = (1+8l^3)^2[(1+2l^3)(\xi^3+\eta^3+\zeta^3) + 18l^2\xi\eta\zeta].$$

244. We have, conversely,

$$3R.\;\; U = 3T\,.\,CU + 24S\,.\,DU,$$
$$3R\,.\,HU = 8S^3\,.\,CU + \;T\,.\,DU,$$

and

$$-3R\,.\,PU = \;T\,.\,YU + 4S\,.\,QU,$$
$$-3R\,.\,QU = 48S^3\,.\,YU + 3T\,.\,PU,$$

and also the following formulæ, viz. if

$$2\alpha CU - 2\beta DU = \alpha' U + 6\beta' HU;$$

then

$$\alpha' = -2T\alpha - 16S^3\beta,$$
$$\beta' = \;\; 8S\alpha + \;\; T\beta,$$

which give, conversely,

$$\alpha = \tfrac{1}{2R}(\quad T\alpha' + 16S^3\beta'),$$
$$\beta = \tfrac{1}{2R}(-8S\alpha' - \;2T\beta');$$

and moreover, if

$$-2\alpha YU + 2\beta ZU = 6\alpha' PU + \beta' QU,$$

then

$$\alpha' = -(\quad T\alpha + 16S^3\beta),$$
$$\beta' = -(-8S\alpha - \;2T\beta),$$

which give, conversely,

$$\alpha = -\tfrac{1}{2R}(-2T\alpha' - 16S^3\beta'),$$
$$\beta = -\tfrac{1}{2R}(\quad 8S\alpha' + \;\; T\beta');$$

so that the relation between (α, β) and (α', β') in the present case is similar to that between (α', β') and (α, β) in the former case. It may be noticed that in all these systems of linear equations, the determinant of transformation is a multiple of $64S^3 - T^2 (= R)$.

245. It will be convenient, before giving the Tables for the covariants of

$$\alpha U+6\beta HU,\quad 2\alpha CU-2\beta DU,\quad 6\alpha PU+\beta QU,\quad 2\alpha YU-2\beta ZU,$$

which replace Tables 68 and 69 of my Third Memoir, to give the following separate Table of the quantics in (α, β) which enter into the expressions of the invariants in Tables 68 and 69, and in these new Tables.

Table No. 73.

$$(1,\ 0,\ -24S,\ -8T,\quad -48S^2)(\alpha, \beta)^4,$$

$$(S,\ T,\ 24S^2,\ 4TS,\ T^2-48S^2)(\alpha, \beta)^4,$$

$$(T,\ 96S^2,\ 60TS,\ 20T^2,\ 240TS^2,\ -48T^2S+4608S^4,\ -8T^3+576TS^2)(\alpha, \beta)^6.$$

$$(48S,\ 8T,\ -96S^2,\ -24TS,\ -T^2-16S^2)(\alpha, \beta)^4.$$

$$\left\{\begin{array}{l} T^2+192S^2, \\ 128TS^2, \\ 18T^2S+384S^4, \\ T^3+64TS^2, \\ 5T^2S^2-64S^4, \end{array}\right\}(\alpha, \beta)^4,$$

$$\left\{\begin{array}{l} -8T^3+4608TS^2, \\ 1920T^2S^2+73728S^4, \\ 360T^2S+38400TS^3, \\ 20T^4+8960T^2S^2, \\ 840T^2S^3+7680TS^4, \\ 36T^4S+384T^2S^4+24576S^6, \\ 1T^5-40T^2S^3+2560TS^5, \end{array}\right\}(\alpha, \beta)^6,$$

where the first part of the Table contains the quantics in (α, β) which relate to the forms $\alpha U+6\beta HU$ and $2\alpha YU-2\beta ZU$, and the second part of the Table contains the quantics in (α, β) which relate to the forms $6\alpha PU+\beta QU$ and $-2\alpha CU+2\beta DU$.

The quantics in (α, β) contained in the foregoing Table are in the sequel indicated by means of their leading coefficients; as thus,

$$(1,\ 0,\ -24S,\ ..)(\alpha, \beta)^4,\quad (S,\ T,\ ..)(\alpha, \beta)^4,\quad (T^2+192S^2,\ ..)(\alpha, \beta)^4,\ \&c.$$

246. It is easy to see what transformations must be performed on the results in Tables 68 and 69, in order to obtain the new Tables. Thus, in the formation of Table 74, Table 68 gives $\alpha U+6\beta HU$ and $H(\alpha U+6\beta HU)$, and from these $C(\alpha U+6\beta HU)$, $D(\alpha U+6\beta HU)$ have to be found: the same Table gives also $P(\alpha U+6\beta HU)$, $Q(\alpha U+6\beta HU)$, but the expressions of these quantities YU, ZU have to be intro-

duced in the place of PU, QU; and from the expressions so transformed are deduced also the expressions for $Y(\alpha U+6\beta HU)$, $Z(\alpha U+6\beta HU)$. Table 75 is to be deduced from Table 69 by writing therein (α', β') for (α, β), and then putting $6\alpha' PU+\beta' QU = 2\alpha YU-2\beta ZU$, which, as is seen above, gives α', β' as functions of α, β and of the invariants S and T; but in some of the formulæ YU, ZU, have to be introduced in the place of PU, QU. And so for the Tables 76 and 77. The actual effectuation of the transformations would, it is almost needless to remark, be very laborious, but the forms of the results are easily foreseen, and the results can then be verified by means of one or two coefficients only. The new Tables are

Table No. 74.

$$R(\alpha U+6\beta HU) = R\times[(1, 0, -24S, .. \Box(\alpha, \beta)^4]^3,$$
$$S(\alpha U+6\beta HU) = (S, T, .. \Box(\alpha, \beta)^4,$$
$$T(\alpha U+6\beta HU) = [(T, 96S^2, .. \Box(\alpha, \beta)]^6.$$

$$(\alpha U+6\beta HU) = \alpha U+6\beta HU,$$
$$H(\alpha U+6\beta HU) = -\tfrac{1}{24}\times\begin{cases}\partial_\beta(1, 0, -24S, .. \Box(\alpha, \beta)^4 . U\\ -6\partial_\alpha(1, 0, -24S, .. \Box(\alpha, \beta)^4 . HU,\end{cases}$$
$$C(\alpha U+6\beta HU) = (1, 0, -24S, .. \Box(\alpha, \beta)^4\times\begin{cases}\partial_\beta(S, T, .. \Box(\alpha, \beta)^4 . U\\ -6\partial_\alpha(S, T, .. \Box(\alpha, \beta)^4 . HU,\end{cases}$$
$$D(\alpha U+6\beta HU) = \tfrac{1}{12}(1, 0, -24S, .. \Box(\alpha, \beta)^4\times\begin{cases}\partial_\beta(T, 96S^2, .. \Box(\alpha, \beta)^6 . U\\ -6\partial_\alpha(T, 96S^2, .. \Box(\alpha, \beta)^6 . HU,\end{cases}$$
$$P(\alpha U+6\beta HU) = -\tfrac{1}{3R}\times\begin{cases}\partial_\beta(S, T, .. \Box(\alpha, \beta)^4 . YU\\ +\partial_\alpha(S, T, .. \Box(\alpha, \beta)^4 . ZU,\end{cases}$$
$$Q(\alpha U+6\beta HU) = -\tfrac{1}{6R}\times\begin{cases}\partial_\beta(T, 96S^2, .. \Box(\alpha, \beta)^6 . YU\\ +\partial_\alpha(T, 96S^2, .. \Box(\alpha, \beta)^6 . ZU,\end{cases}$$
$$Y(\alpha U+6\beta HU) = [(1, 0, -24S, .. \Box(\alpha, \beta)^4]^2\times(-2\alpha YU+2\beta ZU),$$
$$Z(\alpha U+6\beta HU) = \tfrac{1}{4}[(1, 0, -24S, .. \Box(\alpha, \beta)^4]^3\times\begin{cases}\partial_\beta(1, 0, -24S, .. \Box(\alpha, \beta)^4 . YU\\ +\partial_\alpha(1, 0, -24S, .. \Box(\alpha, \beta)^4 . ZU.\end{cases}$$

No. 75.

$$R(-2\alpha YU+2\beta ZU)=-4096R^3\times[(S, T, .. \nabla(\alpha, \beta)^4)^3],$$
$$S(-2\alpha YU+2\beta ZU)= -R^2\times(1, 0, -24S, .. \nabla(\alpha, \beta)^4),$$
$$T(-2\alpha YU+2\beta ZU)= -8R^4\times(T, 96S^2, .. \nabla(\alpha, \beta)^6).$$

$$-2\alpha YU+2\beta ZU = -2\alpha YU+2\beta ZU,$$

$$H(-2\alpha YU+2\beta ZU)=-\tfrac{1}{3}R\times\begin{cases}\partial_\beta(S, T, .. \nabla(\alpha, \beta)^4).YU\\ +\partial_\alpha(S, T, .. \nabla(\alpha, \beta)^4).ZU,\end{cases}$$

$$C(-2\alpha YU+2\beta ZU)=-16R^4(S, T, .. \nabla(\alpha, \beta)^4)\times\begin{cases}\partial_\beta(1, 0, -24S, .. \nabla(\alpha, \beta)^4).YU\\ +\partial_\alpha(1, 0, -24S, .. \nabla(\alpha, \beta)^4).ZU,\end{cases}$$

$$D(-2\alpha YU+2\beta ZU)=-\tfrac{32}{3}R^4(S, T, .. \nabla(\alpha, \beta)^4)\times\begin{cases}\partial_\beta(T, 96S^2, .. \nabla(\alpha, \beta)^6).ZU\\ +\partial_\alpha(T, 96S^2, .. \nabla(\alpha, \beta)^6).ZU,\end{cases}$$

$$P(-2\alpha YU+2\beta ZU)=\tfrac{1}{6}R^3\times\begin{cases}\partial_\beta(1, 0, -24S, .. \nabla(\alpha, \beta)^4).U\\ -6\partial_\alpha(1, 0, -24S, .. \nabla(\alpha, \beta)^4).HU,\end{cases}$$

$$Q(-2\alpha YU+2\beta ZU)=-\tfrac{1}{3}R^3\times\begin{cases}\partial_\beta(T, 96S^2, .. \nabla(\alpha, \beta)^6).U\\ -6\partial_\alpha(T, 96S^2, .. \nabla(\alpha, \beta)^6).HU,\end{cases}$$

$$Y(-2\alpha YU+2\beta ZU)=256R^6[(S, T, .. \nabla(\alpha, \beta)^4)^3]\times(\alpha U+6\beta HU),$$

$$Z(-2\alpha YU+2\beta ZU)=-512R^7[(S, T, .. \nabla(\alpha, \beta)^4)^3]\times\begin{cases}\partial_\beta(S, T, .. \nabla(\alpha, \beta)^4).U\\ -6\partial_\alpha(S, T, .. \nabla(\alpha, \beta)^4).HU.\end{cases}$$

No. 76.

$$R(2\alpha CU-2\beta DU) = -4096R^4\times[(T^3+192S^2\quad ,..\,(\alpha,\beta)^4]^3,$$
$$S(2\alpha CU-2\beta DU) = -R^2\times(48S,\ 8T\quad ,..\,(\alpha,\beta)^4,$$
$$T(2\alpha CU-2\beta DU) = -8R^2\times(-8T^3+4608TS^2,..\,(\alpha,\beta)^6.$$

$$2\alpha CU\ -2\beta DU = 2\alpha CU-2\beta DU,$$

$$H(2\alpha CU-2\beta DU) = \tfrac{1}{8}\times\begin{cases}\partial_\beta(T^3+192S^2,..\,(\alpha,\beta)^4.CU\\ +\partial_\alpha(T^3+192S^2,..\,(\alpha,\beta)^4.DU,\end{cases}$$

$$C(2\alpha CU-2\beta DU) = -16R^2(T^3+192S^2,..\,(\alpha,\beta)^4\times\begin{cases}\partial_\beta(48S,\ 8T\ ,..\,(\alpha,\beta)^4.CU\\ +\partial_\alpha(48S,\ 8T\ ,..\,(\alpha,\beta)^4.DU,\end{cases}$$

$$D(2\alpha CU-2\beta DU) = \tfrac{1}{8}R^2(T^3+192S^2\quad ,..\,(\alpha,\beta)^4\times\begin{cases}\partial_\beta(-8T^3+4608TS^2,..\,(\alpha,\beta)^6.CU\\ +\partial_\alpha(-8T^3+4608TS^2,..\,(\alpha,\beta)^6.DU,\end{cases}$$

$$P(2\alpha CU-2\beta DU) = \tfrac{1}{8}R\times\begin{cases}6\partial_\beta(48S,\ 8T,..\,(\alpha,\beta)^4.PU\\ -\partial_\alpha(48S,\ 8T,..\,(\alpha,\beta)^4.QU,\end{cases}$$

$$Q(2\alpha CU-2\beta DU) = \tfrac{1}{8}R\times\begin{cases}6\partial_\beta(-8T^3+4608TS^2,..\,(\alpha,\beta)^6.PU\\ -\partial_\alpha(-8T^3+4608TS^2,..\,(\alpha,\beta)^6.QU,\end{cases}$$

$$Y(2\alpha CU-2\beta DU) = 216R^2\ [(T^3+192S^2,..\,(\alpha,\beta)^4]^2\times(6\alpha PU+\beta QU),$$

$$Z(2\alpha CU-2\beta DU) = -512R^3[(T^3+192S^2,..\,(\alpha,\beta)^4]^3\times\begin{cases}6\partial_\beta(T^3+192S^2,..\,(\alpha,\beta)^4.PU\\ -\partial_\alpha(T^3+192S^2,..\,(\alpha,\beta)^4.QU.\end{cases}$$

No. 77.

$$R(6\alpha PU+\beta QU)=R^3\times[(48S,\ 8T\quad ,..\ \Omega(\alpha,\beta)^4]^3,$$

$$S(6\alpha PU+\beta QU)=\quad (T^3+192S^3\quad ,..\ \Omega(\alpha,\beta)^4,$$

$$T(6\alpha PU+\beta QU)=\quad (-8T^3+4608TS^3,..\ \Omega(\alpha,\beta)^6$$

$$6\alpha PU+\beta QU)=\quad 6\alpha PU+\beta QU,$$

$$H(6\alpha PU+\beta QU)=-\tfrac{1}{72}\times\begin{cases}6\partial_\beta(48S,\ 8T,..\ \Omega(\alpha,\beta)^4\,.\,PU\\ -\ \partial_\alpha(48S,\ 8T,..\ \Omega(\alpha,\beta)^4\,.\,QU,\end{cases}$$

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$$C(6\alpha PU+\beta QU)=\quad -(48S,\ 8T,..\ \Omega(\alpha,\beta)^4\times$$
$$\begin{cases}6\partial_\beta(T^3+192S^3,..\ \Omega(\alpha,\beta)^4\,.\,PU\\ -\ \partial_\alpha(T^3+192S^3,..\ \Omega(\alpha,\beta)^4\,.\,QU,\end{cases}$$

$$D(6\alpha PU+\beta QU)=\quad \tfrac{1}{72}(48S,\ 8T,..\ \Omega(\alpha,\beta)^4\times$$
$$\begin{cases}6\partial_\beta(-8T^3+4608TS^3,..\ \Omega(\alpha,\beta)^6\,.\,PU\\ -\ \partial_\alpha(-8T^3+4608TS^3,..\ \Omega(\alpha,\beta)^6\,.\,QU,\end{cases}$$

$$P(6\alpha PU+\beta QU)=\quad \tfrac{1}{3R}\times\begin{cases}\partial_\beta(T^3+192S^3,..\ \Omega(\alpha,\beta)^4\,.\,CU\\ +\partial_\alpha(T^3+192S^3,..\ \Omega(\alpha,\beta)^4\,.\,DU,\end{cases}$$

$$Q(6\alpha PU+\beta QU)=\quad \tfrac{1}{6R}\times\begin{cases}\partial_\beta(-8T^3+4608TS^3,..\ \Omega(\alpha,\beta)^6\,.\,CU\\ +\partial_\alpha(-8T^3+4608TS^3,..\ \Omega(\alpha,\beta)^6\,.\,DU,\end{cases}$$

$$Y(6\alpha PU+\beta QU)=\quad -\tfrac{1}{2}R.[(48S,\ 8T,..\ \Omega(\alpha,\beta)^4]^2\times$$
$$(-2\alpha CU+2\beta DU),$$

$$Z(6\alpha PU+\beta QU)=\quad -\tfrac{1}{4}R[(48S,\ 8T,..\ \Omega(\alpha,\beta)^4]^2\times$$
$$\begin{cases}\partial_\beta(48S,\ 8T,..\ \Omega(\alpha,\beta)^4\,.\,CU\\ +\partial_\alpha(48S,\ 8T,..\ \Omega(\alpha,\beta)^4\,.\,DU.\end{cases}$$

247. It will be noticed how Tables 74 and 75 form a system involving only the quantics in (α, β) contained in the first part of Table 73, and how, in like manner, Tables 76 and 77 form a system involving only the quantics in (α, β) contained in the second part of Table 73; and, moreover, how in each pair of Tables the covariants, &c. correspond to each other as follows, viz.—

R, S, T, 1, H, C, D, P, Q, Y, Z to
S, R, T, Y, P, Z, Q, H, D, 1, C.

Thus in Table 74,—the formula for $H(\alpha U + 6\beta HU)$,
and in Table 75,—the formula for $P(2\alpha YU - 2\beta ZU)$,
each of them involve the same factor

$$\begin{cases} \partial_\beta(1, 0, -24S, .. \nabla(\alpha, \beta)^4 . U \\ -6\partial_\alpha(1, 0, -24S, .. \nabla(\alpha, \beta)^4 . HU, \end{cases}$$

and so in all the other cases.

248. The quantics in (α, β) in each part of the foregoing Table 73 are covariantively connected together. In fact, considering the function $(1, 0, -24S, .. \nabla(\alpha, \beta)^4$, which for shortness I call G, we have

$$\begin{aligned} G &= (1, 0, -24S, .. \nabla(\alpha, \beta)^4, \\ IG &= 0, \\ JG &= 4(64S^3 - T^2) = 4R, \\ \square G &= (IG)^3 - 27(JG)^2 = -432R^2, \\ HG &= -4(S, T, .. \nabla(\alpha, \beta)^4, \\ \Phi G &= 2(T, 96S^2, .. \nabla(\alpha, \beta)^6. \end{aligned}$$

The last-mentioned formulæ, by the aid of Table 72, give rise to the following more general system in which they are themselves included.

Table No. 78.

$$\begin{aligned} \lambda G + 6\mu HG &= \lambda G + 6\mu HG, \\ H(\lambda G + 6\mu HG) &= 36\mu^2 G + \lambda^2 HG, \\ \Phi(\lambda G + 6\mu HG) &= (\lambda^3 - 216R\mu^3)2(T, 96S^2, \ldots \nabla(\alpha, \beta)^6, \\ I(\lambda G + 6\mu HG) &= 72R\lambda\mu, \\ J(\lambda G + 6\mu HG) &= 4R(\lambda^3 + 216R\mu^3), \\ \square(\lambda G + 6\mu HG) &= -432R^2(\lambda^3 - 216R\mu^3)^2. \end{aligned}$$

The expression for $H(\lambda G + 6\mu HG)$, putting therein $\lambda = 0$, shows that, to a numerical factor *près*, H.HG is equal to G, and hence, disregarding numerical factors, we may say that each of the quartics $(1, 0, 24S, .. \nabla(\alpha, \beta)^4$, $(S, T, .. \nabla(\alpha, \beta)^4$, is the Hessian of the other of them, and that the sextic $(T, 96S^2, .. \nabla(\alpha, \beta)^6$ is the cubicovariant of each of them.

249. Similarly, if the function $(48S, 8T, .. \chi(\alpha, \beta)^4$ is for shortness called G, then we have

$$\begin{aligned} G &= (48S, 8T, .. \chi(\alpha, \beta)^4, \\ IG &= 0, \\ JG &= 4(64S^3 - T^2)^2 = 4R^2, \\ \square G &= (IG)^3 - 27(JG)^2 = -432R^4, \\ HG &= -4(T^2 + 192S^3, .. \chi(\alpha, \beta)^4, \\ \Phi G &= -2(-8T^3 + 4608TS^2, .. \chi(\alpha, \beta)^4. \end{aligned}$$

The last-mentioned formulæ, by the aid of the same Table 72, give rise to the more general system in which they are themselves included.

Table No. 79.

$$\begin{aligned} \lambda G + 6\mu HG &= \lambda G + 6\mu HG, \\ H(\lambda G + 6\mu HG) &= 36R^2\mu^2 G + \lambda^2 HG, \\ \Phi(\lambda G + 6\mu HG) &= (\lambda^3 - 216R^2\mu^3) \times -2(-8T^3 + 4608TS^2, .. \chi(\alpha, \beta)^6, \\ I(\lambda G + 6\mu HG) &= 72R^2\lambda\mu, \\ J(\lambda G + 6\mu HG) &= 4R^2(\lambda^3 + 216R^2\mu^3), \\ \square(\lambda G + 6\mu HG) &= -432R^4(\lambda^3 - 216R^2\mu^3)^2. \end{aligned}$$

The expression for $H(\lambda G + 6\mu HG)$, putting therein $\lambda = 0$, shows that, to a numerical factor *près*, H . HG is equal to G; so that, disregarding numerical factors, we may say that each of the quartics $(48S, T, .. \chi(\alpha, \beta)^4$, $(T^2 + 192S^3, .. \chi(\alpha, \beta)^4$, is the Hessian of the other of them, and that the sextic $(-8T^3 + 4608TS^2, .. \chi(\alpha, \beta)^6$ is the cubicovariant of each of them.

250. But besides this, the quantics in (α, β) in the two parts of the Table 73 are linearly connected together: the linear relations in question are in fact the equations whereon depend the expressions for the invariants in Tables 76 and 77 as deduced from those in Tables 74 and 75; and in the order of proof, they precede the formulæ in these four Tables. The linear relations are

Table No. 80.

$$\begin{aligned} &(1, 0, -24S, .. \quad \chi - 2T\alpha - 16S^2\beta, \quad 8S\alpha + T\beta)^4 = -16R\,(T^2 + 192S^3, .. \quad \chi(\alpha, \beta)^4, \\ &(S, T, .. \quad \chi - 2T\alpha - 16S^2\beta, \quad 8S\alpha + T\beta)^4 = \quad R^2(48S, 8T, .. \quad \chi(\alpha, \beta)^4, \\ &(T, 96S^2, .. \quad \chi - 2T\alpha - 16S^2\beta, \quad 8S\alpha + T\beta)^6 = -\ 8R^3(-8T^3 + 4608TS^2, .. \chi(\alpha, \beta)^6. \end{aligned}$$

$$\begin{aligned} &(48S, 8T, .. \quad \chi \quad T\alpha + 16S^2\beta, \ -8S\alpha - 2T\beta)^4 = -16R^2(S, T, .. \quad \chi(\alpha, \beta)^4, \\ &(T^2 + 192S^3, .. \quad \chi \quad T\alpha + 16S^2\beta, \ -8S\alpha - 2T\beta)^4 = -\ R^2(1, 0, -24S, .. \quad \chi(\alpha, \beta)^4, \\ &(-8T^3 + 4608TS^2, .. \chi \quad T\alpha + 16S^2\beta, \ -8S\alpha - 2T\beta)^6 = -\ 8R^3(T, 96S^2, .. \quad \chi(\alpha, \beta)^6. \end{aligned}$$

Hence, attending to the remarks on the Tables 78 and 79, we may say that the quartics

$$(1, 0, -24S, .. \nabla(\alpha, \beta)^4,$$
$$(48S, T, .. \quad \nabla(\alpha, \beta)^4,$$

which belong to the two parts respectively of Table 73, and which are related, the first of them to the discriminants of $\alpha U + 6\beta HU$ and $2\alpha YU - 2\beta ZU$, and the second to the discriminants of $6\alpha PU + \beta QU$, $-2\alpha CU + 2\beta DU$, have these relations to each other, viz. each is a linear transformation of the Hessian of the other of them, and the cubicovariant of each is a linear transformation of the cubicovariant of the other.

XV. *On Systems of Linear Indeterminate Equations and Congruences.* *By* HENRY J. STEPHEN SMITH, *M.A., Fellow and Mathematical Lecturer of Balliol College, Oxford. Communicated by* J. J. SYLVESTER, *Esq., F.R.S.*

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THE theory of the solution, in positive or negative integral numbers, of systems of linear indeterminate equations, requires the consideration of rectangular matrices, the constituents of which are integral numbers. It will therefore be convenient to explain the meaning which we shall attach to certain phrases and symbols relating to such matrices.

A matrix containing p constituents in every horizontal row, and q in every vertical column, is a matrix of the type $q \times p$. We shall employ the symbol $\left\| \underset{\mathrm{A}}{^{q\times p}} \right\|$, or (when it is not necessary that the type of the matrix should be indicated in its symbol) the simpler symbol $\|\mathrm{A}\|$ to represent the matrix

$$\left\| \begin{matrix} \mathrm{A}_{1,1}, \mathrm{A}_{1,2}, \ldots\ldots \mathrm{A}_{1,p} \\ \mathrm{A}_{2,1}, \mathrm{A}_{2,2}, \ldots\ldots \mathrm{A}_{2,p} \\ \ldots\ldots\ldots\ldots \\ \mathrm{A}_{q,1}, \mathrm{A}_{q,2}, \ldots\ldots \mathrm{A}_{q,p} \end{matrix} \right\|$$

If $\|\mathrm{A}\|$ and $\|\mathrm{B}\|$ be two matrices of the same type, the equation $\|\mathrm{A}\|=\|\mathrm{B}\|$ indicates that the constituents of $\|\mathrm{A}\|$ are respectively equal to the constituents of $\|\mathrm{B}\|$; whereas the equation $|\mathrm{A}|=|\mathrm{B}|$ will merely express that the determinants of $\|\mathrm{A}\|$ are equal to the corresponding determinants of $\|\mathrm{B}\|$. The determinants of a matrix are, of course, the determinants of the greatest square matrices contained in it; similarly, its minor determinants of order i are the determinants of the square matrices of the type $i \times i$ that are contained in it. Matrices of the types $n\times(m+n)$ and $m\times(m+n)$ are said to be of complementary types; if $\|\mathrm{A}\|$ and $\|\mathrm{B}\|$ be two such matrices, we shall employ the equation

$$|\mathrm{A}|=|\mathrm{B}|$$

to express that each determinant of $\|\mathrm{A}\|$ is equal to that determinant of $\|\mathrm{B}\|$, by which it is multiplied in the development of the determinant of the square matrix $\left\|\begin{matrix}\mathrm{A}\\ \mathrm{B}\end{matrix}\right\|$. When m and n are both uneven numbers, the signs of the determinants $\left|\begin{matrix}\mathrm{A}\\ \mathrm{B}\end{matrix}\right|$ and $\left|\begin{matrix}\mathrm{B}\\ \mathrm{A}\end{matrix}\right|$ are different: this occasions a certain ambiguity of sign in the interpretation of the equation $|\mathrm{A}|=|\mathrm{B}|$, which, however, will occasion no inconvenience. If $m=n$, the matrices $\|\mathrm{A}\|$ and $\|\mathrm{B}\|$ are at once of the same, and of complementary types; so that, in this case, the equation $|\mathrm{A}|=|\mathrm{B}|$ may stand for either of two very different sets of equations; but this

also is an imperfection of the notation here employed, which it is sufficient to have pointed out. If k denote any quantity whatever, it is hardly necessary to state that the equality

$$|A| = k \times |B|$$

implies that the determinants of $\|A\|$ are respectively k times the corresponding determinants of $\|B\|$.

Let $\|P\|$ be a square matrix of the type $n \times n$, and $\|Q\|$ a matrix of the type $n \times (n+m)$ (where $m \geq 0$), we shall understand by the matrix compounded of $\|P\|$ and $\|Q\|$, the matrix $\|X\|$ of the same type as $\|Q\|$, the constituents of which are defined by the equation

$$X_{i,j} = P_{i,1} Q_{1,j} + P_{i,2} Q_{2,j} + \ldots + P_{i,n} Q_{n,j};$$

and we shall write

$$\|X\| = \|P\| \times \|Q\|;$$

in this equation $\|Q\|$ is said to be *premultiplied* by $\|P\|$, and $\|P\|$ to be *post-multiplied* by $\|Q\|$. This definition will suffice for our present purpose; as the only case of composition which we shall have to consider, is that in which the vertical dimensions of the matrices to be compounded are all equal, and in which every premultiplying matrix is square, so that if an oblong matrix present itself at all in a series of matrices to be compounded, it will occupy the last place in the series.

By the greatest divisor of a matrix we are to understand the greatest common divisor of the determinants of the matrix. If the matrix be square, its greatest divisor is, consequently, the determinant of the matrix. A *prime matrix* is one of which the greatest divisor is unity; *i. e.* the determinants of which are relatively prime. A prime square matrix (*i. e.* a matrix of which the determinant is unity) we shall call a *unit-matrix*.

In any system of linear equations, whether defective or redundant, or neither, we shall understand by the matrix of the system the matrix formed by the coefficients of the unknown quantities. If to this matrix we add an additional vertical column, composed of the absolute terms of the equations, the resulting matrix we shall term (for brevity) the *augmented* matrix of the system.

Lastly, when we have occasion to consider square matrices, the constituents of which, excepting those on the principal diameter, are zero, we shall represent them by symbols of the form

$$\|q_1, q_2, q_3, \ldots q_n\|,$$

where $q_1, q_2, \ldots q_n$ are the constituents of the principal diameter.

Art. 2. If every determinant of the augmented matrix of a redundant system of linear equations is equal to zero, while the determinants of the unaugmented matrix are not all equal to zero, the system admits of one solution, and one only. And in particular if the matrix of the system be a prime matrix, the values of the unknown quantities which satisfy the system are integral numbers. For these values may be expressed as fractions having for their denominators any one of the determinants of the matrix; and these determinants are relatively prime.

Let $\|A\|$ be a given prime matrix of the type $n \times (n+m)$, $\|K\|$ a given matrix of the same type connected with $\|A\|$ by the equation

$$|K| = k \times |A|, \quad \ldots \ldots \ldots \ldots \quad (1.)$$

which implies that k is the greatest divisor of $\|K\|$; then the symbolic equation

$$\|K\| = \|k\| \times \|A\|, \quad \ldots \ldots \ldots \ldots \quad (2.)$$

in which $\|k\|$ denotes a square matrix of the type $n \times n$, will admit of one solution, and one only.

For, to determine $k_{r,1}, k_{r,2}, \ldots k_{r,n}$, the constituents of the rth horizontal row of $\|k\|$, we have the redundant system

$$\left.\begin{aligned} K_{r,i} = A_{1,i}\, k_{r,1} + A_{2,i}\, k_{r,2} + \ldots + A_{n,i}\, k_{r,n}, \\ i = 1, 2, 3, \ldots n+m \end{aligned}\right\} \quad \ldots \ldots \quad (3.)$$

which is involved in the symbolic equation (2.). The matrix of this system is the prime matrix $\|A\|$; and the determinants of its augmented matrix are all equal to zero; for, by virtue of equation (1.), they are equal to the determinants

$$-\frac{1}{k} \times \begin{vmatrix} K_{r,1}, & K_{r,2}, & \ldots . & K_{r,n+m} \\ K_{1,1}, & K_{1,2}, & \ldots . & K_{2,n+m} \\ K_{2,1}, & K_{2,2}, & \ldots . & K_{2,n+m} \\ \ldots & \ldots & \ldots & \ldots \\ K_{n,1}, & K_{n,2}, & \ldots . & K_{n,n+m} \end{vmatrix}$$

in which two horizontal rows are identical. Thus the system (3.), and consequently the equation (2.), admits of one solution, and one only. It is evident that the determinant of $\|k\|$ is k. The case in which $m=0$ is not included in this demonstration; its proof, however, presents no difficulty, and may be omitted here.

A particular case of this theorem (that in which $n=2$) occurs in the 'Disquisitiones Arithmeticæ' (see art. 234 of that work).

Art. 3. If every determinant of the augmented matrix of a redundant system of linear congruences be divisible by the modulus, while the greatest divisor of the unaugmented matrix is prime to the modulus, the system is resoluble and admits of only one solution. For if the modulus be represented by $P \times Q \times R \ldots$, P, Q, R.. denoting powers of unequal primes, one (at least) of the determinants of the unaugmented matrix is prime to P, one (at least) is prime to Q, &c.; whence it may be inferred that the system is resoluble for each of the modules P, Q, R..., and admits of only one solution for each of them; it is therefore resoluble for their product $P \times Q \times R \ldots$, and admits of only one solution for that modulus.

Let $\|K\|$ denote (as in the preceding article) a given matrix of the type $n \times (n+m)$, of which k is the greatest divisor; and let it be required to find the complete solution of the symbolic equation

$$\|K\| = \|k\| \times \|A\|, \quad \ldots \ldots \ldots \ldots \quad (4.)$$

in which $\|k\|$ is a square matrix of which the determinant is k, $\|A\|$ a prime matrix of

the same type as $\|\mathrm{K}\|$, and in which the constituents of $\|\mathrm{A}\|$ and $\|k\|$ are the unknown numbers.

We shall first obtain a particular solution of this equation, and then show how from any particular solution the complete solution may be deduced.

We may suppose that the constituents of any horizontal row of $\|\mathrm{K}\|$ admit of no common divisor but unity; for if $\delta_1, \delta_2, \ldots \delta_n$ be the greatest common divisors of the constituents of the horizontal rows of $\|\mathrm{K}\|$, we find

$$\|\mathrm{K}\| = \|\delta_1, \delta_2, \delta_3, \ldots \delta_n\| \times \|\mathrm{K}'\|, \quad \ldots \quad (5.)$$

$\|\mathrm{K}'\|$ denoting a matrix the constituents of which are derived from those of $\|\mathrm{K}\|$ by the relation

$$\mathrm{K}'_{r,s} = \frac{1}{\delta_r} \mathrm{K}_{r,s}; \quad \ldots \quad (6.)$$

so that the solution of equation (4.) depends on the solution of a similar equation for the matrix $\|\mathrm{K}'\|$, in which the constituents of each horizontal row are relatively prime. Let then the matrix $\left\| \begin{matrix} r \times (n+m) \\ \mathrm{K} \end{matrix} \right\|$, *i. e.* the matrix

$$\left\| \begin{matrix} \mathrm{K}_{1,1}, \mathrm{K}_{1,2}, \ldots \mathrm{K}_{1,n+m} \\ \mathrm{K}_{2,1}, \mathrm{K}_{2,2}, \ldots \mathrm{K}_{2,n+m} \\ \ldots \ldots \ldots \\ \mathrm{K}_{r,1}, \mathrm{K}_{r,2}, \ldots \mathrm{K}_{r,n+m} \end{matrix} \right\} \quad [1 \leqq r < n],$$

be a prime matrix, but let the matrix $\left\| \begin{matrix} (r+1) \times (n+m) \\ \mathrm{K} \end{matrix} \right\|$ admit of a greatest divisor μ. Determine $\omega_1, \omega_2, \ldots \omega_r$ by the system of congruences,

$$\left. \begin{matrix} \mathrm{K}_{1,i}\omega_1 + \mathrm{K}_{2,i}\omega_2 + \ldots \mathrm{K}_{r,i}\omega_r \equiv \mathrm{K}_{r+1,i}, \text{ mod. } \mu, \\ i = 1, 2, 3, \ldots n+m \end{matrix} \right\} \quad \ldots \quad (7.)$$

(which, as we have just seen, is always resoluble), and in $\|\mathrm{K}\|$ replace the constituents $\mathrm{K}_{r+1,i}$ by the numbers

$$\frac{1}{\mu}[\mathrm{K}_{r+1,i} - \Sigma_{s=1}^{s=r} \omega_s \mathrm{K}_{s,i}];$$

we thus deduce from $\|\mathrm{K}\|$ another matrix $\|\mathrm{K}''\|$ connected with it by the relation $|\mathrm{K}| = \mu \times |\mathrm{K}''|$, and such that the matrix of its first $r+1$ horizontal rows is prime. By proceeding in this manner, we shall at last obtain a prime matrix $\|\mathrm{A}_0\|$, which satisfies the equation $|\mathrm{K}| = k \times |\mathrm{A}_0|$; we may then, by the method of the last article, determine a square matrix $\|k_0\|$ satisfying the equation

$$\|\mathrm{K}\| = \|k_0\| \times \|\mathrm{A}_0\|, \quad \ldots \quad (8.)$$

and thus obtain a particular solution of the proposed equation (4.).

To deduce the general solution of that equation, let $\|k_1\|$ and $\|\mathrm{A}_1\|$ be any two matrices satisfying it. We have therefore the equality

$$\|k_1\| \times \|\mathrm{A}_1\| = \|k_0\| \times \|\mathrm{A}_0\|, \quad \ldots \quad (9.)$$

which evidently implies that

$$|A_1|=|A_0|; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

whence, by the theorem of the last article,

$$\|A_1\|=\|\alpha\|\times\|A_0\|, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

$\|\alpha\|$ denoting a unit-matrix. Combining (9.) and (11.), we find

$$\|k_1\|\times\|\alpha\|\times\|A_0\|=\|k_0\|\times\|A_0\|, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

whence, by the same theorem, it follows that

$$\|k_1\|\times\|\alpha\|=\|k_0\|; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

or, which is the same thing,

$$\|k_1\|=\|k_0\|\times\|\alpha\|^{-1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

$\|\alpha\|^{-1}$ denoting the matrix reciprocal to $\|\alpha\|$. The complete solution of equation (4.) is therefore contained in the formulæ

$$\left.\begin{aligned}\|A\|&=\|\alpha\ \|\times\|A_0\|\\ \|K\|&=\|K_0\|\times\|\alpha\ \|^{-1}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15.)$$

$\|\alpha\|$ denoting an arbitrary unit-matrix of the type $n\times n$, and $\|A_0\|$, $\|k_0\|$ being any two matrices that satisfy the equation.

In this, as in the preceding article, we have for simplicity excluded the case in which $m=0$, and the matrices $\|K\|$ and $\|A\|$ are squares. But it is readily seen that no exception is presented by this particular case.

Art. 4. Let

$$\left.\begin{gathered}A_{i,1}\,x_1+A_{i,2}\,x_2+\ldots+A_{i,n+m}\,x_{n+m}=0,\\ i=1,\ 2,\ 3,\ \ldots\ n\end{gathered}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16.)$$

represent a system of indeterminate equations of which the matrix is $\|A\|$. We shall suppose that the determinants of $\|A\|$ are not all equal to zero, *i. e.* that the system is independent; so that its *index of indeterminateness* (or the excess of the number of indeterminates above the number of really independent equations) is m. If we take r solutions of the system, for example the solutions

$$\left.\begin{gathered}x_{s,1},\ x_{s,2},\ x_{s,3}\ \ldots\ x_{s,n+m},\\ s=1,\ 2,\ 3,\ \ldots r\end{gathered}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17.)$$

it is evident that if $r>m$, the determinants of the matrix $\|x\|$ are all equal to zero. If $r\leqq m$, and if the determinants of the matrix $\|x\|$ be not all equal to zero, the solutions (17.) are said to form *a set of* r *independent solutions*; if $r=m$, they form *a complete set of independent solutions.* A *set of relatively prime solutions* is an independent set of which the matrix is prime; a complete set of relatively prime solutions may be called, for a reason which will presently appear, a *fundamental set of solutions.* It is always possible, in an infinite number of ways, to assign complete sets of independent solutions of a system of equations of the form (16.). Among the methods by which this may be accomplished, we shall select one which depends on the following principle:—

If $\left\| \underset{a}{r \times (m+r)} \right\|$ represent any matrix of the type $r \times (m+r)$, the determinants of which are not all equal to zero, and if $\lambda_1, \lambda_2, \ldots \lambda_{m+r}$ be integers which satisfy the equations

$$\left.\begin{matrix} \Sigma_{k=1}^{k=m+r} a_{i,k} \lambda_k = 0, \\ i = 1, 2, 3 \ldots r \end{matrix}\right\} \quad \ldots \ldots \ldots \ldots \quad (18.)$$

while $a_{r+1,1}, a_{r+1,2}, a_{r+1,3} \ldots\ldots a_{r+1,m+r}$ are integers satisfying the inequality

$$\Sigma_{k=}^{k=m+r} a_{r+1,k} \lambda_k \gtrless 0, \quad \ldots \ldots \ldots \ldots \quad (19.)$$

the determinants of the matrix

$$\left\| \underset{a}{(r+1) \times (m+r)} \right\|$$

are not all equal to zero.

For if $\Sigma_{k=1}^{k=m+r} a_{r+1,k} \lambda_k = \theta$, it is evident that by combining this equation with the equations (18.), we may express each of the determinants $\theta \times \left| \underset{a}{r \times (m+r)} \right|$ in succession as a linear function of the determinants of $\left\| \underset{a}{(r+1) \times (m+r)} \right\|$. If, therefore, the former determinants do not all vanish, neither can the latter.

Let, then, $a_{m,1}, a_{m,2}, \ldots a_{m,n+m}$ represent any particular solution (other, of course, than that in which every indeterminate is equal to zero) of the system (16.); and let $A_{n+1,1}, A_{n+1,2}, \ldots A_{n+1,n+m}$ be integral numbers satisfying the inequality

$$\Sigma_{k=1}^{k=n+m} A_{n+1,k} a_{m,k} \gtrless 0; \quad \ldots \ldots \ldots \quad (20.)$$

the system

$$\left.\begin{matrix} A_{i,1} x_1 + A_{i,2} x_2 + \ldots + A_{i,n+m} x_{n+m} = 0 \\ i = 1, 2, 3, \ldots n+1 \end{matrix}\right\} \quad \ldots \ldots \quad (21.)$$

(which is obtained by the addition of a single equation to the system (16.)) is itself an independent system, as appears from the principle just enunciated; its index of indeterminateness is therefore $m-1$. Let $\left\| \underset{a}{(m-1) \times (n+m)} \right\|$ represent a complete set of independent solutions of (21.); it may then be inferred, from a second application of the same principle, that $\left\| \underset{a}{m \times (n+m)} \right\|$ represents a complete set of independent solutions of the proposed system (16.). Thus the determination of a complete set of independent solutions of a system of which the index of indeterminateness is m, depends on the determination of a similar set of solutions for a system of which the index is lower by a unit. By successive reductions, therefore, we shall at last arrive at a system of which the index of indeterminateness is unity, the complete solution of which is of course immediately found by evaluating the determinants of its matrix.

The practical application of this method supposes only that we can always assign a *particular* solution of a system of the form (16.) or (21.). And this, it may be observed,

can always be done, either by trial, or by other obvious and not unsymmetrical expedients.

Art. 5. If $\|a\|$ represent the matrix of a complete set of independent solutions of the proposed system (16.), and $\|b\|$ be any matrix of the same type as $\|a\|$, and connected with $\|a\|$ by the equation

$$\|b\| = \|k\| \times \|a\|, \quad \ldots \ldots \ldots \ldots \quad (22.)$$

in which $\|k\|$ denotes a square matrix of which the determinant is not zero, it is evident that the constituents of $\|b\|$ are also a complete set of independent solutions. And, conversely, if $\|b\|$ be the matrix of a complete set of independent solutions, $\|a\|$ is also the matrix of a similar set. For if $\|\mathrm{K}\|$ be the matrix composed of the first minors of $\|k\|$, so that

$$\|\mathrm{K}\| \times \|k\| = \|k,\ k,\ k, \ldots .\|,$$

we have from (22.),

$$\|\mathrm{K}\| \times \|b\| = \|k,\ k, \ldots k,\ k\| \times \|a\|;$$

from which it appears that $\|k,\ k, \ldots\| \times \|a\|$, and therefore $\|a\|$ itself, is the matrix of an independent set of solutions.

This observation enables us to obtain a complete set of relatively prime solutions, as soon as we have obtained an independent set. If $\|b\|$ be the matrix of the independent set, we have only to determine, by the method of art. 3, a square matrix $\|k\|$, and an oblong prime matrix $\|a\|$, satisfying the equation

$$\|b\| = \|k\| \times \|a\|;$$

the constituents of $\|a\|$ are then the terms of a set of fundamental solutions.

Or again, if in art. 4 we employ, instead of the inequality (19.), the equation

$$\Sigma_{k=1}^{k=n+m} a_{r+1,k} \lambda_k = 1, \quad \ldots \ldots \ldots \ldots \quad (23.)$$

it is easily shown that if $\left\| \underset{a}{r \times (n+m)} \right\|$ be a prime matrix, $\left\| \underset{a}{(r+1) \times (n+m)} \right\|$ is also a prime matrix; so that, by following the method of that article, we may obtain directly a set of fundamental solutions of any proposed system. Only, it will be observed, that in this mode of obtaining such a set, we suppose that we can assign particular solutions, not only of systems of the form (16.), but also of equations of the form (23.).

Art. 6. The importance of fundamental sets of solutions in the theory of linear indeterminate equations is evident from the following proposition:—

"If $\|a\|$ represent a set of fundamental solutions of the system (16.), the complete solution of that system is contained in the formula

$$\left.\begin{array}{l} x_i = \Sigma_{k=1}^{k=m} \xi_k a_{k,i} \\ i = 1,\ 2,\ 3, \ldots n+m \end{array}\right\}, \quad \ldots \ldots \ldots \quad (24.)$$

in which $\xi_1, \xi_2, \ldots \xi_m$ are absolutely indeterminate integral numbers."

For it is evident that every set of numbers included in (24.) satisfies (16.); and, conversely, if $a_{m+1,1}, a_{m+1,2}, \ldots a_{m+1,n+m}$ be any solution of (16.), the determinants of the

matrix $\left\|\underset{a}{}^{(m+1)\times(n+m)}\right\|$ are all zero, while the matrix $\left\|\underset{a}{}^{m\times(n+m)}\right\|$ is prime; whence, by a principle employed in art. 2, the system

$$a_{m+1,\,i}=\Sigma_{k=1}^{k=m}\xi_k a_{k,\,i}$$
$$i=1,\ 2,\ 3,\ \ldots\ n+m$$

is satisfied by one, and only one system of integral values of $\xi_1, \xi_2, \ldots \xi_m$; or, which is the same thing, the numbers $a_{m+1,\,1}$, $a_{m+1,\,2}$, $\cdots$ $a_{m+1,\,n+m}$ are included in the formula (24.).

It may be added, that no fractional values of $\xi_1, \xi_2, \ldots \xi_m$ can give integral values to $x_1, x_2, \ldots x_{n+m}$; and that the same values of $x_1, x_2, x_3, \ldots x_{n+m}$ cannot arise from different values of $\xi_1, \xi_2, \ldots \xi_m$.

The converse of the proposition just established is also true; *i. e.*

"If the formula

$$x_i=\Sigma_{k=1}^{k=m}\xi_k a_{k,\,i} \quad \ldots\ldots\ldots\ldots \quad (24.)$$

represent every solution of an indeterminate system of equations, the matrix $\|a\|$ is a prime matrix."

For if $\|b\|$ represent a set of fundamental solutions of the indeterminate system, we may express the constituents of $\|b\|$ as linear functions of the constituents of $\|a\|$, by means of the equations (24.), so as to obtain an equation of the form

$$\|b\|=|\xi|\times\|a\|,$$

$|\xi|$ denoting a square matrix; whence it immediately appears that $\|a\|$ is a prime matrix, and $|\xi|$ a unit-matrix.

Thus, if we apply EULER'S method for the resolution of indeterminate equations to the system (16.), we obtain, as the final result of the process, a system of equations of the form (24.); and as it is demonstrable, from the nature of the method itself, that these final equations contain the complete solution of the proposed system, their matrix is a prime matrix.

If $\|a\|$ and $\|b\|$ be any two sets of fundamental solutions of the same system, we shall have the equation

$$\|b\|=|\xi|\times\|a\|,$$

$|\xi|$ denoting a unit-matrix. The matrices, therefore, of all sets of fundamental solutions are deducible, by premultiplication with unit-matrices, from the matrix of any given set of such solutions.

Art. 7. If $\|a\|$ and $\|b\|$ represent two complete sets of independent solutions of the same system, the determinants of $\|a\|$ and $\|b\|$ are evidently connected by the relation $\beta\times|a|=\pm\alpha\times|b|$, α and β denoting the greatest divisors of $\|a\|$ and $\|b\|$ respectively. A similar relation subsists between the matrix of the system and the matrix of any complete set of independent solutions of it.

Let $\|A\|$ and $\|a\|$ represent those matrices respectively, K and k their greatest divisors;

the relation in question is expressed by the formula

$$k\times|\mathrm{A}|=\mathrm{K}\times|a|, \qquad (25.)$$

where it is to be remembered that the types of the matrices $\|\mathrm{A}\|$ and $\|a\|$ are complementary; so that, as has been already observed (see art. 1), there is an ambiguity of sign in the equation (25.).

To obtain its demonstration, let Q and q denote the sums of the squares of the determinants of $\|\mathrm{A}\|$ and $\|a\|$ respectively, and consider the determinant $\left|\begin{matrix}\mathrm{A}\\ a\end{matrix}\right|$. This determinant is certainly not zero, for multiplying it by itself, we find

$$\left|\begin{matrix}\mathrm{A}\\ a\end{matrix}\right|^2=\mathrm{Q}\times q. \qquad (26.)$$

Let, then, $\left|\begin{matrix}\mathrm{A}\\ a\end{matrix}\right|$ be multiplied by any determinant of $\|\mathrm{A}\|$; for example, by $\Sigma\pm\mathrm{A}_{1,1}, \mathrm{A}_{2,2}, \ldots \mathrm{A}_{n,n}$. Observing that $\Sigma\pm\mathrm{A}_{1,1}, \mathrm{A}_{2,2}, \ldots \mathrm{A}_{n,n}$ may assume the form

$$\begin{vmatrix}
\mathrm{A}_{1,1}, & \mathrm{A}_{2,1}, & \ldots & \mathrm{A}_{n,1}, & 0, & 0, & \ldots\ldots & 0\\
\mathrm{A}_{1,2}, & \mathrm{A}_{2,2}, & \ldots & \mathrm{A}_{n,2}, & 0, & 0, & \ldots\ldots & 0\\
\ldots & \ldots & \ldots & \ldots & \ldots & \ldots & \ldots & \ldots\\
\mathrm{A}_{1,n}, & \mathrm{A}_{2,n}, & \ldots & \mathrm{A}_{n,n}, & 0, & 0, & \ldots\ldots & 0\\
\mathrm{A}_{1,n+1}, & \mathrm{A}_{2,n+1}, & \ldots & \mathrm{A}_{n,n+1}, & 1, & 0, & \ldots\ldots & 0\\
\mathrm{A}_{1,n+2}, & \mathrm{A}_{2,n+2}, & \ldots & \mathrm{A}_{n,n+2}, & 0, & 1, & \ldots\ldots & 0\\
\ldots & \ldots & \ldots & \ldots & \ldots & \ldots & \ldots & \ldots\\
\mathrm{A}_{1,n+m}, & \mathrm{A}_{2,n+m}, & \ldots & \mathrm{A}_{n,n+m}, & 0, & 0, & \ldots\ldots & 1
\end{vmatrix}$$

we obtain the equation

$$\left|\begin{matrix}\mathrm{A}\\ a\end{matrix}\right|\times\Sigma\pm\mathrm{A}_{1,1}, \mathrm{A}_{2,2}, \ldots \mathrm{A}_{n,n}=\mathrm{Q}\times\Sigma\pm a_{1,n+1}, a_{2,n+2}, \ldots a_{m,n+m}, \qquad (27.)$$

in which we may permute the second set of indices in any manner consistent with the condition that $\left|\begin{matrix}\mathrm{A}\\ a\end{matrix}\right|$ should not change its sign; so that we may write

$$\left|\begin{matrix}\mathrm{A}\\ a\end{matrix}\right|\times|\mathrm{A}|=\mathrm{Q}\times|a|, \qquad (28.)$$

the correspondence of the determinants in $|\mathrm{A}|$ and $|a|$ being fixed by the matrix $\left\|\begin{matrix}\mathrm{A}\\ a\end{matrix}\right\|$. The equation (25.) is an immediate consequence of this result; and if in that equation we suppose the correspondence of the determinants to be still fixed by the matrix $\left\|\begin{matrix}\mathrm{A}\\ a\end{matrix}\right\|$, we shall have to write

$$k\times|\mathrm{A}|= \mathrm{K}\times|a|,$$

or

$$k\times|\mathrm{A}|=-\mathrm{K}\times|a|,$$

according as $\left|\begin{matrix}\mathrm{A}\\ a\end{matrix}\right|$ is a positive or negative number.

Art. 8. From the preceding principles we may deduce the solution of the following problem, which admits of important applications in other parts of arithmetic:—

"To find all the matrices of a given type, of which the determinants have given values, not all equal to zero."

Two particular cases of this problem (those in which the matrix is of the type 2×3 and 2×4) occur in the 'Disquisitiones Arithmeticæ' (see arts. 279 and 236). In both places GAUSS has suppressed the analysis of the problem, and has only given a synthetical demonstration that its conditions are satisfied by the solution he assigns. This, indeed, in art. 279, he expressly observes. He has also suppressed his method of deducing the complete solution from any particular solution,—an omission, however, which may probably be supplied by a comparison of art. 234 with art. 213, I. The very general and most important case, of a matrix of the type $n\times(n+1)$, has been subsequently treated of by M. HERMITE*.

Let $\|x\|$ denote a matrix of the type $n\times(n+m)$, of which the constituents are absolutely indeterminate quantities; writing λ for $\frac{\Pi(n+m)}{\Pi n.\Pi m}$, we shall represent its determinants by $X_1, X_2, \ldots X_\lambda$. If $m>1$, these determinants are not all independent, but are connected by certain identities of the form

$$\Phi(X_1, X_2, \ldots X_\lambda)=0, \quad \ldots\ldots\ldots \quad (29.)$$

Φ denoting a rational and integral homogeneous function with numerical coefficients. If, therefore, $C_1, C_2, \ldots C_\lambda$ be a given set of integral numbers, which can be represented as the determinants of a matrix of the type $n\times(n+m)$, these numbers will satisfy every relation of the form (29.); so that the identity

$$\Phi(X_1, X_2, \ldots X_\lambda)=0$$

will involve also the numerical equation

$$\Phi(C_1, C_2, \ldots C_\lambda)=0. \quad \ldots\ldots\ldots \quad (30.)$$

To obtain a convenient notation for $C_1, C_2, \ldots C_\lambda$, let us imagine that we have formed a square matrix of the type $(n+m)\times(n+m)$ by the addition of m horizontal rows to the matrix $\|x\|$; if, in the development of the determinant of this matrix, the coefficient of X_i be the determinant

$$\left|x_{n+r,\,\mu_s}\right|, \quad \begin{matrix} r=1, 2, 3, \ldots m \\ s=1, 2, 3, \ldots m \end{matrix}$$

($\mu_1, \mu_2, \ldots \mu_m$ denoting m of the numbers $1, 2, \ldots n+m$), we may represent X_i and C_i by the symbols $[\mu_1, \mu_2, \ldots \mu_m]$ and $(\mu_1, \mu_2, \ldots \mu_m)$ respectively; observing, however, that if two of the numbers $\mu_1, \mu_2, \ldots$ are equal, the value zero is to be attributed to each of these symbols.

If r denote one of the numbers $1, 2, 3, \ldots n$, the determinants of the matrix obtained

* CRELLE'S Journal, vol. xl. p. 264; see also EISENSTEIN, ibid. vol. xxviii. p. 327.

by adding the horizontal row

$$x_{r,1},\ x_{r,2},\ \dots\ x_{r,n+m}$$

to the matrix $\left\| \begin{matrix} n\times(n+m) \\ x \end{matrix} \right\|$, are identically equal to zero. We thus obtain $\lambda\times\frac{m}{n+1}$ equations of the form

$$\Sigma_{i=1}^{i=m+n}[i, \mu_1, \mu_2, \dots \mu_{m-1}]x_{r,i}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31.)$$

$\mu_1, \mu_2, \dots \mu_{m-1}$ representing any combination of $m-1$ of the numbers 1, 2, 3, ... $m+n$. In connexion with these equations, consider also the similarly formed system,

$$\Sigma_{i=1}^{i=m+n}(i, \mu_1, \mu_2, \dots \mu_{m-1})y_i=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32.)$$

This system, which is in appearance redundant (containing $\lambda\times\frac{m}{n+1}$ equations, and only $m+n$ indeterminates), is in reality defective, and is equivalent to m independent equations. For if $(k_1, k_2, \dots k_m)$ be one of the given numbers C which is not equal to zero, the partial system of m equations

$$\left.\begin{matrix} \Sigma_{i=1}^{i=m+n}(i, k_1, k_2, \dots k_{j-1}, k_{j+1}, \dots k_m)y_i=0 \\ j=1, 2, 3, \dots m \end{matrix}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (33.)$$

is certainly an independent system, because the determinant of the coefficients of $y_{k_1}, y_{k_2}, \dots y_{k_m}$ is $(k_1, k_2, \dots k_m)^n$, and is therefore different from zero. Again, every equation of (32.) which is not already comprised in (33.), may be obtained by linearly combining the equations of that partial system. To verify this assertion, let

$$\left.\begin{matrix} \Sigma[i, \mu_1, \mu_2, \dots \mu_{m-1}]x_{r,i}=0 \\ r=1, 2, 3, \dots n \end{matrix}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34.)$$

be the system of n equations obtained by attributing to r the n values of which it is susceptible in any one of the equations (31.). Eliminating from this system those $n-1$ determinants $[i, \mu_1, \mu_2, \dots \mu_{m-1}]$ in which i has a value *not* included in a set of $m+1$ numbers $\nu_1, \nu_2, \dots \nu_{m+1}$, arbitrarily selected from the series 1, 2, 3, ... $n+m$, we obtain a relation, which may be expressed in the form

$$\Sigma_{i=1}^{i=m+1}(-1)^i[\nu_i, \mu_1, \mu_2, \dots \mu_{m-1}]\times[\nu_1, \nu_2, \dots \nu_{i-1}, \nu_{i+1}, \dots \nu_{m+1}]=0, \quad . \quad . \quad (35.)$$

representing $\frac{mn\lambda^2}{(m+1)(n+1)}$ equations, since the sets

$$\mu_1, \mu_2, \dots \mu_{m-1},$$

$$\nu_1, \nu_2, \dots \nu_{m+1}$$

may respectively denote any sets of $m-1$ and $m+1$ numbers taken from the series 1, 2, ... $m+n$. Since (35.) is of the form $\Phi(X_1, X_2, \dots X_\lambda)=0$, we may at once infer the corresponding relation,

$$\Sigma_{i=1}^{i=m+1}(-1)^i(\nu_i, \mu_1, \mu_2, \dots \mu_{m-1})\times(\nu_1, \nu_2, \dots \nu_{i-1}, \nu_{i+1}, \dots \nu_{m+1})=0, \quad . \quad . \quad (36.)$$

by means of which any one of the equations (32.) may be deduced from the equations of the partial system (33.). Thus, if we multiply the equations of that system taken in

order, by the determinants $(-1)^i(k_i, h_1, h_2, \dots h_{m-1})$, and add the results, we obtain

$$(k_1, k_2, \dots k_m)\Sigma_{i=1}^{i=n+m}(i, h_1, h_2, \dots h_{m-1})y_i=0,$$

i. e. since $(k_1, k_2, \dots k_m)$ is not zero,

$$\Sigma_{i=1}^{i=n+m}(i, h_1, h_2, \dots \overset{*}{h}_{m-1})y_i=0.$$

The system (32.) is therefore equivalent to a system of m independent equations.

Let $\left\|\begin{matrix} m\times(m+n) \\ \gamma \end{matrix}\right\|$ represent the matrix of (33.), or of any other independent system equivalent to (32.) (the determinants of all such matrices are proportional); let $\Gamma_1, \Gamma_2, \dots \Gamma_\lambda$ be the determinants of $\|\gamma\|$; $\|\xi\|$ and $\Xi_1, \Xi_2, \dots \Xi_\lambda$ the matrix and determinants of the system similarly derived from (31.). By the theorem of art. 7, we have

$$\left|\begin{matrix} \xi \\ x \end{matrix}\right|\times|\xi|=\Sigma\Xi^s\times|x|, \quad \dots \dots \dots \dots \quad (37.)$$

or observing that $\left|\begin{matrix} \xi \\ x \end{matrix}\right|=\Sigma.\Xi X$, and that (37) is an identity of the form $\Phi=0$,

$$\Sigma.\Gamma C\times|\gamma|=\Sigma\Gamma^s\times|C|, \quad \dots \dots \dots \dots \quad (38.)$$

where $|C|$ symbolizes the numbers $C_1, C_2, \dots C_\lambda$, which correspond to the determinants of $|\gamma|$ in the same inverse order in which in equation (37.) the determinants of $|x|$ correspond to those of $|\xi|$. But if $\left\|\begin{matrix} n\times(m+n) \\ \theta \end{matrix}\right\|$ represent a system of fundamental solutions of (32.) or (33.), we have also

$$\left|\begin{matrix} \gamma \\ \theta \end{matrix}\right|\times|\gamma|=\Sigma\Gamma^s\times|\theta|, \quad \dots \dots \dots \dots \quad (39.)$$

whence, combining (38.) and (39.), and representing the greatest common divisor of $C_1, C_2, \dots C_\lambda$ by c, we find

$$c\times|\theta|=|C|. \quad \dots \dots \dots \dots \quad (40.)$$

If, then, $|c|$ denote any square matrix of determinant c, and of the type $n\times n$, the formula $|c|\times\|\theta\|$ contains the complete solution of the problem.

If γ represent the greatest divisor of $\|\gamma\|$, we infer from (38.)

$$c\times|\gamma|=\gamma\times|C|, \quad \dots \dots \dots \dots \quad (41.)$$

whence, if $\|\gamma'\|$ be a prime matrix of the type $m\times(m+n)$ satisfying the equation

$$|\gamma|=\gamma\times|\gamma'| \text{ (see art. 3)},$$

we find

$$|C|=c\times|\gamma'|. \quad \dots \dots \dots \dots \quad (42.)$$

The preceding analysis enables us therefore to obtain simultaneously the representation of the determinants $|C|$ as the determinants of two complementary matrices, of the types $n\times(m+n)$ and $m\times(m+n)$ respectively. We have thus two distinct methods of arriving at the solution of the problem, of which one requires the determination of a set of fundamental solutions of a system of linear equations; the other the reduction (by the method of art. 3) of a given matrix to a prime matrix. The greatest divisor of $\|\gamma\|$,

which we have represented by γ, is evidently $(k_1, k_2, \dots k_m)^{m-1} \times c$. If, therefore, C, one of the given numbers $C_1, C_2, \dots C_n$, be a unit, we have only to take C for $(k_1, k_2 \dots k_m)$, and we shall immediately obtain a matrix $\|\gamma\|$ of the type $m \times (m+n)$ satisfying the equation

$$|\gamma| = |C|.$$

And similarly might a matrix of the type $n \times (m+n)$, satisfying the same equation, be written down without calculation.

Art. 9. The importance of the case, in which $m=1$, is so great, that we may be allowed to point out the identity of the solution obtained by the preceding method with that already given by M. Hermite. Let, then, $C_1, C_2, \dots C_{n+1}$ represent the determinants of a matrix of the type $n \times (n+1)$ taken in their natural order (*i. e.* so taken that if the matrix be completed by an additional row of constituents,

$$c_1, c_2, \dots c_{n+1},$$

the value of its determinant would be

$$c_1C_1 + c_2C_2 + c_3C_3 + \dots c_{n+1}C_{n+1}).$$

We have then to obtain a set of fundamental solutions of the equation

$$C_1y_1 + C_2y_2 + C_3y_3 + \dots + C_{n+1}y_{n+1} = 0. \quad \dots \quad (43.)$$

Such a set may always be obtained by the following particular method. Supposing that C_1 is not zero, consider the equations

$$\left.\begin{aligned} 0 &= C_1y_1 + C_2y_2 \\ 0 &= C_1y_1 + C_2y_2 + C_3y_3 \\ &\dots\dots\dots\dots\dots \\ 0 &= C_1y_1 + C_2y_2 + C_3y_3 + \dots\dots + C_{n+1}y_{n+1} \end{aligned}\right\} \quad \dots\dots \quad (44.)$$

and take a particular solution of each of them, assigning to the last indeterminate in each, the least value (zero excepted) of which it is susceptible. If we denote by Δ_k the greatest common divisor of $C_1, C_2, \dots C_k$, so that $\Delta_1 = C_1$, $\Delta_{n+1} = c$, it is evident that the value of y_{k+1} in the equation

$$C_1y_1 + C_2y_2 + \dots + C_{k+1}y_{k+1} = 0$$

will be $\frac{\Delta^k}{\Delta_{k+1}}$; and if in the same equation we represent the values of $y_1, y_2, \dots y_k$ by

$$r_{k,1}, r_{k,2}, r_{k,3}, \dots r_{k,k},$$

the matrix

$$\left\| \begin{array}{llllll} r_{1,1}, \frac{\Delta_1}{\Delta_2}, & 0, & 0 & \dots & \dots & 0 \\ r_{2,1}, & r_{2,2}, \frac{\Delta_2}{\Delta_3}, & 0 & \dots & \dots & 0 \\ r_{3,1}, & r_{3,2}, & r_{3,3}, \frac{\Delta_3}{\Delta_4} & \dots & \dots & 0 \\ \dots & \dots & \dots & \dots & \dots & 0 \\ r_{n,1}, & r_{n,2}, & r_{n,3}, & r_{n,4} & \dots & \frac{\Delta_n}{\Delta_{n+1}} \end{array} \right\| \quad \dots\dots \quad (45.)$$

will represent a set of fundamental solutions of (43.). For, in the first place, it represents a set of independent solutions; because its first determinant is $\frac{\Delta_1}{\Delta_2}\times\frac{\Delta_2}{\Delta_3}\times\dots\frac{\Delta_n}{\Delta_{n+1}}$, or $\frac{\Delta_1}{\Delta_{n+1}}$, or $\frac{C_1}{c}$; therefore its determinants are proportional to $C_1, C_2, \dots$ &c.; or since the first of them is $\frac{C_1}{c}$, they are respectively equal to the numbers

$$\frac{C_1}{c}, \frac{C_2}{c}, \frac{C_3}{c}, \dots \frac{C_{n+1}}{c},$$

which admit of no common divisor.

To obtain a set of values for the constituents $r_{i,j}$, which occur in the matrix (45.), we may form the series of equations

$$\left.\begin{aligned} &\lambda_1 \quad C_2+\mu_1\Delta_1=\Delta_2\\ &\lambda_2 \quad C_3+\mu_2\Delta_2=\Delta_3\\ &\quad\cdots\cdots\cdots\\ &\lambda_{n-1}C_n+\mu_n\Delta_n=\Delta_{n+1}\end{aligned}\right\} \quad \dots\dots\dots\dots \quad (46.)$$

It will then be found that the equations (44.) are satisfied by the values of r comprised in the formula

$$r_{i,j}=-\lambda_{j-1}\mu_j\dots\mu_{i-1}\frac{C_{i+1}}{\Delta_{i+1}} \quad [j\leq i]; \quad \dots\dots\dots \quad (47.)$$

and on substituting these values in the matrix (45.), it will coincide, after an unimportant modification, with that occurring in M. HERMITE'S solution of the problem.

But, in practice, the simplest method of obtaining a solution of the problem considered in this article, is to solve the equation (43.) by EULER'S method, and to employ in the place of the matrix (45.), the matrix of the set of fundamental solutions thus obtained (see art. 6).

Art. 10. Another problem, closely connected with the preceding, and of no less frequent application, has also been completely solved by M. HERMITE*; but as it may serve to illustrate the utility of the methods employed in this paper, we shall venture to resume and generalize it here. The problem is

"Given a set of $n+1$ numbers $C_1, C_2, \dots C_{n+1}$ without any common divisor, to assign all the matrices $\|x\|$ of the type $n\times(n+1)$ which satisfy the equation

$$\left|\frac{C}{x}\right|=1."$$

Let $c_1, c_2, \dots c_{n+1}$ be any particular solution of the equation

$$C_1y_1+C_2y_2+\dots+C_{n+1}y_{n+1}=1 \quad \dots\dots\dots \quad (48.)$$

(which is always possible because $C_1, C_2, \dots C_{n+1}$ are relatively prime); and let $\|\gamma\|$ represent a set of fundamental solutions of the equation

$$c_1y_1+c_2y_2+\dots+c_{n+1}y_{n+1}=0. \quad \dots\dots\dots \quad (49.)$$

* LIOUVILLE, vol. xiv. p. 21.

Then, if $\|u\|$ represent any unit-matrix of the type $n \times n$, and $\lambda_1, \lambda_2, \ldots \lambda_n$ absolutely indeterminate integers, the complete solution of the problem is contained in the formula

$$\left.\begin{matrix} \|u\| \times \|\gamma_{i,j} + \lambda_i C_j\| \\ i = 1, 2, 3 \ldots n \\ j = 1, 2, 3 \ldots n+1 \end{matrix}\right\} \quad \ldots \ldots \ldots \ldots \quad (50.)$$

For if $\|x\|$ be any one of the matrices contained in that formula, it is readily seen that

$$\left|\begin{matrix} C \\ x \end{matrix}\right| = \left|\begin{matrix} C \\ \gamma \end{matrix}\right| = C_1 c_1 + C_2 c_2 + \ldots + C_{n+1} c_{n+1} = 1.$$

Conversely, if $\|x\|$ be a matrix satisfying the equation $\left|\begin{matrix} C \\ x \end{matrix}\right| = 1$, $\|x\|$ is included in the formula (50.). To show this, we observe that the complete solution of equation (48.) is contained in the formula

$$y_j = c_j + \Sigma_{i=1}^{i=n} \theta_{i,j} \lambda_i, \; j = 1, 2, \ldots n+1, \quad \ldots \ldots \ldots \quad (51.)$$

in which $\|\theta\|$ is *any* set of fundamental solutions of the equation

$$C_1 y_1 + C_2 y_2 + \ldots + C_{n+1} y_{n+1} = 0, \quad \ldots \ldots \ldots \quad (52.)$$

and $\lambda_1, \lambda_2, \ldots \lambda_n$ are indeterminate integers. The complete solution of the same equation (48.) is therefore supplied by the determinants of the matrix $\|\gamma_{i,j} + \lambda_i C_j\|$. For those determinants may be represented by the formula

$$c_j + \Sigma_{i=1}^{i=n} [i, j] \lambda_i, \; j = 1, 2, 3, \ldots n+1,$$

in which $[i, j]$ symbolizes a first minor of the determinant $\left|\begin{matrix} C \\ \gamma \end{matrix}\right|$, so that

$$[i, j] = \frac{d \left|\begin{matrix} C \\ \gamma \end{matrix}\right|}{d \gamma_{i,j}}.$$

But the numbers $[i, 1], [i, 2], \ldots [i, n+1]$ satisfy (52.) for every value of i; and, since $\left|\begin{matrix} C \\ \gamma \end{matrix}\right| = 1$, the determinants of the matrix

$$\left.\begin{matrix} \|[i, j]\| \\ i = 1, 2, 3, \ldots n \\ j = 1, 2, 3, \ldots n+1 \end{matrix}\right\} \quad \ldots \ldots \ldots \quad (53.)$$

are the numbers $C_1, C_2, \ldots C_{n+1}$, and are therefore relatively prime. It follows from this that (53.) represents a set of fundamental solutions of (52.); *i. e.* that the complete solution of (48.) is represented by the determinants of $\|\gamma_{i,j} + \lambda_i C_j\|$. If then $\|x\|$ be a matrix satisfying the equation $\left|\begin{matrix} C \\ x \end{matrix}\right| = 1$, since the determinants of $\|x\|$ evidently satisfy (48.), values can be assigned to $\lambda_1, \lambda_2, \ldots \lambda_n$ which shall verify the equation

$$|\gamma_{i,j} + \lambda_i C_j| = |x|,$$

whence it follows that

$$\|x\| = \|u\| \times \|\gamma_{i,j} + \lambda_i C_j\|,$$

$\|u\|$ denoting a unit-matrix, *i. e.* $\|x\|$ is one of the matrices included in the formula (50.).

The result incidentally obtained in the foregoing analysis, that the complete solution

of an equation of the form

$$C_1x_1+C_2x_2+\ldots\ldots+C_{n+1}x_{n+1}=1$$

can be exhibited in the determinantal form (50.), is occasionally useful.

The preceding problem is a particular case of the following more general enunciation:—

"Given a prime matrix $\|C\|$ of the type $m\times(m+n)$, to find all the matrices $\|x\|$ of the type $n\times(m+n)$ which satisfy the equation

$$\left|\begin{matrix}C\\x\end{matrix}\right|=1." \qquad \ldots\ldots\ldots\ldots \quad (54.)$$

Let $\|\gamma\|$ be a matrix which satisfies (54.), let the numbers $\mu_{i,j}$ represent absolute indeterminates, and $\|u\|$ any unit-matrix; the complete solution of the problem is contained in the formula

$$\|x\|=\|u\|\times\|\gamma+\Sigma\mu C\|, \qquad \ldots\ldots\ldots\ldots \quad (55.)$$

where $\|\gamma+\Sigma\mu C\|$ represents the matrix,

$$\left\|\gamma_{i,j}+\Sigma_{\theta=1}^{\theta=m}\mu_{i,\theta}C_{\theta,j}\right\| \qquad \begin{matrix}i=1,\ 2,\ 3,\ \ldots\ n\\ j=1,\ 2,\ 3,\ \ldots\ n+m.\end{matrix}$$

For if $\|x\|$ be a matrix satisfying the equation (54.), we have

$$\left|\begin{matrix}C\\x\end{matrix}\right|=1=\left|\begin{matrix}C\\\gamma\end{matrix}\right|;$$

and consequently

$$\left\|\begin{matrix}C\\x\end{matrix}\right\|=\|v\|\times\left\|\begin{matrix}C\\\gamma\end{matrix}\right\|,$$

$\|v\|$ denoting a unit of the type $(m+n)\times(m+n)$. But because the first m horizontal rows in $\left\|\begin{matrix}C\\x\end{matrix}\right\|$ and $\left\|\begin{matrix}C\\\gamma\end{matrix}\right\|$ are identical, it is evident that

$$v_{i,j}=0,\ \begin{matrix}i=1,\ 2,\ 3,\ \ldots\ m\\ j=1,\ 2,\ 3,\ \ldots\ m+n,\end{matrix}$$

except when $i=j$, in which case

$$v_{1,1}=v_{2,2}=\ldots v_{m,m}=1.$$

The unit-matrix $\|v\|$ therefore arises from the composition of two unit-matrices of the forms

$$\left\|\begin{matrix}1&,0&,0&,\ldots0&,0,0,\ldots0\\0&,1&,0&,\ldots0&,0,0,\ldots0\\0&,0&,1&,\ldots0&,0,0,\ldots0\\\cdot&\cdot&\cdot&\cdot&\cdot\\\lambda_{1,1},&\lambda_{1,2},&\lambda_{1,3},&\ldots\lambda_{1,m},&1,0,\ldots0\\\lambda_{2,1},&\lambda_{2,2},&\lambda_{2,3},&\ldots\lambda_{2,m},&0,1,\ldots0\\\cdot&\cdot&\cdot&\cdot&\cdot\\\lambda_{n,1},&\lambda_{n,2},&\lambda_{n,3},&\ldots\lambda_{n,m},&0,0,\ldots1\end{matrix}\right\|$$

and
$$\left\|\begin{array}{llll} 1, & 0, & 0, \ldots 0 & , 0 \quad , \ldots 0 \\ 0, & 1, & 0, \ldots 0 & , 0 \quad , \ldots 0 \\ 0, & 0, & 1, \ldots 0 & , 0 \quad , \ldots 0 \\ \cdot & \cdot & \cdot \quad \cdot \quad \cdot & \cdot \quad \cdot \quad \cdot \\ 0, & 0, & 0, \ldots u_{1,1}, & u_{1,2}, \ldots u_{1,n} \\ 0, & 0, & 0, \ldots u_{2,1}, & u_{2,2}, \ldots u_{2,n} \\ \cdot & \cdot & \cdot \quad \cdot \quad \cdot & \cdot \quad \cdot \quad \cdot \\ 0, & 0, & 0, \ldots u_{n,1}, & u_{n,2}, \ldots u_{n,n} \end{array}\right\|$$

$\|\lambda\|$ denoting a matrix of the type $n \times m$ of which the constituents may be any numbers whatever, and $\|u\|$ a unit-matrix of the type $n \times n$. If for $\|\lambda\|$ we substitute the matrix

$$\|\mu\| = \|u\|^{-1} \times \|\lambda\|,$$

it is readily seen that we may invert the order of the factors in the expression of $\|v\|$; so that, using an abbreviated notation, the signification of which is evident, we may write either

$$\|v\| = \left\|\begin{matrix} 1, & 0 \\ \lambda, & 1 \end{matrix}\right\| \times \left\|\begin{matrix} 1, & 0 \\ 0, & u \end{matrix}\right\|,$$

or

$$\|v\| = \left\|\begin{matrix} 1, & 0, \\ 0, & u, \end{matrix}\right\| \times \left\|\begin{matrix} 1, & 0 \\ \mu, & 1 \end{matrix}\right\|.$$

Substituting the latter expression of $\|v\|$ in the equation

$$\left\|\begin{matrix} C \\ x \end{matrix}\right\| = \|v\| \times \left\|\begin{matrix} C \\ \gamma \end{matrix}\right\|,$$

we immediately infer

$$\|x\| = \|u\| \times \|\gamma + \Sigma \mu C\|.$$

Every matrix satisfying the equation $\left|\begin{matrix} C \\ x \end{matrix}\right| = 1$ is therefore comprised in the formula (55.); and since it is evident, conversely, that every matrix comprised in (55.) satisfies the equation, that formula contains the complete solution of the question.

A particular solution of the problem (which may be taken for $\|\gamma\|$) can be obtained as follows:—Complete the matrix $\|C\|$ by any n horizontal rows of constituents which do not cause the determinant of the resulting matrix to vanish. From this matrix a prime (*i. e.* a unit) matrix of the same type is to be deduced by the method of art. 3, a reduction which can always be effected without changing the prime matrix $\|C\|$.

Art. 11. The consideration of sets of fundamental solutions of linear systems is also of use in the theory of indeterminate systems containing terms not affected by any indeterminate. Let

$$\left.\begin{array}{c} A_{i,0} + A_{i,1} x_1 + A_{i,2} x_2 + \ldots + A_{i,n+m} = 0 \\ i = 1, 2, 3, \ldots n \end{array}\right\} \quad \ldots\ldots\ldots \quad (56.)$$

represent such a system; its general solution will assume the form

$$\left.\begin{array}{c} x_k = a_k + \Sigma_{\theta=1}^{\theta=m} \mu_\theta \alpha_{k,\theta} \\ k = 1, 2, 3, \ldots n+m \end{array}\right\} \quad \ldots\ldots\ldots \quad (57.)$$

where $a_1, a_2, \dots a_{n+m}$ is a particular solution of (56.), $\mu_1, \mu_2, \dots \mu_m$ indeterminate numbers, and $\|\alpha\|$ a set of fundamental solutions of the system

$$\left.\begin{matrix} A_{i,1}x_1+A_{i,2}x_2+\dots+A_{i,n+m}x_{n+m}=0 \\ i=1, 2, 3, \dots n \end{matrix}\right\} \quad \dots\dots \quad (58.)$$

Whenever, therefore, the proposed system is resoluble, its complete solution involves m indeterminates; but in order that it should be resoluble, a certain condition must be satisfied by its coefficients. This condition is, "that the greatest divisors of its augmented and unaugmented matrices must be equal*." We shall call these divisors D and D_0 respectively, representing the matrices themselves by $\|A\|$ and $\|A_0\|$. That the condition is necessary may be seen by eliminating in turn every combination of $n-1$ indeterminates from (56). We thus find that every determinant of $\|A\|$ is divisible by D_0, *i. e.* that D is divisible by D_0; but evidently D divides D_0, so that $D_0=D$. To show that the condition is sufficient, as well as necessary, consider the system

$$\left.\begin{matrix} A_{i,0}x_0+A_{i,1}x_1+A_{i,2}x_2+\dots+A_{i,n+m}x_{n+m}=0 \\ i=1, 2, 3, \dots n \end{matrix}\right\}, \quad \dots\dots \quad (59.)$$

and let $\left\|\begin{matrix}(m+1)\times(n+m+1) \\ \theta\end{matrix}\right\|$ represent a set of its fundamental solutions. To say that (56.) is resoluble, is the same thing as to say that (59.) admits of solutions in which the value of x_0 is unity; and (59.) will not, or will admit of such solutions according as $\theta_{1,0}, \theta_{2,0}, \dots \theta_{m,0}$ do, or do not admit of any common divisor beside unity. But, by the theorem of art. 7, those determinants of $\|\theta\|$ into which the column $\theta_{1,0}, \theta_{2,0} \dots$ enters, are equal to the determinants of $\|A_0\|$, taken in a proper order and divided by D. If $D=D_0$, the determinants of $\|A_0\|$, divided by D, are relatively prime, and consequently those determinants of $\|\theta\|$ which contain $\theta_{1,0}, \theta_{2,0}, \dots \theta_{m,0}$ are also relatively prime; a conclusion which implies that $\theta_{1,0}, \theta_{2,0}, \dots \theta_{m,0}$ are themselves relatively prime, *i. e.* that the system (56.) is resoluble.

This criterion is not immediately applicable if the system (56.) be not independent, *i. e.* if the determinants of its augmented matrix $\|A\|$ be all equal to zero. But it may

* [This Theorem has already been given by M. Ignaz Heger (Memoirs of the Vienna Academy, vol. xiv. second part, p. 111). I regret that in the abstract of the present paper, which has been inserted in the 'Proceedings of the Royal Society,' no reference was made to M. Heger's Memoir, with the contents of which I was unacquainted, at the time at which that abstract was prepared. M. Heger's demonstration (adapted to the terminology here employed) is, in the main, as follows. (1) If the unaugmented matrix of an indeterminate system be prime, the system is always resoluble. For every determinate system, of which the matrix is a unit-matrix, is resoluble in integral numbers; and we may suppose the given indeterminate system to form part of such a determinate system (see art. 10, *supra*). (2) The equation $\|A\|=\|D\|\times\|A'\|$, in which $\|D\|$ is a square matrix, having D for its determinant, and $\|A'\|$ a prime matrix of the same type as $\|A\|$, is always resoluble (see art. 3). We can therefore replace the given system (56.) by a system of which the augmented matrix is $\|A'\|$, and which is resoluble or irresoluble at the same time with the given system. But if $D_0=D$, the unaugmented matrix of this derived system is prime; *i. e.* if $D_0=D$, the proposed system is resoluble. (3) That the condition is necessary as well as sufficient may be proved as in the text.—Sept. 1861, H. J. S. S.]

be applied to any independent system, equivalent to the proposed system, and deduced linearly from it.

If we represent by D_k the greatest divisor of the matrix, deduced from the matrix of (59.) by omitting from it the column $A_{1,k}, A_{2,k}, \dots A_{n,k}$, we may enunciate the following proposition:—

"In every solution of the system (59.), the value of x_k is divisible by $\frac{D_k}{D}$; and, conversely, a solution of that system can always be assigned in which x_k shall have any given value divisible by $\frac{D_k}{D}$."

It will be seen that the solution of (56.) depends, first, on the solution of (59.), and, secondly, on that of the indeterminate equation

$$\theta_{1,0}x_1+\theta_{2,0}x_2+\dots+\theta_{m+1,0}x_{m+1}=1. \quad \dots\dots \quad (60.)$$

If we represent the values of the indeterminates in this equation as the determinants of the matrix

$$\|\gamma_{i,j}+\mu_i\theta_{j,0}\| \qquad \begin{matrix} i=1, 2, 3, \dots m \\ j=1, 2, \dots m+1 \end{matrix}$$

(see art. 10), we may express the most general values of the indeterminates which satisfy (56.) in the determinantal form

$$x_k=\begin{vmatrix} \theta_{1,k} & \theta_{2,k} & \dots & \theta_{m+1,k} \\ \gamma_{1,1}+\mu_1\theta_{1,0} & \gamma_{1,2}+\mu_1\theta_{2,0} & \dots & \gamma_{1,m+1}+\mu_1\theta_{m+1,0} \\ \gamma_{2,1}+\mu_2\theta_{1,0} & \gamma_{2,2}+\mu_2\theta_{2,0} & \dots & \gamma_{2,m+1}+\mu_2\theta_{m+1,0} \\ \dots & \dots & \dots & \dots \\ \gamma_{m,1}+\mu_m\theta_{1,0} & \gamma_{m,2}+\mu_m\theta_{2,0} & \dots & \gamma_{m,m+1}+\mu_m\theta_{m+1,0} \end{vmatrix} \quad (61.)$$

Art. 12. We shall now indicate an important transformation of which any square matrix of integral numbers is susceptible. We begin with the following theorem:—

"If a given rectangular matrix be premultiplied by a unit matrix, the greatest common divisor of any vertical column of minor determinants is the same in the resulting as in the given matrix."

For it is evident that any minor, either in the given or in the resulting matrix, is an integral and linear function of the minors formed from the same vertical columns in the other matrix.

Similarly, it may be shown that

"When a square matrix is post-multiplied by any prime rectangular matrix, the greatest common divisor of any horizontal row of minors is the same in the resulting rectangular matrix as in the given square matrix."

For if

$$\left\|\underset{A}{n\times(n+m)}\right\|=\left\|\underset{B}{n\times n}\right\|\times\left\|\underset{C}{n\times(n+m)}\right\|,$$

where $\|C\|$ is a prime matrix, it is clear that every minor of $\|A\|$ is a linear function of the minors formed from the same horizontal rows of $\|B\|$; so that if a and b be the greatest common divisors of any corresponding horizontal rows of minors in those two matrices, a is divisible by b. But again, if θ be any one of the determinants of $\|C\|$, and s be the order of the minors under consideration, any minor of $\|B\|$, after multiplication by θ^s, may be expressed as a linear function of a certain group of the minors taken from the same horizontal rows of $\|A\|$. Consequently $\theta^s \times b$ is divisible by a; or, since θ may have any one of a series of values which are relatively prime, b is divisible by a, i. e. $b=a$.

By combining these results we obtain the theorem.

"If $\nabla_n, \nabla_{n-1}, \nabla_{n-2}, \dots \nabla_1$ represent the greatest common divisors of all the minors of order n, $n-1, \dots 1$, respectively which can be formed out of a given square matrix, these numbers will remain unchanged, when the given matrix is premultiplied by any unit-matrix, and post-multiplied by any prime matrix whatsoever."

Art. 13. Let θ, the determinant of the square matrix $\left\|\begin{matrix} n \times n \\ a \end{matrix}\right\|$, be a positive number, different from zero. It may be shown that by post-multiplication with a properly assumed unit $\|\alpha\|$, the matrix $\|a\|$ can be reduced to the form

$$\left\| \begin{matrix} \mu_1, r_{1,2}, r_{1,3} \dots . \dots r_{1,n} \\ 0\ , \mu_2\ , r_{2,3} \dots . \dots r_{2,n} \\ 0\ , 0\ \ , \mu_3\ \ \dots . \dots r_{3,n} \\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot \\ 0\ , 0\ \ , 0\ \ \dots . \dots \mu_n \end{matrix} \right\| . \qquad (62.)$$

where $\mu_1, \mu_2, \dots \mu_n$ are positive numbers, such that $\mu_1 \times \mu_2 \times \dots \times \mu_n = \theta$, and the constituents $r_{i,k}$ satisfy the inequalities

$$0 \leqq r_{i,k} < \mu_i . \qquad (63.)$$

This was first observed by GAUSS for the case $n=2$; by SEEBER for $n=3$; and the general theorem has been enunciated by M. HERMITE*. Its precise statement is

"Every matrix of the type $n \times n$ is equivalent (by post-multiplication) to one, and only one, of the *reduced* matrices included in the formula (62.)."

To show this, let $v_{1,1}, v_{2,1}, \dots v_{n,1}$ be the integral and relatively prime numbers which satisfy the equations

$$\left.\begin{matrix} a_{i,1}\, v_{1,1} + a_{i,2}\, v_{2,1} + \dots a_{i,n}\, v_{n,1} = 0 \\ i = 2, 3, \dots n \end{matrix}\right\} , \qquad (64.)$$

and the inequality

$$a_{1,1}\, v_{1,1} + a_{1,2}\, v_{2,1} + \dots a_{1,n}\, v_{n,1} > 0.$$

Then it is evident that, if $\|v\|$ be a unit-matrix of which $v_{1,1}, v_{2,1}, \dots v_{n,1}$ form the first column, the matrix $\|a\| \times \|v\|$ will assume the form

* GAUSS, Disq. Arith. art. 213; SEEBER, "Untersuchungen ueber die Eigenschaften der positiven ternären quadratischen Formen" (Mannheim, 1831), art. 31; M. HERMITE, CRELLE, vol. xli. p. 192.

$$\left\| \begin{matrix} \mu_1, & b_{1,2}, & b_{1,3}, \dots b_{1,n} \\ 0, & b_{2,2}, & b_{2,3}, \dots b_{2,n} \\ 0, & b_{3,2}, & b_{3,3}, \dots b_{3,n} \\ \cdot & \cdot & \cdot \quad \cdot \quad \cdot \\ 0, & b_{n,2}, & b_{n,3}, \dots b_{n,n} \end{matrix} \right\|, \qquad \dots\dots\dots (65.)$$

where $\mu_1 = a_{1,1}, v_{1,1} + a_{1,2}, v_{2,1} + \dots a_{1,n}, v_{n,1}$.

If this matrix be post-multiplied by the unit,

$$\left\| \begin{matrix} 1, & k_2, & k_3, \dots k_n \\ 0, & 1, & 0, \dots 0 \\ 0, & 0, & 1, \dots 0 \\ \cdot & \cdot & \cdot \quad \cdot \quad \cdot \\ 0, & 0, & 0, \dots 1 \end{matrix} \right\|$$

the constituents $b_{1,i}$ will be changed into $b_{1,i} + \mu_1 k_i$, while all the other constituents will remain unaltered; so that by assigning proper values to the numbers $k_2 \dots k_n$, we may bring the given matrix $\|a\|$ into the form

$$\left\| \begin{matrix} \mu_1, & r_{1,2}, & r_{1,3}, \dots r_{1,n} \\ 0, & b_{2,2}, & b_{2,3}, \dots b_{2,n} \\ 0, & b_{3,2}, & b_{3,3}, \dots b_{3,n} \\ \cdot & \cdot & \cdot \quad \cdot \quad \cdot \\ 0, & b_{n,2}, & b_{n,3}, \dots b_{n,n} \end{matrix} \right\|$$

where $r_{1,i}$ verifies the inequality

$$0 \leqq r_{1,i} < \mu_1.$$

From this it is easy to infer that if a matrix of the type $(n-1)\times(n-1)$ can be reduced to the form (62.), the same reduction is possible for a matrix of the type $n \times n$, i. e. since that reduction is possible when $n=1$, $n=2$, .. it is possible for every value of n.

To prove that $\|a\|$ is equivalent (by post-multiplication) to only one of the *reduced* matrices (62.), it is sufficient to show that no two reduced matrices can be equivalent. If $\|a\|$ and $\|a'\|$ be two reduced matrices, and $\|v\|$ a unit-matrix, such that $\|a\| \times \|v\| = \|a'\|$, it may be inferred, by comparing the corresponding constituents of the two matrices $\|a\| \times \|v\|$ and $\|a'\|$ (beginning with the lowest horizontal rows of each and proceeding upwards), that all the constituents of $\|v\|$ which lie below its principal diameter are zero; and consequently that the constituents of the principal diameter itself are all positive units. Further, that the constituents above the principal diameter of $\|v\|$ are likewise zero, may be established (for each line of constituents parallel to the diameter, beginning with that nearest to it) by means of the inequalities (63.) which are satisfied by the constituents both of $\|a\|$ and $\|a'\|$. It thus appears that two reduced matrices cannot be equivalent, without being identical. It will be observed that the reducing unit is unique;

i. e. that only one post-multiplying unit can be assigned by which a given matrix can be reduced to the form (62.).

If instead of reducing the given matrix $\|\alpha\|$ by post-multiplication we employ a pre-multiplying unit, we obtain the following theorem:—

"Every matrix of the type $n \times n$ and of determinant θ is equivalent (by pre-multiplication) to one, and only one of the matrices included in the formula (62.), in which $\mu_1, \mu_2 \ldots$ are positive, $\mu_1, \mu_2 \ldots \mu_n = \theta$, and $r_{i,k}$ satisfies the inequality

$$0 \leqq r_{i,k} < \mu_k." \quad \ldots \ldots \ldots \ldots \quad (66.)$$

Art. 14*. The transformation to which we have referred in art. 12 is obtained by employing simultaneously a pre-multiplying and a post-multiplying unit-matrix. It is expressed by the equation

$$\|\alpha\| = \|\alpha\| \times \left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \ldots \frac{\nabla_1}{\nabla_0} \right\| \times \|\beta\|, \quad \ldots \ldots \quad (67.)$$

in which $\|\alpha\|$ is a given square matrix of the type $n \times n$, $\|\alpha\|$ and $\|\beta\|$ are unit-matrices, and $\nabla_n, \nabla_{n-1}, \nabla_{n-2}, \ldots \nabla_1, \nabla_0$ are the determinant and greatest common divisors of the minor determinants of $\|\alpha\|$, so that, in particular, ∇_n is the determinant of $\|\alpha\|$, ∇_{n-1} the greatest common divisor of its minor determinants of order $n-1$, ∇_1 the greatest common divisor of its constituents, and $\nabla_0 = 1$. The units $\|\alpha\|$ and $\|\beta\|$ are not absolutely determined, but admit, when $n > 1$, of an infinite number of different values. If $n = 1$, it is evident that the formula (67.) is verified; for we have the identical equation $\|\alpha\| = \|1\| \times \left\| \frac{\nabla_1}{\nabla_0} \right\| \times \|1\|$. It is therefore sufficient to show that, if the transformation indicated in the formula can be effected for matrices of the type $(n-1) \times (n-1)$, it can also be effected for matrices of the type $n \times n$. The demonstration depends on an elementary principle, which it is worth while to enunciate separately.

"If

$$\left.\begin{aligned} U_i = A_{i,1} x_1 + A_{i,2} x_2 + \ldots + A_{i,n+m} x_{n+m}, \\ i = 1, 2, 3, \ldots n \end{aligned}\right\} \quad \ldots \ldots \quad (68.)$$

denote a system of n linear functions of $n+m$ indeterminates, $(m \geqq 0)$, and if the constituents of the matrix $\|A\|$ do not admit of any common divisor, it is always possible to assign integral values to $x_1, x_2, \ldots x_{n+m}$, which shall render $U_1, U_2, \ldots U_n$ relatively prime."

For, in the first place, we can obtain values for $U_1, U_2, \ldots U_n$, which shall not have any common divisor with a given number M. Let $p, q, r \ldots$ be the different prime divisors of M; one at least of the constituents of $\|A\|$, for example $A_{i,p}$ is prime to p. Attributing to x_j a value prime to p, and values divisible by p to the remaining indeterminates, we shall obtain for U_i a value which is certainly prime to p. Similarly, by subjecting the indeterminates to proper congruential conditions with respect to the modules $q, r, \ldots$, we can render one, at least, of the functions U prime to q, one prime to r, and

* [This article has been in great part rewritten since the paper was read. The demonstration is not essentially changed, but is presented in what seems to be a simpler form.—Sept. 1861, H. J. S. S.]

so on; *i. e.* since we can assign to the indeterminates values simultaneously satisfying all these congruential conditions, we can give to $U_1, U_2, \dots U_n$ values the greatest common divisor of which is prime to M. Let D_n be the greatest divisor of $\|A\|$, D_{n-1} the greatest common divisor of the first minors of $\|A\|$; and let $C_1, C_2, \dots C_n$ be a set of simultaneous values of $U_1, U_2, \dots U_n$, having a greatest common divisor c, which is prime to $\frac{D_n}{D_{n-1}}$. Since the equations

$$A_{i,1}x_1+A_{i,2}x_2+\dots+A_{i,n+m}x_{n+m}=C_i,$$
$$i=1, 2, 3, \dots n$$

are resoluble, it will follow from the condition of resolubility (see art. 11), that the determinants of its augmented matrix, and in particular those which contain the column $C_1, C_2, \dots C_n$, are divisible by D_n. Let $\theta\times c\times D_{n-1}$ be the greatest common divisor of these last determinants; then $\theta\times c\times D_{n-1}$ is divisible by D_n, *i. e.* θ is divisible by $\frac{D_n}{D_{n-1}}$. It appears from this, that the condition of resolubility is satisfied by the system

$$A_{i,1}x_1+A_{i,2}x_2+\dots+A_{i,n+m}x_{n+m}=\frac{C_i}{c},$$
$$i=1, 2, 3, \dots n,$$

that is to say, it is possible to obtain a simultaneous system of relatively prime values for $U_1, U_2, \dots U_n$.

To apply this principle to the transformation of the matrix $\|a\|$, let

$$[a_{i,j}]=\frac{1}{\nabla_{n-1}}\frac{d\nabla_n}{da_{i,j}}. \quad \dots\dots\dots\dots \quad (69.)$$

The constituents of the matrix $\|[a]\|$ do not admit of any common divisor; consequently, in the system

$$\left.\begin{array}{c}[a_{i,1}]b_{1,1}+[a_{2,i}]b_{2,1}+\dots+[a_{n,i}]b_{n,1}=u_{i,1},\\ i=1, 2, 3, \dots n\end{array}\right\}, \quad \dots\dots \quad (70.)$$

we can assign values to $b_{1,1}, b_{2,1}, \dots b_{n,1}$, which shall render $u_{1,1}, u_{2,1}, \dots u_{n,1}$ relatively prime. Let $\|u\|$ denote a unit-matrix of which the first column is $u_{1,1}, u_{2,1}, \dots u_{n,1}$; and $\|b\|$ a square matrix of which the first column is $b_{1,1}, b_{2,1}, \dots b_{n,1}$, and of which the remaining constituents are defined by the equations

$$\left.\begin{array}{c}b_{i,j}=a_{i,1}u_{1,j}+a_{i,2}u_{2,j}+\dots+a_{i,n}u_{n,j}\\ i=1, 2, 3, \dots n\\ j=\quad 2, 3, \dots n.\end{array}\right\} \quad \dots\dots\dots \quad (71.)$$

Observing that the systems (69.) and (70.) involve the inverse system,

$$\left.\begin{array}{c}a_{i,1}u_{1,1}+a_{i,2}u_{2,1}+\dots+a_{i,n}u_{n,1}=\frac{\nabla_n}{\nabla_{n-1}}b_{i,1},\\ i=1, 2, 3, \dots n,\end{array}\right\} \quad \dots\dots \quad (72.)$$

we infer that the matrices $\|u\|$ and $\|b\|$ verify the equation

$$\|a\|\times\|u\|=\|b\|\times\left\|\frac{\nabla_n}{\nabla_{n-1}}, 1, 1, \dots\right\|, \quad \dots\dots\dots \quad (73.)$$

in which $\left\|\frac{\nabla_n}{\nabla_{n-1}}, 1, 1, \ldots\right\|$ denotes a matrix of the type $n \times n$. It follows from (73.) that ∇_{n-1} is the determinant of $\|b\|$; let that matrix be reduced by premultiplication with a unit-matrix; and let

$$\|b\| = \|v\| \times \|\nabla_{n-1}\|, \quad \ldots \quad (74.)$$

where $\|v\|$ is the reducing unit, and $\|\nabla_{n-1}\|$ the reduced matrix,

$$\left\| \begin{matrix} \mu_1, & r_{1,2}, & r_{1,3} \ldots & r_{1,n} \\ 0, & \mu_2, & r_{2,3} \ldots & r_{2,n} \\ 0, & 0, & \mu_3 \ldots & r_{3,n} \\ \cdot & \cdot & \cdot & \cdot \\ 0, & 0, & 0 \ldots & \mu_n \end{matrix} \right\|, \quad \ldots \quad (75.)$$

so that (73.) assumes the form

$$\|a\| = \|v\| \times \|\nabla_{n-1}\| \times \left\|\frac{\nabla_n}{\nabla_{n-1}}, 1, 1, \ldots\right\| \times \|u\|^{-1}. \quad \ldots \quad (76.)$$

It may be proved that in (75.) $\mu_1 = 1$, $r_{1,2} = 0$, $r_{1,3} = 0 \ldots r_{1,n} = 0$. For since the matrix $\|\nabla_{n-1}\| \times \left\|\frac{\nabla_n}{\nabla_{n-1}}, 1, 1 \ldots\right\|$ is derived from $\|a\|$ by multiplication with unit-matrices, ∇_{n-1} is the greatest common divisor of the first minors of $\|\nabla_{n-1}\| \times \left\|\frac{\nabla_n}{\nabla_{n-1}}, 1, 1, \ldots\right\|$. Therefore ∇_{n-1} divides $\mu_2 \times \mu_3 \times \ldots \times \mu_n$, which is one of those minors; but also $\nabla_{n-1} = \mu_1 \times \mu_2 \ldots \times \mu_n$; *i. e.* $\mu_1 = 1$, $\mu_2 \times \mu_3 \times \ldots \times \mu_n = \nabla_{n-1}$, and the product $\|\nabla_{n-1}\| \times \left\|\frac{\nabla_n}{\nabla_{n-1}}, 1, 1, \ldots\right\|$ assumes the form

$$\left\| \begin{matrix} \frac{\nabla_n}{\nabla_{n-1}}, & r_{1,2}, & r_{1,3}, \ldots & r_{1,n} \\ 0 & , \mu_2, & r_{2,3}, \ldots & r_{2,n} \\ 0 & , 0 & , \mu_3, \ldots & r_{3,n} \\ \cdot & \cdot & \cdot & \cdot \\ 0 & , 0 & , 0, \ldots & \mu_n \end{matrix} \right\|$$

One of the minors of this matrix is $r_{1,2} \times \mu_3 \ldots \times \mu_n$, which cannot be divisible by ∇_{n-1} or $\mu_2 \times \mu_3 \times \ldots \times \mu_n$, unless $r_{1,2}$ is a multiple of μ_2; but $r_{1,2} < \mu_2$, because $\|\nabla_{n-1}\|$ is reduced, therefore $r_{1,2} = 0$. Similarly, it may successively be shown that $r_{1,3} = 0 \ldots r_{1,n} = 0$. Now if the matrix

$$\left\| \begin{matrix} \mu_2, & r_{2,3}, \ldots & r_{2,n} \\ 0, & \mu_3, \ldots & r_{3,n} \\ \cdot & \cdot & \cdot \\ 0, & 0, \ldots & \mu_n \end{matrix} \right\|, \quad \ldots \quad (77.)$$

which is of the type $(n-1) \times (n-1)$, be reduced to the form

$$\|v'\| \times \left\|\frac{\nabla_{n-1}}{\nabla_{n-2}}, \frac{\nabla_{n-2}}{\nabla_{n-3}}, \ldots \frac{\nabla_1}{\nabla_0}\right\| \times \|u'\|,$$

in which $\nabla_{n-2}, \nabla_{n-3}, \dots$ represent the greatest common divisors of the minors of (77.), we may replace $\|\nabla_{n-1}\|$ by the matrix

$$\|\nabla_{n-1}\| = \begin{Vmatrix} 1 & 0 \\ 0 & v \end{Vmatrix} \times \left\| 1, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \frac{\nabla_{n-2}}{\nabla_{n-3}}, \dots \frac{\nabla_1}{\nabla_0} \right\| \times \begin{Vmatrix} 1 & 0 \\ 0 & u \end{Vmatrix},$$

where $\begin{Vmatrix} 1 & 0 \\ 0 & v \end{Vmatrix}$ and $\begin{Vmatrix} 1 & 0 \\ 0 & u \end{Vmatrix}$ denote unit-matrices of the type $n \times n$, the forms of which are sufficiently indicated by the symbols themselves. Hence, observing that

$$\begin{Vmatrix} 1 & 0 \\ 0 & u' \end{Vmatrix} \times \left\| \frac{\nabla_n}{\nabla_{n-1}}, 1, 1, \dots \right\| = \left\| \frac{\nabla_n}{\nabla_{n-1}}, 1, 1, \dots \right\| \times \begin{Vmatrix} 1 & 0 \\ 0 & u' \end{Vmatrix},$$

and that

$$\left\| 1, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \frac{\nabla_{n-2}}{\nabla_{n-3}}, \dots \frac{\nabla_1}{\nabla_0} \right\| \times \left\| \frac{\nabla_n}{\nabla_{n-1}}, 1, 1, \dots \right\| = \left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \dots \frac{\nabla_1}{\nabla_0} \right\|,$$

we obtain, from (76.),

$$\|\alpha\| = \|v\| \times \begin{Vmatrix} 1 & 0 \\ 0 & v' \end{Vmatrix} \times \left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \dots \frac{\nabla_1}{\nabla_0} \right\| \times \begin{Vmatrix} 1 & 0 \\ 0 & u' \end{Vmatrix} \times \|u^{-1}\|,$$

or more simply,

$$\|\alpha\| = \|\omega\| \times \left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \dots \frac{\nabla_1}{\nabla_0} \right\| \times \|\beta\|.$$

It has, however, still to be shown that $\nabla_{n-2}, \nabla_{n-3}, \dots$ which have been defined with reference to the matrix (77.) are the greatest common divisors of the successive systems of minors of $\|a\|$. These greatest common divisors are the same for the given matrix $\|a\|$ and for the matrix $\left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \dots \frac{\nabla_1}{\nabla_0} \right\|$ which is derived from it by multiplication with unit-matrices; consequently ∇_{n-1} divides every first minor of $\left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \dots \frac{\nabla_1}{\nabla_0} \right\|$, and, in particular, it divides $\frac{\nabla_n}{\nabla_{n-1}} \times \frac{\nabla_{n-2}}{\nabla_{n-3}} \times \frac{\nabla_{n-3}}{\nabla_{n-4}} \times \dots \times \frac{\nabla_1}{\nabla_0} = \frac{\nabla_n \times \nabla_{n-2}}{\nabla_{n-1}}$; *i. e.* $\frac{\nabla_{n-1}}{\nabla_{n-2}}$ divides $\frac{\nabla_n}{\nabla_{n-1}}$. Again, $\nabla_{n-1}, \nabla_{n-2}, \dots \nabla_1, \nabla_0$, which are the determinant and greatest common divisors of the minors of (77.), are also the determinant and greatest common divisors of the minors of the matrix

$$\left\| \frac{\nabla_{n-1}}{\nabla_{n-2}}, \frac{\nabla_{n-2}}{\nabla_{n-3}}, \dots \frac{\nabla_1}{\nabla_0} \right\|; \qquad \dots \dots \dots \dots \qquad (78.)$$

so that if $s \leqq n-2$, ∇_s divides every minor of order s in (78.), and, consequently, the minor $\frac{\nabla_{s+1}}{\nabla_s} \times \frac{\nabla_{s-1}}{\nabla_{s-2}} \times \frac{\nabla_{s-2}}{\nabla_{s-3}} \times \dots \times \frac{\nabla_1}{\nabla_0}$; or $\frac{\nabla_s}{\nabla_{s-1}}$ divides $\frac{\nabla_{s+1}}{\nabla_s}$. It thus appears that in the series of numbers

$$\frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \dots \frac{\nabla_2}{\nabla_1}, \frac{\nabla_1}{\nabla_0}$$

each term is divisible by that which comes after it. Every product of s terms of that series is therefore divisible by the product $\frac{\nabla_s}{\nabla_{s-1}} \times \frac{\nabla_{s-1}}{\nabla_{s-2}} \times \dots \times \frac{\nabla_1}{\nabla_0} = \nabla_s$; or, which is the same thing, ∇_s is the greatest common divisor of the minors of order s in the reduced matrix (78.), and therefore in the given matrix $\|a\|$.

Art. 15. If the proposed matrix $\|a\|$ be not square, but of the type $n \times (n+m)$, let $\|a\| = \|\nabla_n\| \times \|a'\|$, where $\|a'\|$ is a prime matrix of the same type as $\|a\|$, and $\|\nabla_n\|$ a square matrix, of which the determinant is ∇_n, the greatest divisor of $\|a\|$. Then if $\|\nabla_n\|$ be expressed in the form

$$\|v\| \times \left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \ldots \frac{\nabla_1}{\nabla_0} \right\| \times \|u\|,$$

and if, for brevity, we write $\|V\|$ for $\|u\| \times \|a'\|$, we obtain for $\|a\|$ the expression

$$\|a\| = \|v\| \times \left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \ldots \frac{\nabla_1}{\nabla_0} \right\| \times \|V\|. \quad \ldots \quad (79.)$$

The numbers $\nabla_n, \nabla_{n-1}, \ldots$, which are the greatest common divisors of the minors of $\|\nabla_n\|$, are also by the theorem of art. 12, the greatest common divisors of the minors of $\|a\|$. We see therefore that $\frac{\nabla_s}{\nabla_{s-1}}$ is always divisible by $\frac{\nabla_{s-1}}{\nabla_{s-2}}$, in the case of an oblong as well as a square matrix.

Art. 16. To show still more clearly the nature of the quotients $\frac{\nabla_s}{\nabla_{s-1}}$, we add the following proposition:—

"If in any rectangular matrix we divide each minor determinant of order s by the greatest common divisor of its own first minors, the greatest common divisor of all the quotients thus obtained is $\frac{\nabla_s}{\nabla_{s-1}}$."

By this proposition $\frac{\nabla_s}{\nabla_{s-1}}$ is itself defined as a greatest common divisor, instead of being defined as the quotient of one greatest common divisor, divided by another.

To establish its truth we may first consider the quotient $\frac{\nabla_n}{\nabla_{n-1}}$ in any rectangular matrix $\|A\|$ of the type $n \times (m+n)$. Let ω denote the greatest common divisor of the quotients obtained by dividing each determinant of $\|A\|$ by the greatest common divisor of the first minors of that determinant: we have then to show that

$$\frac{\nabla_n}{\nabla_{n-1}} = \omega.$$

Since the greatest common divisor of any vertical column of minors in $\|A\|$ is not altered by premultiplication with a unit-matrix, it is evident that ω as well as $\frac{\nabla_n}{\nabla_{n-1}}$ will remain unchanged by that operation. If, therefore,

$$\|A\| = \|v\| \times \left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \ldots \frac{\nabla_1}{\nabla_0} \right\| \times \|V\|, \quad \ldots \quad (79.)$$

where $\|v\|$ is a unit, and $\|V\|$ a prime matrix, we may consider instead of $\|A\|$, the simpler matrix

$$\left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \ldots \frac{\nabla_1}{\nabla_0} \right\| \times \|V\|. \quad \ldots \quad (80.)$$

Let $\|\theta_1\|, \|\theta_2\|, \ldots$ &c. be the different square matrices of $\|V\|$; $\theta_1, \theta_2, \ldots$ their determi-

nants; ψ_i the greatest common divisor of those first minors in $\|\theta_i\|$ which do not contain the constituents of its uppermost row, so that $\frac{\theta_i}{\psi_i}$ is integral; lastly, let ω_i be the quotient obtained by dividing the determinant

$$\left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \dots \frac{\nabla_1}{\nabla_0} \right\| \times \|\theta_i\|, \quad \dots \dots \dots \dots \quad (81.)$$

by the greatest common divisor of its first minors, so that ω is the greatest common divisor of $\omega_1, \omega_2 \dots$. Now the greatest common divisor of the first minors of (81.) is evidently divisible by ∇_{n-1}, and divides $\nabla_{n-1} \times \psi_i$ (because $\nabla_{n-1}\psi_i$ is the greatest common divisor of one of its rows of minors). Consequently ω_i divides $\nabla_n\theta_i \div \nabla_{n-1}$, and is divisible by $\nabla_n\theta_i \div \nabla_{n-1}\psi_i$. Therefore $\frac{\nabla_n}{\nabla_{n-1}}$ is a common divisor of certain numbers respectively dividing the numbers $\omega_1, \omega_2, \dots$, viz. the numbers $\frac{\nabla_n}{\nabla_{n-1}} \cdot \frac{\theta_i}{\psi_i}$; it is also (because $\theta_1, \theta_2 \dots$ are relatively prime) the greatest common divisor of the numbers $\frac{\nabla_n}{\nabla_{n-1}}\theta_i$, in which the same numbers ω_i are respectively contained; *i. e.* $\frac{\nabla_n}{\nabla_{n-1}}$ is the greatest common divisor of the numbers $\omega_1, \omega_2 \dots$ themselves, or

$$\frac{\nabla_n}{\nabla_{n-1}} = \omega.$$

By the aid of this particular case of the theorem the general proposition itself may be proved as follows:—

If in any rectangular matrix of the type $n \times (m+n)$ we propose to determine Ω_s, the greatest common divisor of the quotients obtained by dividing each minor determinant of order s, by the greatest common divisor of its own first minors, we may begin by selecting any s vertical columns $[s<n]$, and forming the proper quotient for each determinant of order s, contained in this partial matrix of the type $n \times s$. Let λ_i denote the greatest common divisor of these quotients; then, as we have just seen, λ_i is the greatest common divisor of all the determinants of the partial matrix, divided by the greatest common divisor of all its first minors. Hence (by art. 12) λ_i will remain unchanged when the given matrix is premultiplied by a unit-matrix. But Ω_s is the greatest common divisor of all the divisors $\lambda_1, \lambda_2 \dots$ corresponding to every group of s vertical columns; therefore Ω_s is itself unchanged by premultiplication. Similarly, if a *square* matrix be post-multiplied by a rectangular prime matrix, it may be shown that Ω_s is the same for the given square matrix, and for the resulting rectangular matrix. Hence if, as before,

$$\|A\| = \|\omega\| \times \left\| \frac{\nabla_n}{\nabla_{n-1}}, \dots \frac{\nabla_1}{\nabla_0} \right\| \times \|V\|,$$

Ω_s and $\frac{\nabla_s}{\nabla_{s-1}}$ are the same for $\left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \dots \frac{\nabla_1}{\nabla_0} \right\|$ and for $\|A\|$. But in the matrix $\left\| \frac{\nabla_n}{\nabla_{n-1}}, \dots \frac{\nabla_1}{\nabla_0} \right\|$ it is evident that $\frac{\nabla_s}{\nabla_{s-1}}$ and Ω_s coincide; therefore in any rectangular matrix

$$\frac{\nabla_s}{\nabla_{s-1}} = \Omega_s.$$

From the definition of $\frac{\nabla_s}{\nabla_{s-1}}$ as a greatest common divisor, which we have now obtained, we infer that if $\|D\|$ be any matrix containing another matrix $\|\nabla\|$, and if D_s, $D_{s-1} \dots \nabla_s$, $\nabla_{s-1}, \dots$ be the greatest common divisors of the corresponding minors in $\|D\|$ and $\|\nabla\|$ respectively, not only is ∇_s divisible by D_s, and ∇_{s-1} by D_{s-1}, but also $\frac{\nabla_s}{\nabla_{s-1}}$ by $\frac{D_s}{D_{s-1}}$.

It is not difficult to show that in any matrix $\frac{\nabla_s}{\nabla_{s-k}}$ is the greatest common divisor of all the quotients obtained by dividing each minor of order s by the greatest common divisor of its minors of order $s-k$. But as this extension of the preceding result is not needed in what follows, we may omit it here.

We may add, that the theorem of this article is precisely equivalent to the following, which may be demonstrated by a different method.

"If P^{l_s} be the highest power of a given prime that divides all the minors of order s in a given matrix, and if all the minors of order $s-1$ contained in one particular minor of order s are divisible by $P^{l_{s-1}+m}$, that minor is itself divisible by P^{l_s+m}."

It should be observed that whenever all the minors of any determinant are zero, the quotient obtained by dividing the determinant by the greatest common divisor of its minors is also zero.

Art. 17. These results admit of immediate application to the theory of systems of linear congruences. The general type of such systems is

$$\left.\begin{matrix} A_{i,1}x_1 + A_{i,2}x_2 + \dots A_{i,n}x_n \equiv A_{i,n+1}, \text{ mod. } M \\ i = 1, 2, 3, \dots n' \end{matrix}\right\}; \quad \dots\dots \quad (82.)$$

and to construct a complete theory of them it is requisite, first, to assign a criterion for their resolubility or irresolubility; secondly, when they are resoluble, to investigate the number of incongruous solutions of which they are susceptible; and, lastly, to exhibit a method for obtaining all these solutions. We shall first suppose that $n'=n$; i. e. that the proposed system is neither defective nor redundant.

Let D_n, $D_{n-1} \dots \nabla_n$, $\nabla_{n-1}, \dots$ respectively denote the greatest common divisors of the determinants and minors of the augmented and unaugmented matrices of the system (82.); also let δ_n, $\delta_{n-1}, \dots \delta_1$ denote the greatest common divisors of M with $\frac{\nabla_n}{\nabla_{n-1}}$, of M with $\frac{\nabla_{n-1}}{\nabla_{n-2}}, \dots$, and let d_n, $d_{n-1} \dots$ similarly represent the greatest common divisors of M with $\frac{D_n}{D_{n-1}}$, of M with $\frac{D_{n-1}}{D_{n-2}}$, &c.; then, if $d = d_n \times d_{n-1} \times \dots \times d_1$, $\delta = \delta_n \times \delta_{n-1} \times \dots \times \delta_1$, we have the two following theorems:

(i.) "The necessary and sufficient condition for the resolubility of the system (81.) is $d=\delta$."

(ii.) "When this condition is satisfied, the number of its incongruous solutions is d."

To demonstrate the first of these theorems, we revert to the principle of art. 11, from which it appears that the necessary and sufficient condition for the resolubility of the system (82.) is that the greatest divisors of the two matrices

$$\left\|\begin{matrix} M, 0, 0, .\ 0, A_{1,1}, \dots A_{1,n} \\ 0, M, 0, .\ 0, A_{2,1}, \dots A_{2,n} \\ 0, 0, M, .\ 0, A_{3,1}, \dots A_{3,n} \\ \dots\dots\dots\dots \\ 0, 0, 0, .\ M, A_{n,1}, \dots A_{n,n} \end{matrix}\right| \quad \dots\dots\dots \quad (83.)$$

and

$$\left\|\begin{matrix} M, 0, 0, .\ 0, A_{1,1}, \dots A_{1,n+1} \\ 0, M, 0, .\ 0, A_{2,1}, \dots A_{2,n+1} \\ 0, 0, M, .\ 0, A_{3,1}, \dots A_{3,n+1} \\ \dots\dots\dots\dots\dots \\ 0, 0, 0, .\ M, A_{n,1}, \dots A_{n,n+1} \end{matrix}\right| \quad \dots\dots\dots \quad (84.)$$

are to be equal to one another. Now the first of those greatest common divisors is evidently the greatest common divisor of

$$M^n, M^{n-1}\nabla_1, M^{n-2}\nabla_2, \dots M\nabla_{n-1}, \nabla_n;$$

which, for brevity, we shall represent by the symbol

$$[M^n, M^{n-1}\nabla_1, M^{n-2}\nabla_2, \dots \nabla_{n-1}, M, \nabla_n]. \quad \dots\dots\dots \quad (85.)$$

Let $M = P \times Q \times R \dots$, P, Q, R, ... denoting powers of different primes; we may then, in (85.), replace M by P, Q, R, ... successively, since

$$[M^n, M^{n-1}\nabla_1, \dots M\nabla_{n-1}, \nabla_n]$$
$$= [P^n, P^{n-1}\nabla_1, \dots P\nabla_{n-1}, \nabla_n] \times [Q^n, Q^{n-1}\nabla, \dots \nabla_n] \times \dots$$

If P divide any one of the numbers $\frac{\nabla_s}{\nabla_{s-1}} \dots$, let $\frac{\nabla_s}{\nabla_{s-1}}$ be the least of them that it divides; also let $P_i = \left[P, \frac{\nabla_i}{\nabla_{i-1}}\right]$; so that $P_i = P$, if $i \overline{=} s$. Then

$$[P^n, P^{n-1}\nabla_1, \dots P\nabla_{n-1}, \nabla_n]$$
$$= P_1 \times \left[\frac{P^n}{P_1}, P^{n-1}\frac{\nabla_1}{P_1}, P^{n-2}\frac{\nabla_2}{P_1}, \dots \frac{\nabla_n}{P_1}\right]$$
$$= P_1 \times \left[P^{n-1}, P^{n-2}\frac{\nabla_2}{\nabla_1}, P^{n-3}\frac{\nabla_3}{\nabla_1}, \dots \frac{\nabla_n}{\nabla_1}\right],$$

observing that $\frac{\nabla_1}{P_1}$ is prime to P [if $s > 1$], and that we may therefore divide the last n numbers by $\frac{\nabla_1}{P_1}$; and may then omit $\frac{P^n}{P_1}$, which is divisible by P^{n-1}. Continuing this process, we find

$$[P^n, P^{n-1}\nabla_1, \dots P\nabla_{n-1}, \nabla_n]$$
$$= P_1 \times P_2 \times \dots \times P_{s-1}\left[P^{n-s+1}, P^{n-s}\frac{\nabla_s}{\nabla_{s-1}}, P^{n-s-1}\frac{\nabla_{s+1}}{\nabla_{s-1}}, \dots \frac{\nabla_n}{\nabla_{s-1}}\right];$$

or, since $\frac{\nabla_s}{\nabla_{s-1}}$ is divisible by P, and $\frac{\nabla_{s+k}}{\nabla_{s-1}} = \frac{\nabla_{s+k}}{\nabla_{s+k-1}} \times \frac{\nabla_{s+k-1}}{\nabla_{s+k-2}} \cdots \times \frac{\nabla_s}{\nabla_{s-1}}$ by P^{k+1},

$$[P^n, P^{n-1}\nabla_1, P^{n-2}\nabla_2, \ldots . P\nabla_{n-1}, \nabla_n]$$
$$= P_1 \times P_2 \times P_3 \ldots \times P_{s-1} \times P^{n-s+1}$$
$$= \Pi_{i=1}^{i=s} . P_i.$$

But $\delta_i = P_i \times Q_i \times R_i \times \ldots$; and consequently the greatest common divisor of the determinants of (83.) is $\delta_1 \times \delta_2 \times \ldots \times \delta_n$ or δ. Similarly, the greatest divisor of (84.) is $d_1 \times d_2 \times \ldots \times d_n$ or d. The necessary and sufficient condition for the resolubility of the proposed system of congruences is therefore contained in the formula

$$d = \delta.$$

It should, however, be observed that, since $\frac{D_s}{D_{s-1}}$ divides $\frac{\nabla_s}{\nabla_{s-1}}$ (art. 16), d_s divides δ_s, and therefore the equation

$$d = \delta$$

involves the coexistence of the n equations

$$d_1 = \delta_1, \; d_2 = \delta_2, \ldots d_n = \delta_n. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (86.)$$

To investigate the number of solutions of the system (82.), supposed to be resoluble, let $\|\alpha\|$ and $\|\beta\|$ be two unit-matrices satisfying the equation

$$\|\alpha\| \times \|A\| \times \|\beta\| = \left\| \frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \ldots \frac{\nabla_1}{\nabla_0} \right\|; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (87.)$$

also let

$$\left.\begin{array}{c} x_i = \beta_{i,1} v_1 + \beta_{i,2} v_2 + \ldots \beta_{i,n} v_n \\ i = 1, 2, 3, \ldots n \end{array}\right\}$$

$$\left.\begin{array}{c} c_i = \alpha_{i,1} A_{1,n+1} + \alpha_{i,2} A_{2,n+1} + \ldots \alpha_{i,n} A_{n,n+1} \\ i = 1, 2, 3, \ldots n. \end{array}\right\}$$

Then it is evident that the proposed system of congruences is precisely equivalent to the system

$$\left.\begin{array}{c} \frac{\nabla_{n-i+1}}{\nabla_{n-i}} v_i \equiv c_i \text{ mod. M}, \\ i = 1, 2, 3, \ldots n \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (88.)$$

in such a manner that the two systems are simultaneously resoluble or irresoluble; and that from any number of incongruous solutions of the one an equal number of incongruous solutions of the other is deducible. But the whole number of incongruous solutions of (88.) is $\delta_1 \times \delta_2 \times \ldots \times \delta_n = \delta$; *i. e.* the number of solutions of the proposed system is δ.

By the use of the unit-matrices $\|\alpha\|$ and $\|\beta\|$ the actual resolution of the proposed system is made to depend on the resolution of the n congruences contained in (88.). But this method of solving a system of linear congruences, though very symmetrical, is perhaps too tedious for the purposes of computation.

Art. 18*. Let the proposed system of congruences be the *defective* system

$$\left.\begin{aligned} &A_{i,1}x_1+A_{i,2}x_2+\ldots+A_{i,n+m}x_{n+m}\equiv A_{i,n+m+1}, \text{ mod. M},\\ &i=1, 2, 3, \ldots n,\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (89.)$$

and let the notation of the last Article be retained. It is easily seen that the condition of resolubility of the system (89.) is, as before,

$$\delta=d.$$

But the number of its incongruous solutions, when that condition is satisfied, is not δ, but $\delta\times M^m$. For we have seen that we can find a unit-matrix $\|\alpha\|$, and a prime matrix $\|A'\|$ of the type $n\times(n+m)$, satisfying the equation

$$\|\alpha\|\times\|A\|=\left\|\frac{\nabla_n}{\nabla_{n-1}}, \frac{\nabla_{n-1}}{\nabla_{n-2}}, \ldots \frac{\nabla_2}{\nabla_1}, \frac{\nabla_1}{\nabla_0}\right\|\times\|A'\|;$$

we may therefore replace the system (89.) by a system of the form

$$\frac{\nabla_{n-i+1}}{\nabla_{n-i}}U_i\equiv C_i, \text{ mod. M}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (90.)$$

in which

$$U_i=A'_{i,1}x_1+A'_{i,2}x_2+\ldots+A'_{i,n+m}x_{n+m},$$

and

$$C_i=\alpha_{i,1}A_{1,n+m+1}+\alpha_{i,2}A_{2,n+m+1}+\ldots\alpha_{i,n}A_{n,n+m+1}.$$

If the system (89.) is resoluble, the system (90.) will be so too, and will give d or δ different systems of values for $U_1, U_2, \ldots U_n$, any one of which may be represented by the formula

$$\left.\begin{aligned} &U_i\equiv u_i, \text{ mod. M},\\ &i=1, 2, 3, \ldots n\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (91.)$$

Let us replace the modulus M by P, the highest power of one of its prime divisors. Since $\|A'\|$ is a prime matrix, one at least of its determinants, for example, the determinant $\Sigma\pm A'_{1,1}A'_{2,2}\ldots A'_{n,n}$, is prime to P. It will follow from this that, whatever values we attribute to $x_{n+1}, x_{n+2}, \ldots x_{n+m}$, each of the δ systems represented by (91.) is resoluble for the modulus P, and gives, for any assumed values of $x_{n+1}, x_{n+2}, \ldots x_{n+m}$, only one set of values of $x_1, x_2, \ldots x_n$. Each of those δ systems admits, therefore, of P^m solutions for the modulus P, *i. e.* of M^m for the modulus M. The system (89.) will consequently admit of $\delta\times M^m$ solutions.

Let us also consider the *redundant* system of congruences,

$$\left.\begin{aligned} &A_{i,1}x_1+A_{i,2}x_2+\ldots+A_{i,n}x_n\equiv A_{i,n+1}, \text{ mod. M},\\ &i=1, 2, 3, \ldots n+m,\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (92.)$$

and let D_{n+1} denote the greatest divisor of its augmented matrix. Let p represent a prime divisor of M, and let p^θ, p^{θ_1}, p^{θ_2} be the highest powers of p, which divide M, D_n, ∇_n respectively. The condition of resolubility of art. 11, applied to the system (92.), considered with respect to the modulus p^θ, becomes, after division by $p^{(m-1)\theta}$,

$$\left.\begin{aligned} &[p^{\theta_{n+1}}, p^{\theta_n+\theta}, p^{\theta_{n-1}+2\theta}, \ldots\ldots p^{(n+1)\theta}]\\ &=[p^{\theta_n+\theta}, p^{\theta_{n-1}+2\theta}, \ldots\ldots p^{(n+1)\theta}]\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (93.)$$

* [This article has been added since the paper was read. The theorems contained in it are supplementary to that of the preceding article. September 1861, H. J. S. S.]

And this equation is impossible, if $\theta > I_{n+1} - I_n$. For $I_{r+1} - I_r \geq I_r - I_{r-1}$, because $\frac{D_{r+1}}{D_r}$ is divisible by $\frac{D_r}{D_{r-1}}$; the inequality, $I_{n+1} < I_n + \theta$, involves, therefore, the inequalities

$$\left.\begin{aligned} &I_{n+1} < I_{n-s+1} + s\theta, \\ &s = 1, 2, 3, \ldots n+1, \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (94.)$$

and these, again, imply the corresponding inequalities

$$\left.\begin{aligned} &I_{n+1} < i_{n-s+1} + s\theta, \\ &s = 1, 2, 3, \ldots n+1, \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (95.)$$

because $I_{n-s+1} \leqq i_{n-s+1}$. From (94.) it appears that the value of $[p^{I_{n+1}}, p^{I_n+\theta}, \ldots p^{(n+1)\theta}]$ is $p^{I_{n+1}}$, and from (95.), that the value of $[p^{I_n+\theta}, p^{I_{n-1}+2\theta}, \ldots p^{(n+1)\theta}]$ is a power of p superior to $p^{I_{n+1}}$; *i. e.* the equation (93.) is impossible. We thus obtain, as a first condition for the resolubility of the proposed system (92.), the congruence

$$\frac{D_{n+1}}{D_n} \equiv 0, \text{ mod. } M. \quad \ldots\ldots\ldots\ldots \quad (96.)$$

When this condition is satisfied, we obtain from (93.), omitting the term $p^{I_{n+1}}$ (because $I_n + \theta \geqq I_n + \theta$), and dividing by p^θ, the equation of condition,

$$[p^{I_n}, p^{I_{n-1}+\theta}, p^{I_{n-2}+2\theta}, \ldots p^{n\theta}] = [p^{I_n}, p^{I_{n-1}+\theta}, p^{I_{n-2}+2\theta}, \ldots p^{n\theta}],$$

which leads us (as in the last article) to the simple formula

$$d = \delta.$$

This equation, therefore, and the congruence (96.), express the necessary and sufficient conditions for the resolubility of the proposed redundant system.

When these two conditions are simultaneously satisfied, the number of incongruous solutions is δ. For, if we again consider the proposed system of congruences with respect to the modulus p^θ, and select from it a partial system of n congruences such that the determinants of its augmented matrix, which are necessarily divisible by p^{I_n}, are not divisible by any higher power of p, it is readily seen that every set of values of the indeterminates $x_1, x_2, \ldots x_n$, which satisfies the partial system, will also (by virtue of the inequality $\theta \leqq I_{n+1} - I_n$) satisfy the remaining congruences of the proposed system. The number of solutions of the proposed system is therefore the same as that of the partial system. And because p^{I_n} the highest power of p which divides every determinant of order n in the augmented matrix of the proposed system is also the highest power of p which divides the augmented matrix of the partial system, it follows from the last theorem of art. 16, that $p^{I_{n-1}}, p^{I_{n-2}}, \ldots\ldots$ are the highest powers of p which divide the corresponding orders of determinants in the latter, as well as in the former matrix. The number of solutions of the partial system (and consequently of the proposed system), considered with respect to the modulus p^θ, is therefore expressed by the formula

$$[p^{I_n}, p^{I_{n-1}+\theta}, \ldots\ldots p^{n\theta}];$$

or, finally, the number of solutions of the proposed system, considered with respect to M as modulus, is d or δ.

Art. 19. We shall terminate this paper with an elementary theorem relating to linear systems of equations, which admits of frequent application in other parts of the theory of numbers.

Resuming the notation of art. 11, we may see from the theorem of that article, that if the system (56.) be resoluble for any given values of the numbers $A_{1,0}, A_{2,0}, \dots A_{n,0}$, it is also resoluble for any other values of those numbers, respectively congruous, for the modulus D, to the given values; so that the resolubility or irresolubility of the system depends exclusively on the residues of the numbers $A_{i,0}$, mod. D. There are D^n possible combinations of these residues, and we shall now show that for D^{n-1} of them the system is resoluble, while for the remaining $D^{n-1}(D-1)$ it is irresoluble. For this purpose let

$$\|\alpha\| \times \|A\| = \left\| \frac{D_n}{D_{n-1}}, \frac{D_{n-1}}{D_{n-2}}, \dots \frac{D_1}{D_0} \right\| \times \|A'\|, \quad \dots\dots (97.)$$

$\|\alpha\|$ denoting a unit-matrix, and $\|A'\|$ a prime matrix of the same type as $\|A\|$, while D_n, D_{n-1}, D_1, D_0 are of course the greatest common divisors of the determinants and minors of $\|A\|$. Let also

$$-C_i = A_{1,0}\,\alpha_{i,1} + A_{2,0}\,\alpha_{i,2} + \dots + A_{n,0}\,\alpha_{i,n}.$$

The given system is then exactly equivalent to the system

$$\left.\begin{aligned} \frac{D_{n-i+1}}{D_{n-i}}[A'_{i,1}x_1 + A'_{i,2}x_2 + \dots + A'_{i,n+m}x_{n+m}] &= C_i \\ i &= 1, 2, 3, \dots n. \end{aligned}\right\} \quad \dots\dots (98.)$$

For the resolubility of this system it is requisite that C_i should be divisible by $\frac{D_{n-i+1}}{D_{n-i}}$; and this condition is sufficient as well as necessary, because $\|A'\|$ is a prime matrix. Now of the D or D_n values, incongruous for the modulus D, which may be attributed to C_n, $\frac{D_n \times D_{n-i}}{D_{n-i+1}}$ are divisible by $\frac{D_{n-i+1}}{D_{n-i}}$; whence it is evident that of the D^n systems of values which may be attributed to $C_1, C_2, \dots C_n$, $D^n \div \left[\frac{D_1}{D_0}, \frac{D_2}{D_1}, \frac{D_3}{D_2} \cdots \frac{D_n}{D_{n-1}}\right]$, *i. e.* D^{n-1} render the system (98.) resoluble. Consequently the given system is also resoluble for D^{n-1}, and no more, of the systems of values that can be attributed (mod. D) to $A_{1,0}, A_{2,0}, \dots A_{n,0}$.

Art. 20. The methods employed in the present paper are without exception such as to be immediately applicable to any species of complex numbers which admit of resolution into actual or ideal prime factors. And the greater part of the results at which we have arrived may be transferred, *mutatis mutandis*, to the theories of such numbers. For example, if in the equations (56.) we suppose the constituents of $\|A\|$ to represent complex numbers, it will be found that the criterion for the resolubility or irresolubility of the system, which we have demonstrated in the case of ordinary integers, applies equally in the case of complex numbers; and again, the condition of resolubility of a system of congruences of which the modulus as well as the coefficients are complex numbers, is

precisely the same as in the case of common whole numbers; while the expression for the number of the solutions (when the condition of resolubility is satisfied) is simply the *norm* of m.

But without entering into the developments which this extension of the subject of this paper would require, we shall confine ourselves to an application of the result of the last article to a demonstration of the fundamental principle in the arithmetical theory of complex numbers, that the number of incongruous residues for any complex modulus is represented by the norm of the modulus.

Let α be one of the roots $\alpha_1, \alpha_2, \ldots \alpha_n$ of the equation $F_n(x)=0$, which is supposed to be of n dimensions, to be irreducible, and to have all its coefficients integral, and that of its first term unity. Let also $\varphi_{n-1}(\alpha)$ be the complex modulus under consideration; its norm, which we shall symbolize by N, is defined by the equation

$$N=N.\varphi_{n-1}(\alpha)=\Pi_{i=1}^{i=n}\varphi_{n-1}(\alpha_i).$$

Consider the N_{2n-1} residues (incongruous mod. N) which are included in the formula

$$R_{2n-2}(\alpha), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (99.)$$

where R_{2n-2} denotes an integer function of $2n-2$ dimensions; it is evident that every complex number is congruous, for the modulus $\varphi_{n-1}(\alpha)$, to one at least of these N^{2n-1} residues. If R and R′ be any two (the same or different) of the same residues, it is also plain that the congruence

$$R \equiv R', \text{ mod. } \varphi_{n-1}(\alpha)$$

will, or will not, be satisfied, according as it is, or is not, possible to assign two functions of x, $F_{n-1}(x)$ and $\varphi_{n-2}(x)$ having integer coefficients, and satisfying the equation

$$F_n(x)\varphi_{n-2}(x)+F_{n-1}(x)\varphi_{n-1}(x)=R(x)-R'(x). \quad . \quad . \quad . \quad . \quad (100.)$$

This equation is equivalent to a system of $2n-1$ linear equations, in which the unknown quantities are the $2n-1$ coefficients of $\varphi_{n-2}(x)$ and $F_{n-1}(x)$, and of which the determinant is the dialytic resultant of $F_n(x)$ and $\varphi_{n-1}(x)$, *i. e.* the norm of $\varphi_{n-1}(\alpha)$ or N. If then we suppose $R(\alpha)$ to represent any given residue included in the formula (99.), it will appear from the theorem of the last article that the equation (100.) is resoluble for N^{2n-2} different values of $R'(x)$, *i. e.* that every complex number is congruous, for the modulus $\varphi_{n-1}(\alpha)$, to precisely N^{2n-2} of the N^{2n-1} residues contained in the formula (99.), or that the number of residues, incongruous mod. $\varphi_{n-1}(\alpha)$, is precisely N.

It is, however, proper to observe, that a complete demonstration of this important theorem has already been given by Professor SYLVESTER (see a paper signed "Lanavicensis," in the 'Quarterly Journal of Pure and Applied Mathematics,' vol. iv. p. 94 and 124).

XVI. *On the Method of Symmetric Products, and on Certain Circular Functions connected with that Method.* By the Rev. Robert Harley, *F.R.A.S., Corresponding Member of the Literary and Philosophical Society of Manchester. Communicated by* A. Cayley, *Esq., F.R.S.*

Received October 18,—Read December 13, 1860.

In a paper printed in the second part of the fifteenth volume of the 'Manchester Memoirs,' I have given a systematic exposition of Mr. Cockle's Method of Symmetric Products, and its application to the finite algebraic solution of the lower equations. In that paper, to which I shall in future refer as my original memoir, I have also defined a new cyclical symbol, and I have by its aid succeeded in effecting the direct calculation of a certain sextic equation, on whose solution that of the general quintic may be made to depend. In an Addendum I have pointed out the connexion between the circular functions which occur in my own researches and those to which we are led by the theory of Lagrange and Vandermonde, and, by means of the cyclical process, I have given a neat expression for the first coefficient of Lagrange's reducing equation. These researches I have followed up in an article "On the Theory of Quintics," in the third volume of the 'Quarterly Journal of Pure and Applied Mathematics.' My present purpose is not to repeat, but to endeavour to generalize and extend former results. I shall therefore content myself with a very brief *résumé* of my investigations, referring the reader for details to the above works. Mr. Cockle's earlier researches on the subject were published in a series of five papers "On the Transformation of Algebraic Equations," printed in the first and third volumes of 'The Mathematician*.'

Section I.

The Method of Symmetric Products, and a New Application of it to the Solution of the Lower Equations.

1. Any n symbols $x_1, x_2, x_3, \dots x_n$ may be regarded as the roots of an equation of the form

$$(a, b, c, \dots \overline{x, 1})^n = a(x-x_1)(x-x_2)(x-x_3)\dots(x-x_n) = 0\dagger.$$

* The first paper of the series appeared in 'The Mathematician' for March 1844. Mr. Cockle's subsequent contributions to the subject will be found in the 'Mechanics' Magazine,' the 'Cambridge and Dublin Mathematical Journal,' the 'Lady's and Gentleman's Diary,' the 'Philosophical Magazine,' 'Liouville's Journal,' the 'Quarterly Journal of Pure and Applied Mathematics,' the 'Manchester Memoirs,' and elsewhere.

† Throughout the whole of this paper I adopt Mr. Cayley's quantical notation. In the ordinary nota-

Let $X_1, X_2, X_3, \dots X_{n-1}$ be linear unsymmetric functions of x and of the form

$$x_1+k_1x_2+k_2x_3+\dots+k_{n-2}x_{n-1}+k_{n-1}x_n,$$

where the $n-1$ constants $k_1, k_2, \dots k_{n-1}$ are arbitrary. Then if n be less than 5, the constants which occur in the $n-1$ functions may be so distributed and determined as to render the product

$$\pi_{n-1}(x) \text{ or } X_1X_2X_3\dots X_{n-1}$$

(or, when $n=2$, X^2) symmetric relatively to x; but if n be equal to, or greater than 5, the symmetry is not in general attainable. The product $\pi_{n-1}(x)$ is called the *symmetric* or *resolvent product* according as it is or is not symmetric. When this symmetry exists, $\pi_{n-1}(x)$ can of course be expressed as a rational function of the coefficients a, b, c, &c. When it does not exist, we form the analogues of the functions which enter into the symmetric cases, and the symmetric product $\Pi(x)$ is then obtained by multiplying together the several unequal values which the resolvent product $\pi_{n-1}(x)$ can be made to take by the permutation of the x's; and $\Pi(x)$ may, in this case also, be expressed as a rational function of the coefficients.

2. The simplest case is the quadratic

$$(a, b, c\,\overline{]}x, 1)^2=a(x-x_1)(x-x_2)=0.$$

Here

$$\{\pi_1(x)\}^2=X^2=(x_1+kx_2)^2,$$

and it is seen at a glance that when $k=-1$, X is unsymmetric and X^2 is symmetric. Hence

$$X^2=(\Sigma x)^2-4x_1x_2=\frac{1}{a^2}(b^2-4ac),$$

and the roots of the quadratic are

$$\frac{1}{2a}\{-b\pm a\pi_1(x)\}=\frac{1}{2a}(-b\pm\sqrt{b^2-4ac}).$$

3. The next case is the cubic

$$(a, b, c, d\,\overline{]}x, 1)^3=a(x-x_1)(x-x_2)(x-x_3)=0.$$

Assume

$$X_1=x_1+k_1x_2+k_2x_3,$$

$$X_2=x_1+k_2x_2+k_1x_3,$$

and

$$\pi_2(x)=X_1X_2;$$

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tion $(a, b, c, \dots\,\overline{]}x, y)^n$ would be written

$$ax^n+bx^{n-1}y+cx^{n-2}y^2+\text{ &c.},$$

and $(a, b, c, \dots\,\overline{[}x, y)^n$ would be written

$$ax^n+\frac{n}{1}bx^{n-1}y+\frac{n(n-1)}{1.2}cx^{n-2}y^2+\text{ &c.}$$

then, combining the conditions of symmetry, and rejecting those values of k (k, or k, indifferently) which render X symmetric, we find

$$k^2+k+1=0,$$

that is, k is an unreal cube root of unity. Represent this root by α, and put

$$f(\alpha)=x_1+\alpha x_2+\alpha^2 x_3;$$

then

$$f(\alpha^2)=x_1+\alpha^2 x_2+\alpha x_3,$$

and

$$\pi_3(x)=f(\alpha)\cdot f(\alpha^2)=(\Sigma x)^3-3\Sigma x_1x_2=\frac{1}{a^3}(b^3-3ac).$$

If, in evolving the roots by this method, we replace $f(\alpha)$ by f, and $\pi_3(x)$ by π, there will result

$$f=x_1+\alpha x_2+\alpha^2 x_3,$$

$$\frac{\pi}{f}=x_1+\alpha^2 x_2+\alpha x_3,$$

$$-\frac{b}{a}=x_1+\quad x_2+\quad x_3;$$

so that

$$f+\frac{\pi}{f}=\frac{1}{a}(b+3ax_1),$$

$$\alpha^2 f+\frac{\pi}{\alpha^2 f}=\frac{1}{a}(b+3ax_2),$$

$$\alpha f+\frac{\pi}{\alpha f}=\frac{1}{a}(b+3ax_3);$$

and therefore

$$f^3+\frac{\pi^3}{f^3}=\frac{1}{a^3}(1,\ -3b,\ 3^2ac,\ -3^3a^2d)(b,\ 1)^3$$

$$=\frac{1}{a^3}(-2b^3+9abc-27a^2d);$$

whence, solving as for a quadratic in f^3, restoring the value of π, and extracting the cube root, we have

$$f=\frac{1}{a\sqrt[3]{2}}\left\{-27a^2d+9abc-2b^3+3a\sqrt{3}(27a^2d^2-18abcd+4ac^3+4b^3d-b^2c^2)^{\frac{1}{2}}\right\}^{\frac{1}{3}},$$

and the roots of the complete cubic are included in the formula

$$\frac{1}{3}\left(\alpha^m f+\frac{\pi}{\alpha^m f}-\frac{b}{a}\right),$$

where $m=1$, 2, or 3.

In my original memoir I followed Mr. Cockle, and in the application of the theory to the solution of the cubic and the biquadratic, I employed a subsidiary equation of the same degree. This equation was obtained by eliminating x between the given one and

$$y-\psi(x)=0,$$

ψ being rational and so constructed as to make $\pi(y)$ vanish. It is true that the evanescence of π leads to an immediate solution, and that when y is known, x is also known. But this evanescence is not essential to the theory; and we are conducted to more significant results by dispensing with it.

4. For the quartic

$$(a, b, c, d, e)(x, 1)^4=0,$$

assume (cyclically)

$$\begin{aligned}
\mathrm{X}_1&=x_1+k_1x_2+k_2x_3+k_3x_4,\\
\mathrm{X}_2&=x_1+k_2x_2+k_3x_3+k_1x_4,\\
\mathrm{X}_3&=x_1+k_3x_2+k_1x_3+k_2x_4,
\end{aligned}$$

and

$$\pi_3(x)=\mathrm{X}_1\mathrm{X}_2\mathrm{X}_3.$$

Then, as before, combining the conditions of symmetry and rejecting those values of k which would render X symmetric, we are led to the cubic

$$k^3+k^2-k-1=0,$$

of which the roots are -1, 1 and -1. Let therefore

$$\begin{aligned}
\mathrm{X}_1&=x_1-x_2+x_3-x_4,\\
\mathrm{X}_2&=x_1+x_2-x_3-x_4,\\
\mathrm{X}_3&=x_1-x_2-x_3+x_4,
\end{aligned}$$

then

$$\pi_3(x)=\mathrm{X}_1\mathrm{X}_2\mathrm{X}_3=(\Sigma x)^3-4\Sigma x\Sigma x_1x_2+8\Sigma x_1x_2x_3=\frac{1}{a^3}(-b^3+4abc-8a^2d).$$

5. The following solution is due to Mr. Cockle, who communicated it to me in September of last year. It may be considered as an extension of a solution of the complete cubic which I sent to him in January of the same year, and which, not essentially differing from the above, has a certain resemblance to that given by Murphy in the 'Philosophical Transactions' for 1837.

Let

$$f_1=x_1-x_2+x_3-x_4,$$

and

$$f_2=x_1+x_2-x_3-x_4;$$

then

$$\frac{\pi}{f_1f_2}=x_1-x_2-x_3-x_4,$$

and as in cubics,

$$\begin{aligned}
f_1+f_2-\frac{\pi}{f_1f_2}&=\frac{1}{a}(b+4ax_1),\\
-f_1+f_2-\frac{\pi}{f_1f_2}&=\frac{1}{a}(b+4ax_2),\\
f_1-f_2-\frac{\pi}{f_1f_2}&=\frac{1}{a}(b+4ax_3),\\
-f_1-f_2+\frac{\pi}{f_1f_2}&=\frac{1}{a}(b+4ax_4);
\end{aligned}$$

therefore

$$\left(f_1^2+f_2^2-\frac{\pi^2}{f_1f_2}\right)^2-4f_1^2f_2^2$$

$$=\frac{1}{a^4}(1,\ -4b,\ 4^2ac,\ -4^2a^2d,\ 4^4a^3e)(b,\ 1)^4$$

$$=\frac{1}{a^4}(256a^3e-64a^2bd+16ab^2c-3b^4);$$

and

$$f_1^2+f_2^2+\frac{\pi^2}{f_1f_2}=\frac{1}{a^2}(b^2+2ab\Sigma x+4a^2\Sigma x^2)$$

$$=\frac{1}{a^2}(-8ac+3b^2);$$

consequently

$$\left(f_1^2+f_2^2+\frac{\pi^2}{f_1f_2}\right)^2-\left(f_1^2+f_2^2-\frac{\pi^2}{f_1f_2}\right)^2+4f_1^2f_2^2$$

$$=\frac{1}{a^4}\{(8ac-3b^2)^2-(256a^3e-64a^2bd+16ab^2c-3b^4)\};$$

or

$$f_1^2f_2^2+f_1^2\cdot\frac{\pi^2}{f_1f_2}+f_2^2\cdot\frac{\pi^2}{f_1f_2}=\frac{1}{a^4}(-64a^3e+16a^2bd+16a^2c^2-16ab^2c+3b^4).$$

It hence appears that f_1^2, f_2^2 and $\frac{\pi^2}{f_1f_2}$ are the roots of the cubic

$$(a^4,\ 8a^2c-3a^2b^2,\ -64a^3e+16a^2bd+16a^2c^2-16ab^2c+3b^4,\ a^4x^{12})(f^2,\ 1)^3=0.$$

When f is known, x is given by

$$x_1=\tfrac{1}{4}\left(f_1+f_2+\frac{\pi}{f_1f_2}-\frac{b}{a}\right),$$

and the corresponding formulæ for x_2, x_3, x_4 may be readily obtained.

6. Next, for the quintic

$$(a,\ b,\ c,\ d,\ e,\ f)(x,\ 1)^5=0,$$

assume

$$X_1=x_1+k_1x_2+k_2x_3+k_3x_4+k_4x_5,$$
$$X_2=x_1+k_2x_2+k_4x_3+k_1x_4+k_3x_5,$$
$$X_3=x_1+k_3x_2+k_1x_3+k_4x_4+k_2x_5,$$
$$X_4=x_1+k_4x_2+k_3x_3+k_2x_4+k_1x_5,$$

and $\pi_4(x)=X_1X_2X_3X_4$.

In regard to the above distribution of the constants k_1, k_2, k_3, k_4, it will be observed that they are arranged according to the following scheme:

1	2	3	4
2	4	1	3
3	1	4	2
4	3	2	1

That is, the four horizontal rows read downwards are identical in value and order with the four vertical columns read from left to right, while k_1 and k_4 lie in inverse symmetry upon, and k_2 and k_3 around, diagonals. Probably no other distribution would render $\pi_4(x)$ more nearly symmetrical*.

Combining, as in former cases, the conditions of symmetry and rejecting incongruous results, we arrive at the quartic

$$k^4+k^3+k^2+k+1=0,$$

of which the roots are the unreal fifth roots of unity. Let then ω, ω^2, ω^3, and ω^4 denote these roots, and let

$$f(\omega)=x_1+\omega x_2+\omega^2 x_3+\omega^3 x_4+\omega^4 x_5;$$

then

$$f(\omega^2)=x_1+\omega^2 x_2+\omega^4 x_3+\omega x_4+\omega^3 x_5,$$
$$f(\omega^3)=x_1+\omega^3 x_2+\omega x_3+\omega^4 x_4+\omega^2 x_5,$$
$$f(\omega^4)=x_1+\omega^4 x_2+\omega^3 x_3+\omega^2 x_4+\omega x_5,$$

and

$$\begin{aligned}\pi_4(x)&=f(\omega).f(\omega^2).f(\omega^3).f(\omega^4)\\&=(\Sigma x)^4-5(\Sigma x)^2\Sigma x_1x_2+5(\Sigma x_1x_2)^2+5\tau\tau'\\&=\frac{1}{a^4}(b^4-5ab^2c+5a^2c^2)+5\tau\tau',\end{aligned}$$

where

$$\tau=x_1x_2+x_2x_3+x_3x_4+x_4x_5+x_5x_1,$$

and

$$\tau'=x_1x_3+x_2x_4+x_3x_5+x_4x_1+x_5x_2.$$

These two functions, τ and τ', are circular, and complementary to each other. Since their product is unsymmetric relatively to x, it follows that $\pi_4(x)$ cannot in general be rendered symmetric. Before proceeding to discuss this product, it will be convenient to introduce some other matters connected with the general theory.

Section II.—*Circular Functions and the New Cyclical Symbol.*

7. In the transformation and general treatment of the higher equations circular functions occupy a conspicuous place, and play an important part. An attentive consideration of the structure of such functions will enable us to devise a calculus whereby operations upon them will be materially abridged. The theory is far from being complete, and its practical application admits of great improvement. In my original memoir I have proposed and applied a symbol which not only helps, I think, to throw some light on the general theory, but also enables us to effect with ease and rapidity calculations which would otherwise be very laborious, if not wholly impracticable. The method there employed exhibits to the eye and to the mind the various combinations of dimen-

* When all the divisors of n are even, a cyclical arrangement must be adopted, as in the case for quartics (art. 4). When n is prime or odd, an arrangement similar to the above, or a modification of it, will probably be found available.

sions without rendering it necessary to examine all the results of multiplication, or to make a hazardous selection of those which may be deemed material. Combining as many terms (n) in one as there are roots involved, the cyclical symbol reduces the labour of multiplication alone by $\frac{n-1}{n}$th; and in passing from the circular to the corresponding symmetric function the process also affords considerable facilities.

8. Let $\chi(0)$ be a function of x, and let $\chi(q)$ be derived from $\chi(0)$ by advancing each of the roots contained in its q steps in a given cycle r. *Ex. gr.* Suppose that

$$\chi(0)=x_1x_2,$$

and that we are following the first cycle, which, indeed, may be taken as a type of all others, and is found in actual practice to be the most easy with which to operate. Then, according to the definition,

$$\begin{aligned}
\chi(1)&=x_2x_3,\\
\chi(2)&=x_3x_4,\\
&\cdots\\
\chi(n-1)&=x_nx_1,\\
\chi(n)\ \ &=x_1x_2=\chi(0),\\
&\cdots
\end{aligned}$$

Or again, if we suppose $n=5$, and

$$\chi(0)=x_1^2(x_2x_3+x_4x_5),$$

then, following the same cycle, we have

$$\begin{aligned}
\chi(1)&=x_2^2(x_3x_4+x_5x_1),\\
\chi(2)&=x_3^2(x_4x_5+x_1x_2),\\
\chi(3)&=x_4^2(x_5x_1+x_2x_3),\\
\chi(4)&=x_5^2(x_1x_2+x_3x_4),\\
\chi(5)&=x_1^2(x_2x_3+x_4x_5)=\chi(0),\\
&\cdots
\end{aligned}$$

and so on.

Next, let $\Sigma_r\chi(0)$ or, more simply, $\Sigma\chi(0)$* represent the circular function

$$\chi(0)+\chi(1)+\ldots+\chi(n-2)+\chi(n-1).$$

Then, since each root recurs at every nth step in the cycle, we have

$$\begin{aligned}
\chi(n)\ \ &=\chi(0),\\
\chi(n+1)&=\chi(1),\\
&\cdots\\
\chi(n+q)&=\chi(q);
\end{aligned}$$

* When there is only one cycle involved in the operation, or when there is no comparison of cycles, it is not necessary to suffix Σ.

and therefore

$$\begin{aligned}\Sigma'\chi(1) &= \chi(1)+\chi(2)+\ldots+\chi(n-1)+\chi(n)\\ &= \{\chi(1)+\chi(2)+\ldots+\chi(n-1)\}+\chi(0)\\ &= \Sigma'\chi(0).\end{aligned}$$

Similarly,

$$\Sigma'\chi(2)=\Sigma'\chi(1)=\Sigma'\chi(0),$$

and by generalization and induction,

$$\Sigma'\chi(q)=\Sigma'\chi(0).$$

Hence

THEOREM I.—A circular function is not affected in value by the simultaneous advancing or receding of the roots which it contains any number of steps in the cycle to which it belongs.

Whence it follows that $\Sigma'\chi_1(0).\Sigma'\chi_2(0)$, or its equivalent,

$$\{\chi_1(0)+\chi_1(1)+\ldots+\chi_1(n-2)+\chi_1(n-1)\}.\Sigma'\chi_2(0),$$

may be written

$$\chi_1(0).\Sigma'\chi_2(0)+\chi_1(1).\Sigma'\chi_2(1)+\ldots+\chi_1(n-2).\Sigma'\chi_2(n-2)+\chi_1(n-1).\Sigma'\chi_2(n-1),$$

which is equal to

$$\Sigma'\{\chi_1(0).\Sigma\chi_2(0)\};$$

and by simply interchanging χ_1 and χ_2, we have also

$$\Sigma'\chi_1(0).\Sigma'\chi_2(0)=\Sigma'\{\chi_2(0).\Sigma'\chi_1(0)\}.$$

Which gives us

THEOREM II.—The product of two circular functions belonging to the same cycle is itself a circular function to that cycle, and is given by the application of the cyclical symbol to the product of either function into the initial or leading terms of the other.

It should be remarked that Σ' is a symbol of cyclical operation, and subject to the same laws, with certain obvious limitations as if it were a symbol of quantity. Thus

$$\Sigma'\{\chi_1(0)+\chi_2(0)+\&c.\}=\Sigma'\chi_1(0)+\Sigma'\chi_2(0)+\&c.;$$

and, in general, if we develope

$$\{\chi_1(0)+\chi_2(0)+\&c.\}^m$$

by the multinomial theorem, and then apply the symbol Σ' to each term, the result will be equal to

$$\Sigma'\{\chi_1(0)+\chi_2(0)+\&c.\}^m.$$

It should also be remarked that if C be a function or quantity such that it is not affected by the cyclical interchange of the roots, as when it is a constant quantity and therefore independent of the roots altogether, or as when it is a circular function and belongs to the same cycle as $\Sigma'\chi(0)$, then will

$$\Sigma'C\chi(0)=C\Sigma'\chi(0).$$

The foregoing theorems are true for any circular functions whatever, whether rational

or irrational, integral or fractional. But the following holds only for those circular functions which are rational and integral.

THEOREM III. If X be a circular function of the form

$$\Sigma'\{\chi_a+\chi_b+\chi_c+\&c.\},$$

χ being defined by

$$\chi_m=\mathfrak{A}'x_1^\alpha x_2^\beta x_3^\gamma\dots x_n^\xi+\mathfrak{A}''x_1^\beta x_2^\gamma x_3^\alpha\dots x_n^\xi+\mathfrak{A}'''\ \&c.,$$

where $\alpha, \beta, \gamma, \dots \xi$ are positive integers or (some of them) zero, and m is the number of values of $x_1^\alpha x_2^\beta x_3^\gamma\dots x_n^\xi$ or (what is the same thing) the number of terms contained in $\Sigma x_1^\alpha x_2^\beta x_3^\gamma\dots x_n^\xi$, then will

$$\Sigma X=nn'\left(\frac{1}{a}\Sigma\chi_a+\frac{1}{b}\Sigma\chi_b+\frac{1}{c}\Sigma\chi_c+\&c.\right),$$

n' being the number of values of X, and $\Sigma\chi_m$ being of the form

$$(\mathfrak{A}'+\mathfrak{A}''+\mathfrak{A}'''+\&c.)\Sigma x_1^\alpha x_2^\beta x_3^\gamma\dots x_n^\xi.$$

For if, as we are permitted, we fix one of the roots, and permute the remaining $n-1$ roots in all possible ways, there will arise $1.2.3\dots(n-1)$, or (say) p, corresponding cycles; and if, for the moment, we represent by $\dot\Sigma$ the sum of the p expressions formed by applying these cycles to any one of the values of X, then, since Σ' consists of n, and consequently $\dot\Sigma\Sigma'$ of np, expressions of the form

$$\chi_a+\chi_b+\chi_c+\&c.,$$

and since also $m=np$, or a submultiple of np, it follows that

$$\dot\Sigma X=\frac{np}{a}\Sigma\chi_a+\frac{np}{b}\Sigma\chi_b+\frac{np}{c}\Sigma\chi_c+\&c.$$

But $\dot\Sigma X=\frac{p}{n'}\Sigma X$. Whence the theorem.

9. The symbol Σ' admits of an easy extension to functions of the form

$$f^m(\varrho)=x_1^m+\varrho x_2^m+\varrho^2x_3^m+\dots+\varrho^{n-2}x_{n-1}^m+\varrho^{n-1}x_n^m\ *,$$

ϱ being an nth root of unity, real or imaginary. For, if we represent this function by $\Sigma_\varrho x^m$, we shall have

$$f^m(\varrho^q).f^p(\varrho^{n-q})=\Sigma'\{x^mf^p(\varrho^{n-q})\}=\Sigma'\{x^pf^m(\varrho^q)\},$$

or

$$\Sigma'_{\varrho^q}x^m.\Sigma'_{\varrho^{n-q}}x^p=\Sigma'(x^m\Sigma'_{\varrho^{n-q}}x^p)=\Sigma'(x^p\Sigma'_{\varrho^q}x^m),$$

where Σ' is the simple or ordinary cyclical function to the first cycle

$$\dots123\dots(n-1)123\dots(n-1)\dots$$

This may be proved thus:—

$$f^m(\varrho^q)=x_1^m+\varrho^qx_2^m+\varrho^{2q}x_3^m+\dots+\varrho^{q(n-2)}x_{n-1}^m+\varrho^{q(n-1)}x_n^m=\Sigma'_{\varrho^q}x^m,$$

and

$$f^p(\varrho^{n-q})=x_1^p+\varrho^{n-q}x_2^p+\varrho^{2n-2q}x_3^p+\dots+\varrho^{(n-q)(n-2)}x_{n-1}^p+\varrho^{(n-q)(n-1)}x_n^p=\Sigma'_{\varrho^{n-q}}x^p;$$

* The idea of extending Σ' to functions of this form was suggested to me by Mr. COCKLE, in a letter under date January 8, 1859.

whence, multiplying, bearing in mind that $\varepsilon^n=1$, and arranging the multiplicand as below,

$$\begin{aligned}
&x_1^p(x_1^m+\varepsilon^p x_2^m+\varepsilon^{2p}x_3^m+\ldots+\varepsilon^{p(n-2)}x_{n-1}^m+\varepsilon^{p(n-1)}x_n^m)\\
+&x_2^p(x_2^m+\varepsilon^p x_3^m+\varepsilon^{2p}x_4^m+\ldots+\varepsilon^{p(n-2)}x_n^m\ \ +\varepsilon^{p(n-1)}x_1^m)\\
+&x_3^p(x_3^m+\varepsilon^p x_4^m+\varepsilon^{2p}x_5^m+\ldots+\varepsilon^{p(n-2)}x_1^m\ \ +\varepsilon^{p(n-1)}x_2^m)\\
&\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\\
+&x_n^p(x_n^m+\varepsilon^p x_1^m+\varepsilon^{2p}x_2^m+\ldots+\varepsilon^{p(n-2)}x_{n-2}^m+\varepsilon^{p(n-1)}x_{n-1}^m),
\end{aligned}$$

we see that $f^m(\varepsilon^p).f^p(\varepsilon^{n-p})$, or

$$\begin{aligned}
\Sigma'_{\varepsilon^p}x^{mp}.\Sigma'_{\varepsilon^{n-p}}x^{np}&=\Sigma'\{x_1^p(x_1^m+\varepsilon^p x_2^m+\varepsilon^{2p}x_3^m+\ldots+\varepsilon^{p(n-2)}x_{n-1}^m+\varepsilon^{p(n-1)}x_n^m)\}\\
&=\Sigma'(x^p\Sigma'_{\varepsilon^p}x^{mp}),
\end{aligned}$$

which establishes one part of the theorem; and the other part is established by simply interchanging m and p.

10. In order to illustrate some of the preceding properties of Σ', let us take one of the factors

$$f(\alpha)=x_1+\alpha x_2+\alpha^2 x_3,$$

which enters into the symmetric product for cubics (art. 3). Then $\pi_3(x)$, or

$$\begin{aligned}
f(\alpha).f(\alpha^2)&=\Sigma'\{x_1(x_1+\alpha x_2+\alpha^2 x_3)\}\\
&=\Sigma x^2+(\alpha+\alpha^2)\Sigma' x_1x_2\\
&=\Sigma x^2-\Sigma x_1x_2.
\end{aligned}$$

Next, let us take the factor

$$f(\omega)=x_1+\omega x_2+\omega^2 x_3+\omega^3 x_4+\omega^4 x_5,$$

which enters into the resolvent product for quintics (art. 6). Then $\pi_4(x)$, or

$$\begin{aligned}
&\{f(\omega).f(\omega^4)\}\times\{(f(\omega^2).f(\omega^3)\}\\
&\quad=\Sigma'\{x_1(x_1+\omega x_2+\omega^2x_3+\omega^3x_4+\omega^4x_5)\}\times\\
&\quad\ \ \Sigma'\{x_1(x_1+\omega^2x_2+\omega^4x_3+\omega x_4+\omega^3x_5)\}\\
&\quad=\{\Sigma x^2+(\omega+\omega^4)\Sigma' x_2+(\omega^2+\omega^3)\Sigma' x_1x_2\}\times\\
&\quad\ \ \{\Sigma x^2+(\omega^2+\omega^3)\Sigma' x_1x_2+(\omega+\omega^4)\Sigma' x_1x_2\}\\
&\quad=(\Sigma x^2)^2-\Sigma x^2\Sigma x_1x_2-(\Sigma x_1x_2)^2+5\Sigma' x_1x_2\Sigma' x_1x_2,
\end{aligned}$$

which, by known relations among symmetric functions, may be readily put under the form exhibited at the foot of art. 6.

I may notice here that if, in place of $f(\omega)$, we had dealt with $f^m(\omega)$, that is,

$$x_1^m+\omega x_2^m+\omega^2x_3^m+\omega^3x_4^m+\omega^4x_5^m,$$

we should have been led to

$$f^m(\omega).f^m(\omega^4)=\Sigma x^{2m}+(\omega+\omega^4)\Sigma' x_1^mx_2^m+(\omega^2+\omega^3)\Sigma' x_1^mx_3^m,$$

and

$$f^m(\omega^2).f^m(\omega^3)=\Sigma x^{2m}+(\omega^2+\omega^3)\Sigma' x_1^m x_2^m+(\omega+\omega^4)\Sigma' x_1^m x_3^m;$$

and therefore to

$$f^m(\omega).f^m(\omega^2).f^m(\omega^3).f^m(\omega^4)=(\Sigma x^{2m})^2-\Sigma x^{2m}\Sigma x_1^m x_2^m-(\Sigma x_1^m x_2^m)^2+5\Sigma' x_1^m x_2^m\Sigma' x_1^m x_3^m,$$

which, when $m=1$, coincides (as it ought to do) with the above result.

11. The expansion of $\Sigma' x_1^m x_2^m \Sigma' x_1^m x_3^m$ may, by Theorems I. and II., be effected thus,—

$$\begin{aligned}\Sigma' x_1^m x_2^m \Sigma' x_1^m x_3^m &= \Sigma' x_1^m x_2^m (x_1^m x_3^m + x_2^m x_4^m + x_3^m x_5^m + x_4^m x_1^m + x_5^m x_2^m)\\ &= \Sigma' (x_1^{2m} x_2^m x_3^m + x_1^m x_2^m x_4^m + x_1^m x_2^m x_3^m x_4^m + x_1^{2m} x_2^m x_5^m + x_1^m x_3^m x_5^m)\\ &= \Sigma' x_1^{2m}(x_2^m x_3^m + x_2^m x_4^m + x_3^m x_5^m + x_4^m x_5^m) + \Sigma x_1^m x_2^m x_3^m x_4^m;\end{aligned}$$

or since

$$\Sigma' x_1^{2m}(x_2^m x_3^m + x_2^m x_4^m + x_3^m x_5^m + x_4^m x_5^m + x_3^m x_4^m + x_4^m x_5^m) = \Sigma x_1^{2m} x_2^m x_3^m,$$

$$\therefore\quad \Sigma' x_1^m x_2^m \Sigma' x_1^m x_3^m = \Sigma x_1^{2m} x_2^m x_3^m + \Sigma x_1^m x_2^m x_3^m x_4^m - \Sigma' x_1^{2m}(x_2^m x_4^m + x_3^m x_4^m).$$

Consequently

$$\begin{aligned}f^m(\omega).f^m(\omega^2).f^m(\omega^3).f^m(\omega^4) &= (\Sigma x^{2m})^2 - \Sigma x^{2m}\Sigma x_1^m x_2^m\\ &\quad + 5\Sigma x_1^{2m} x_2^m x_3^m - (\Sigma x_1^m x_2^m)^2 + 5\Sigma x_1^m x_2^m x_3^m x_4^m - 5\Sigma' x_1^{2m}(x_2^m x_3^m + x_2^m x_4^m);\end{aligned}$$

which, making $m=1$, gives the resolvent product for quintics $\pi_4(x)$

$$=(\Sigma x^2)^2 - \Sigma x^2 \Sigma x_1 x_2 + 5\Sigma x_1^2 x_2 x_3 - (\Sigma x_1 x_2)^2 + 5\Sigma x_1 x_2 x_3 x_4 - 5\Sigma' x_1^2 (x_2 x_3 + x_2 x_4);$$

or, expressing the symmetric portion of this value in terms of the coefficients of the quintic, and writing λ for the unsymmetric portion $\Sigma' x_1^2(x_2 x_3 + x_2 x_4)$, we have

$$\pi_4(x) = \tfrac{1}{a^4}(-15a^3e + 5a^2bd + 5a^2c^2 - 5ab^2c + b^4) - 5\lambda.$$

I remark also that

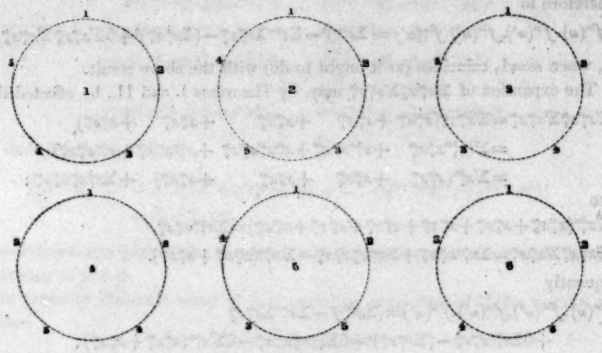
$$\tau\tau' = \Sigma' x_1 x_2 \Sigma' x_1 x_3 = \Sigma x_1^2 x_2 x_3 + \Sigma x_1 x_2 x_3 x_4 - \lambda = \tfrac{1}{a^2}(-3ae + bd) - \lambda.$$

12. The number of unequal values of the several functions τ, τ', $\tau\tau'$, λ and $\pi_4(x)$ may be determined by the following considerations. Since the coefficients of the several terms in the expression for τ are equal, we may, in forming its values, regard one of the roots as fixed, while the others are permuted *inter se*. Thus we shall have $1.2.3.4$ cycles, giving rise to 24 corresponding expressions for τ; but since, for the first cycle,

$$\tau = \Sigma' x_1 x_2 = \Sigma' x_1 x_3,$$

and similar relations obtain for the other cycles, therefore these 24 expressions may be grouped in pairs, the members of each pair being equal. Hence τ has only 12 values. In the same way it may be shown that τ' has only 12 values. And since the several values of τ' may be referred to the same cycles which arise in the formation of the values of τ, and since also τ and τ' are complementary to each other (for their sum $=\Sigma x_1 x_2$, a one-valued function), it follows that $\tau\tau'$, and therefore also λ and $\pi_4(x)$, are six-valued functions. An independent proof that the resolvent product $\pi_4(x)$ is six-valued may be found in my original memoir, Section II., art. 15.

13. Fixing two of the roots, x_1 and x_2, and permuting the others, the following cycles arise, viz. :—



Six values of r will then be included in the formula

$$\Sigma'_{1 \text{ to } 6}\, x_1 x_2,$$

and the other values, which are in fact the corresponding values of r'_1 will be included in the formulæ,

$$\Sigma'_{1,\,2,\,3,\,5}\, x_1 x_3 \text{ and } \Sigma'_{4,\,6}\, x_1 x_r.$$

The values of λ are as follows:—

$$\lambda_1 = \Sigma'_1 x_1^3 (x_3 x_5 + x_4 x_6), \qquad \lambda_4 = \Sigma'_4 x_1^3 (x_2 x_3 + x_4 x_5),$$
$$\lambda_2 = \Sigma'_2 x_1^3 (x_3 x_4 + x_5 x_6), \qquad \lambda_5 = \Sigma'_5 x_1^3 (x_2 x_4 + x_5 x_6),$$
$$\lambda_3 = \Sigma'_3 x_1^3 (x_2 x_5 + x_4 x_6), \qquad \lambda_6 = \Sigma'_6 x_1^3 (x_2 x_3 + x_4 x_5).$$

And by Theorem III., art. 9,

$$\Sigma\lambda = \frac{5.6}{30}(1+1)\Sigma x_1^3 x_2 x_3 = 2\Sigma x_1^3 x_2 x_3,$$

a result which may be verified by actual development and summation. By Theorem II.

$$\lambda_1^3 = \Sigma'_1 \{x_1^3 (x_3 x_5 + x_4 x_6) \Sigma'_1 x_1^3 (x_2 x_5 + x_4 x_6)\}$$
$$= \Sigma'_1 [\, x_1^3 (x_3 x_5 + x_4 x_6) \{x_1^3 (x_2 x_5 + x_4 x_6) + x_3^3 (x_2 x_1 + x_4 x_6)$$
$$+ x_5^3 (x_4 x_2 + x_6 x_1) + x_4^3 (x_6 x_2 + x_1 x_3) + x_6^3 (x_1 x_4 + x_2 x_3)\}\,];$$

or, multiplying out and reducing by the aid of Theorem I.*, we have

$$\lambda_1^3 = \Sigma'_1 \{(x_1^3 x_2^3 x_3^3 \quad + x_1^3 x_2^3 x_4^3) + 2(x_1^3 x_2^3 x_3 x_5 + x_1^3 x_2^3 x_4 x_6)$$
$$+ 2(x_1^3 x_2^3 x_3^2 x_4 + x_1^3 x_2^3 x_4^2 x_5 + \quad x_1^3 x_2^3 x_3^2 x_5 + x_1^3 x_2^3 x_4^2 x_6)\}$$
$$+ 2\Sigma x_1^3 x_2 x_3 x_4 x_5 + 2\Sigma x_1^3 x_2^2 x_3^2 x_4 x_5;$$

* In effecting reductions by the cyclical process, the greatest exponent is made the leading exponent.

and, therefore, by Theorem III.,

$$\Sigma\lambda^3 = 2\Sigma x_1^4 x_2^3 x_3^3 + 12\Sigma x_1^4 x_2^2 x_3 x_4 x_5 + 4\Sigma x_1^3 x_2^3 x_3^2 x_4 x_5 + 4\Sigma x_1^3 x_2^2 x_3^2 x_4^2 x_5 + 12\Sigma x_1^3 x_2^2 x_3^2 x_4 x_5.$$

Operating in the same way upon λ^3, we are led to the corresponding value of λ^3, and thence to $\Sigma\lambda^3$. Repeating the operation upon λ^3, we find $\Sigma\lambda^4$; and so on. But it may be remarked here that in dealing with the higher dimensioned functions, it will be found convenient to drop the subject x and to work only with exponents. Thus, following the first cycle and omitting, for convenience, the unit-suffix, we have

$$\begin{aligned}\lambda = \Sigma'\{(21001)+(20110)\} &= (21001)+(20110)\\ &\quad +(12100)+(02011)\\ &\quad +(01210)+(10201)\\ &\quad +(00121)+(11020)\\ &\quad +(10012)+(01102),\end{aligned}$$

and

$$\begin{aligned}\lambda = \Sigma'[&\{(42002)+(40220)\}+2\{(33101)+(30311)\}\\ &+2\{(32210)+(32021)+(31202)+(30122)\}]\\ &+8\Sigma 42^4+4\Sigma 3^2 21+2\Sigma 3^2 2^2+2\{\Sigma 41^4+\Sigma 2^2 1^2\}\lambda.\end{aligned}$$

And the passage to $\Sigma\lambda^3$ is easily effected. Carrying forward the calculation and collecting results, we have*

$$\begin{aligned}\Sigma\lambda &= 2\Sigma 21^3,\\ \Sigma\lambda^2 &= 2\Sigma 42^2 + 12\Sigma 41^4 + 4\Sigma 3^2 1^3 + 4\Sigma 32^2 1 + 12\Sigma 2^2 1^3,\\ \Sigma\lambda^3 &= 2\Sigma 63^3 + 2\Sigma 62^2 1^2 + 3\Sigma 5421 + 6\Sigma 532^2\\ &\quad + 8\Sigma 5321^3 + 6\Sigma 4^2 31 + 12\Sigma 4^2 21^2 + 12\Sigma 43^2 2\\ &\quad + 8\Sigma 43^2 1^3 + 6\Sigma 432^2 1 + 48\Sigma 42^4 + 24\Sigma 3^2 21\\ &\quad + 12\Sigma 3^2 2^3 + 2\{\Sigma 41^4 + \Sigma 2^2 1^3\}\Sigma\lambda,\\ \Sigma\lambda^4 &= 2\Sigma 84^2 + 4\Sigma 83^2 1^2 + 12\Sigma 82^4 + 4\Sigma 7531\\ &\quad + 4\Sigma 7432 + 16\Sigma 742^2 1 + 16\Sigma 73^2 21\\ &\quad + 12\Sigma 6^2 2^2 + 4\Sigma 6541 + 20\Sigma 6531^2\\ &\quad + 24\Sigma 652^2 1 + 12\Sigma 64^2 2 + 20\Sigma 64^2 1^2 + 24\Sigma 643^2\\ &\quad + 28\Sigma 64321 + 52\Sigma 63^2 2^2 + 24\Sigma 5^2 42 + 24\Sigma 5^2 41^2\\ &\quad + 48\Sigma 5^2 3^2 + 16\Sigma 5^2 321 + 72\Sigma 5^2 2^2 + 44\Sigma 54^2 21\end{aligned}$$

Thus if, in the expression $\Sigma' x_1^\alpha x_2^\beta x_3^\gamma x_4^\delta x_5^\varepsilon$, we suppose (*ex. gr.*) that, of the five exponents, γ is the greatest, then this function must be replaced by its equivalent $\Sigma' x_1^\gamma x_2^\delta x_3^\varepsilon x_4^\alpha x_5^\beta$. Or, suppose that the greatest exponent (γ) is repeated, and that the function takes the form $\Sigma' x_1^\alpha x_2^\beta x_3^\gamma x_4^\delta x_5^\varepsilon$; then this must be replaced by $\Sigma' x_1^\gamma x_2^\delta x_3^\varepsilon x_4^\alpha x_5^\beta$; and so on. Following this method, the comparison of similar functions is greatly facilitated.

* Many of the details of calculation are given in the third section of my original memoir; but the results there exhibited belong to the quintic wanting in its second, third, and fifth term. The results exhibited in the text belong to the perfect form.

$$
\begin{aligned}
&+ 68\Sigma 543^{2}1 + 48\Sigma 5432^{2} + 36\Sigma 53^{2}2 + 144\Sigma 4^{4}\\
&+ 24\Sigma 4^{3}31 + 72\Sigma 4^{2}2^{2} + 56\Sigma 4^{3}2^{2} + 24\Sigma 43^{4}\\
&+ \{8\Sigma 42^{4} + 4\Sigma 3^{2}21 + 2\Sigma 3^{2}2^{2}\}\Sigma\lambda + 2\{\Sigma 41^{4} + \Sigma 2^{2}1^{4}\}\Sigma\lambda^{2},\\
\Sigma\lambda^{3} = {}& 2\Sigma\overline{10}5^{3} + 6\Sigma\overline{10}4^{2}1^{2} + 4\Sigma\overline{10}3^{2}2^{2} + 5\Sigma 9641\\
&+ 5\Sigma 9542 + 14\Sigma 95321 + 28\Sigma 943^{2}1 + 32\Sigma 9432^{2}\\
&+ 10\Sigma 8732 + 5\Sigma 8651 + 44\Sigma 86321 + 36\Sigma 85^{2}1^{2}\\
&+ 30\Sigma 8543 + 39\Sigma 85421 + 96\Sigma 8532^{2} + 96\Sigma 84^{3}31\\
&+ 96\Sigma 84^{2}2^{2} + 20\Sigma 843^{2}2 + 360\Sigma 83^{4} + 72\Sigma 7^{3}41^{2}\\
&+ 180\Sigma 7^{2}2^{3} + 30\Sigma 7652 + 50\Sigma 7643 + 54\Sigma 76421\\
&+ 136\Sigma 763^{2}1 + 48\Sigma 7632^{2} + 136\Sigma 75^{2}21 + 60\Sigma 754^{2}\\
&+ 118\Sigma 75431 + 76\Sigma 7542^{2} + 164\Sigma 753^{2}2 + 36\Sigma 74^{2}1\\
&+ 232\Sigma 74^{3}2 + 48\Sigma 743^{3} + 120\Sigma 6^{2}1^{3} + 60\Sigma 6^{2}53\\
&+ 96\Sigma 6^{2}521 + 92\Sigma 6^{2}431 + 336\Sigma 6^{2}42^{2} + 132\Sigma 6^{2}3^{2}2\\
&+ 72\Sigma 65^{2}4 + 164\Sigma 65^{2}31 + 128\Sigma 65^{2}2^{2} + 188\Sigma 654^{2}1\\
&+ 172\Sigma 65432 + 276\Sigma 653^{2} + 120\Sigma 64^{2}2 + 224\Sigma 64^{3}2\\
&+ 108\Sigma 5^{2}41 + 348\Sigma 5^{2}32 + 184\Sigma 5^{2}4^{2}2 + 104\Sigma 5^{2}43^{2}\\
&+ 144\Sigma 54^{3}3 + \{2\Sigma 82^{4} + 4\Sigma 63^{2}2^{2} + 12\Sigma 5^{2}2^{2}\\
&+ 6\Sigma 543^{2}1 + 6\Sigma 5432^{2} + 6\Sigma 53^{2}2 + 4\Sigma 4^{3}31\\
&+ 12\Sigma 4^{2}2^{3} + 8\Sigma 4^{3}2^{2} + 4\Sigma 43^{4}\}\Sigma\lambda\\
&+ \{8\Sigma 42^{4} + 4\Sigma 3^{2}21 + 2\Sigma 3^{2}2^{2}\}\Sigma\lambda^{2}\\
&+ 2\{\Sigma 41^{4} + \Sigma 2^{2}1^{4}\}\Sigma\lambda^{3},\\
\Sigma\lambda^{4} = {}& 2\Sigma\overline{12}6^{2} + 8\Sigma\overline{12}5^{2}1^{2} + 10\Sigma\overline{12}4^{2}2^{2} + 24\Sigma\overline{12}3^{4}\\
&+ 6\Sigma\overline{11}751 + 6\Sigma\overline{11}652 + 22\Sigma\overline{11}6421 + 22\Sigma\overline{11}5431\\
&+ 64\Sigma\overline{11}53^{2}2 + 64\Sigma\overline{11}4^{3}32 + 15\Sigma\overline{10}842 + 6\Sigma\overline{10}76^{2}\\
&+ 30\Sigma\overline{10}7421 + 96\Sigma\overline{10}73^{2}1 + 56\Sigma\overline{10}6^{2}1^{3} + 30\Sigma\overline{10}653\\
&+ 24\Sigma\overline{10}6521 + 30\Sigma\overline{10}64^{2}2 + 128\Sigma\overline{10}642^{2} + 192\Sigma\overline{10}63^{2}2\\
&+ 96\Sigma\overline{10}5^{2}31 + 208\Sigma\overline{10}5^{2}2^{2} + 192\Sigma\overline{10}54^{2}1 + 158\Sigma\overline{10}5432\\
&+ 264\Sigma\overline{10}4^{3}3^{2} + 40\Sigma 9^{3}3^{3} + 56\Sigma 9851^{3} + 52\Sigma 98421\\
&+ 60\Sigma 9832^{2} + 30\Sigma 9762 + 30\Sigma 9753 + 104\Sigma 97521\\
&+ 120\Sigma 974^{3} + 82\Sigma 97431 + 136\Sigma 9742^{2} + 252\Sigma 973^{2}2\\
&+ 40\Sigma 96^{3}3 + 48\Sigma 96^{2}21 + 336\Sigma 96531 + 340\Sigma 9652^{2}\\
&+ 488\Sigma 964^{2}1 + 270\Sigma 96432 + 288\Sigma 963^{3} + 360\Sigma 95^{2}\\
&+ 16\Sigma 95^{4}41 + 284\Sigma 95^{3}32 + 680\Sigma 954^{2}2 + 584\Sigma 9543^{2}\\
&+ 168\Sigma 94^{3}3 + 30\Sigma 8^{2}62 + 120\Sigma 8^{2}53 + 320\Sigma 8^{2}431
\end{aligned}
$$

$+120\Sigma 8^{2}42^{1}$ $+192\Sigma 8^{2}3^{2}2$ $+20\Sigma 87^{2}1^{3}$ $+236\Sigma 87621$

$+120\Sigma 8754$ $+288\Sigma 87531$ $+380\Sigma 8752^{1}$ $+240\Sigma 874^{2}1$

$+486\Sigma 87432$ $+348\Sigma 873^{2}$ $+372\Sigma 86^{2}4$ $+304\Sigma 86^{3}31$

$+200\Sigma 86^{2}2^{2}$ $+364\Sigma 86541$ $+702\Sigma 86532$ $+646\Sigma 864^{2}2$

$+804\Sigma 8643^{2}$ $+1188\Sigma 85^{2}1$ $+684\Sigma 85^{2}42$ $+844\Sigma 85^{2}3^{2}$

$+488\Sigma 854^{2}3$ $+1920\Sigma 84^{4}$ $+360\Sigma 7^{2}3$ $+400\Sigma 7^{2}631$

$+552\Sigma 7^{2}62^{2}$ $+264\Sigma 7^{2}5^{2}$ $+776\Sigma 7^{2}541$ $+565\Sigma 7^{2}532$

$+480\Sigma 7^{2}4^{2}2$ $+1024\Sigma 7^{2}43^{2}$ $+264\Sigma 76^{2}5$ $+564\Sigma 76^{3}41$

$+707\Sigma 76^{2}32$ $+332\Sigma 765^{2}1$ $+942\Sigma 76542$ $+740\Sigma 7653^{2}$

$+660\Sigma 764^{2}3$ $+504\Sigma 75^{2}2$ $+936\Sigma 75^{2}43$ $+420\Sigma 754^{2}$

$+744\Sigma 6^{2}51$ $+444\Sigma 6^{2}42$ $+1380\Sigma 6^{2}3^{2}$ $+768\Sigma 6^{2}5^{2}2$

$+476\Sigma 6^{2}543$ $+1092\Sigma 6^{2}4^{2}$ $+444\Sigma 65^{2}3$ $+228\Sigma 65^{2}4^{2}$

$+168\Sigma 5^{2}4$ $+\{60\Sigma 83^{4}$ $+30\Sigma 7^{2}2^{2}$ $+6\Sigma 753^{2}2$

$+6\Sigma 74^{2}1$ $+14\Sigma 74^{2}32$ $+8\Sigma 743^{2}$ $+20\Sigma 6^{2}1^{3}$ $+48\Sigma 6^{2}42^{2}$

$+6\Sigma 6^{2}3^{2}2$ $+6\Sigma 65^{2}31$ $+12\Sigma 65^{2}2^{2}+12\Sigma 654^{2}1$ $+10\Sigma 65432$

$+46\Sigma 653^{2}$ $+20\Sigma 64^{2}2$ $+32\Sigma 64^{2}3^{2}$ $+18\Sigma 5^{2}41$ $+58\Sigma 5^{2}32$

$+26\Sigma 5^{2}4^{2}2$ $+14\Sigma 5^{2}43^{2}$ $+24\Sigma 54^{2}3\}\Sigma\lambda$ $+\{2\Sigma 82^{4}$

$+4\Sigma 63^{2}2^{2}$ $+12\Sigma 5^{2}2^{2}$ $+6\Sigma 543^{2}1$ $+6\Sigma 5432^{2}$ $+6\Sigma 53^{2}2$

$+4\Sigma 4^{2}31$ $+12\Sigma 4^{2}2^{2}$ $+8\Sigma 4^{2}3^{2}2$ $+4\Sigma 43^{4}\}\Sigma\lambda^{2}+\{8\Sigma 42^{4}$

$+4\Sigma 3^{2}21$ $+2\Sigma 3^{2}2^{2}\}\Sigma\lambda^{3}+2\{\Sigma 41^{4}$ $+\Sigma 2^{2}1^{2}\}\Sigma\lambda^{4}$.

The bar placed over a number consisting of two digits indicates that it is a single exponent. *Ex. gr.* $\Sigma\overline{12}6^{2}$ represents $\Sigma x_1^{12}x_2^6x_3^2$, $\Sigma\overline{11}751$ represents $\Sigma x_1^{11}x_2^7x_3^5x_4$, and so on.

14. We may regard λ as the root of a sextic equation; and since the coefficients of any equation may be considered as known, when the sums of the powers of its roots are known, therefore the formulæ in the last article may be considered as giving implicitly the coefficients of the sextic in λ. The equation in λ being once obtained, we may, by a transformation linear in λ, deduce the sextic in θ or in $\pi\pi'$. But the high dimensions of the symmetric functions in x present practical (almost insuperable) difficulties in passing to the corresponding functions of the coefficients of the quintic. Mr. CAYLEY has suggested to me a more convenient method, which, however, I must leave its author to expound himself.

15. But if, in place of the complete quintic, we take any one of the trinomial forms

$$x^5+ex+f=0,$$
$$x^5+dx^3+f=0,$$
$$x^5+cx^3+f=0,$$
$$x^5+bx^4+f=0,$$

to which, by the method of Mr. JERRARD, Mr. COCKLE, or Professor SYLVESTER, the complete quintic may be reduced, then the foregoing formulæ may be made available. For, any symmetric function of the roots may be resolved into a function of the sums of the powers of the roots, and these sums, in any of the above cases, can of course be easily calculated. Thus, taking the second form,

$$x^5+dx^3+f=0,$$

which in many respects is the most convenient, we readily find

$$\Sigma 1=0,\ \Sigma 2=0,\ \Sigma 3=-3d,\ \Sigma 4=0,$$

and therefore

$$\Sigma\lambda=2\Sigma 21^3=(\Sigma 1)^3\Sigma 2-2\Sigma 1\Sigma 3-(\Sigma 2)^3+2\Sigma 4=0.$$

In like manner we obtain

$$\Sigma\lambda^2=4.5^2df,$$
$$\Sigma\lambda^3=6d^4,$$
$$\Sigma\lambda^4=4.5^4d^2f^2,$$
$$\Sigma\lambda^5=158.5d^7f+5^6f^4,$$
$$\Sigma\lambda^6=6d^6+4.5^5d^3f^3.$$

Therefore the equation in λ is

$$\lambda^6-2.5^2df\lambda^4-2d^4\lambda^3+5^4d^2f^2\lambda^2-(58d^5+5^5f^3)f\lambda+d^6=0;$$

and since, in this case (art. 11),

$$\pi_4(x)=-5\lambda,$$

the corresponding equation for the resolvent product is

$$\theta^6-2.5^4df\theta^4+2.5^5d^4\theta^3+5^6d^2f^2\theta^2+5^4(58d^5+5^5f^3)f\theta+5^8d^6=0,$$

where I have written θ for $\pi_4(x)$. This equation was first given by Mr. COCKLE. See his paper entitled "Researches in the Higher Algebra," printed in the second part of the fifteenth volume of the 'Manchester Memoirs.' Some interesting and curious transformations of the equation may be found in the same paper.

SECTION III.

The Symmetric Product for Quintics.

16. We know that, for the perfect form (art. 11),

$$a^4\theta=-15a^2e+5a^3bd+5a^2c^2-5ab^2c+b^4-5a^2\lambda.$$

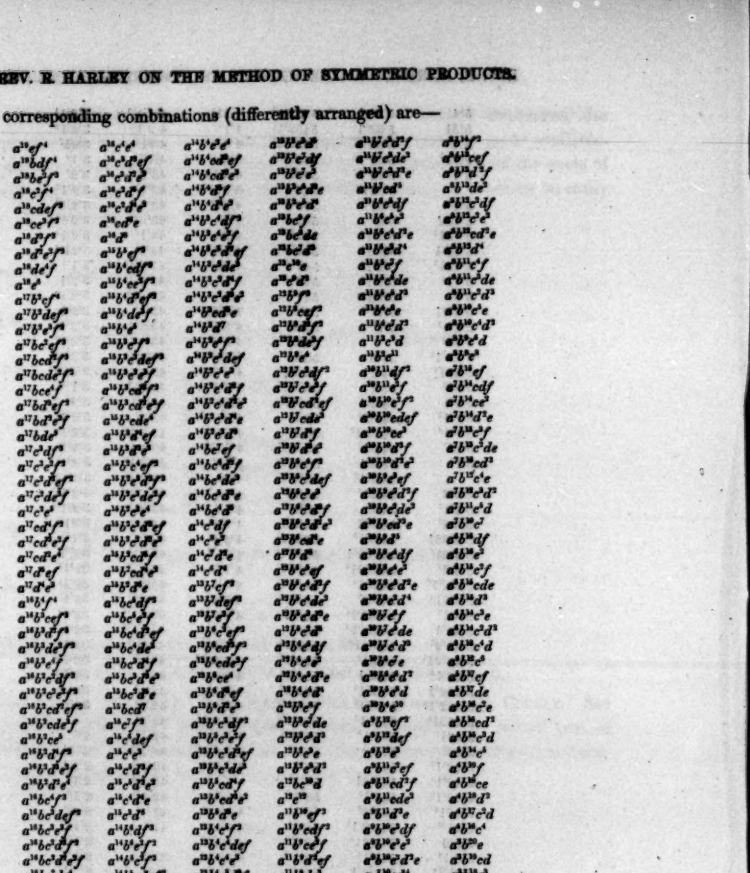
And by definition (art. 1),

$$\Pi(x)=\theta_1\theta_2\theta_3\theta_4\theta_5\theta_6.$$

It hence appears that the symmetric product Π is of twenty-four dimensions with respect to x. The partitions of twenty-four, for the quintic, are as follows:—

5^4	$5^3 3^1$	$543^2 1^4$	$4^2 1^2$	$4^2 2^1 1^2$	$3^2 2^1 1^2$
$5^3 1$	$5^3 2^2 1$	$543^2 1^2$	$4^3 1^2$	$4^2 2^1 1^2$	$3^2 2^1 1^2$
$5^3 2^2$	$5^3 2^2 1^2$	$543^2 1^2$	$4^3 2^2$	$4^2 21^4$	$3^2 21^2$
$5^2 21^2$	$5^2 3^2 1^2$	$5432 1^3$	$4^3 2^1 1^2$	$4^2 1^6$	$3^2 1^2$
$5^2 1^4$	$5^2 3^2 1^2$	5431^3	$4^3 2^2 1$	$43^2 2$	$3^2 2^2$
	$5^2 321^2$	$542^2 1$	$4^3 21^2$	$43^2 1^2$	$3^2 2^1 1^2$
$5^4 1^1$	$5^2 31^3$	$542^2 1^2$	$4^3 1^4$	$43^2 2^1 1$	$3^2 2^1 1^4$
$5^4 32$	$5^2 2^2$	$542^2 1^2$	$4^2 2^2$	$43^2 21^2$	$3^2 2^1 1^2$
$5^3 431$	$5^2 2^1 1^2$	$542^2 1^2$	$4^2 2^1 1^2$	$43^2 1^2$	$3^2 2^1 1^2$
$5^3 42^2 1$	$5^2 2^1 1^4$	$542^2 1^2$	$4^2 2^1 1^4$	$43^2 2^4$	$3^2 2^1 1^2$
$5^3 42 1^2$	$5^2 2^1 1^2$	$542^2 1^{11}$	$4^2 21^2$	$43^2 2^1 1^2$	$3^2 1^{12}$
$5^3 41^2$	$5^2 2^1 1^2$	5421^{12}	$4^2 1^2$	$43^2 2^1 1^4$	$3^2 2^2 1$
$5^3 3^2$	$5^2 2^1 1^{10}$	541^{12}	$4^3 2^2$	$43^2 21^2$	$3^2 2^1 1^2$
$5^3 3^2 21$	$5^2 21^{12}$	$53^2 1$	$4^3 2^1 21$	$43^2 1^2$	$3^2 2^1 1^4$
$5^3 3^1 1^2$	$5^2 1^{14}$	$53^2 2^2$	$4^3 2^1 1^2$	$43^2 2^1 1$	$3^2 2^1 1^7$
$5^3 32^2$		$53^2 21^2$	$4^3 2^2 1^2$	$43^2 2^1 1^2$	$3^2 2^1 1^2$
$5^3 32^2 1^2$	$54^2 3$	$53^2 1^4$	$4^3 2^2 1^2$	$43^2 2^1 1^2$	$3^2 2^1 1^{11}$
$5^3 291^4$	$54^2 21$	$53^2 2^2 1$	$4^3 2^1 21^4$	$43^2 2^1 1^7$	$3^2 21^{13}$
$5^3 31^2$	$54^2 1^3$	$53^2 2^1 1^2$	$4^3 2^1 1^2$	$43^2 21^2$	$3^2 1^{15}$
$5^2 2^1 1$	$54^2 3^1 1$	$53^2 21^4$	$4^3 32^1 1$	$43^2 1^{11}$	$3^2 2^2$
$5^2 2^1 1^2$	$54^2 32^2$	$53^2 1^7$	$4^3 32^1 1^2$	$43^2 2^7$	$3^2 2^1 1^2$
$5^2 2^1 1^3$	$54^2 321^2$	$53^2 2^3$	$4^3 32^1 1^3$	$43^2 2^1 1^2$	$3^2 2^1 1^4$
$5^2 21^7$	$54^2 31^4$	$53^2 2^1 1^2$	$4^3 321^7$	$43^2 2^1 1^4$	$3^2 2^1 1^6$
$5^2 1^9$	$54^2 2^2 1$	$53^2 2^1 1^4$	$4^3 31^7$	$43^2 2^1 1^6$	$3^2 2^1 1^2$
	$54^2 2^1 1^3$	$53^2 2^1 1^6$	$4^3 2^5$	$43^2 2^1 1^3$	$3^2 2^1 1^{10}$
$5^4 3^2$	$54^2 21^2$	$53^2 21^9$	$4^3 2^1 1^3$	$43^2 2^1 1^{10}$	$3^2 2^1 1^{12}$
$5^4 1^2$	$54^2 1^7$	$53^2 1^{10}$	$4^3 2^1 1^4$	$43^2 21^{12}$	$3^2 2^1 1^{14}$
$5^4 2^3$	$54^2 3^2$	$53^2 2^2 1$	$4^3 2^1 1^6$	$43^2 1^{14}$	$3^2 21^{16}$
$5^4 2^2 21$	$54^2 3^1 1^3$	$53^2 2^1 1^3$	$4^3 2^2 1^6$	$432^3 1$	$3^2 1^{18}$
$5^4 2^2 1^3$	$54^2 3^1 2^2 1$	$53^2 2^1 1^5$	$4^3 21^{10}$	$432^2 1^3$	$32^{10} 1$
$5^4 2^3 2^2$	$54^2 3^1 2^1 1^3$	$53^2 2^1 1^7$	$4^3 1^{13}$	$432^2 1^5$	$32^2 1^3$
$5^4 2^2 1^4$	$54^2 3^2 4^1$	$53^2 21^{11}$	$4^3 2^3 2^2$	$432^2 1^9$	$32^2 1^5$
$5^4 21^6$	$54^2 32^1 1^2$	$53^2 1^{13}$	$4^3 2^3 21^3$	$432^2 1^{11}$	$32^2 1^7$
$5^4 3^2 1$	$54^2 32^1 1^4$	532^5	$4^3 2^2 1^4$	$432^2 1^{13}$	$32^2 1^{11}$
$5^4 3^2 2^2$	$54^2 321^3$	$532^7 1^3$	$4^3 2^2 2^2 1$	4321^{15}	$32^2 1^{13}$
$5^4 3^2 21^2$	$54^2 31^6$	$532^6 1^4$	$4^3 2^2 2^1 1^2$	431^{17}	$32^2 1^{15}$
$5^4 3^2 1^4$	$54^2 2^2 1$	$532^4 1^6$	$4^3 2^2 1^4$	42^{10}	$32^2 1^{17}$
$5^4 32^2 1$	$54^2 2^2 1^3$	$532^4 1^8$	$4^3 2^1 1^7$	$42^2 1^2$	321^{19}
$5^4 32^2 1^3$	$54^2 2^2 1^4$	$532^3 1^{10}$	$4^3 2^3 2^3$	$42^2 1^4$	31^{21}
$5^4 321^3$	$54^2 2^2 1^7$	$532^3 1^{12}$	$4^3 2^2 1^2$	$42^2 1^6$	
$5^4 31^7$	$54^2 21^9$	5321^{14}	$4^3 2^2 1^4$	$42^2 1^8$	2^{12}
$5^4 2^5$	$54^2 1^{11}$	531^{16}	$4^3 2^2 1^6$	$42^2 1^{10}$	$2^{11} 1^2$
$5^4 2^2 1^3$	543^5	$52^6 1$	$4^3 2^1 21^3$	$42^2 1^{12}$	$2^{10} 1^4$
$5^4 2^2 1^4$	$543^4 21$	$52^5 1^3$	$4^3 2^1 1^{10}$	$42^2 1^{14}$	$2^9 1^6$
$5^4 2^2 1^6$	$543^4 1^3$	$52^4 1^5$	$4^2 32^2 1$	$42^2 1^{16}$	$2^8 1^8$
$5^4 21^8$	$543^3 2^2$	$52^6 1^7$	$4^2 32^2 1^3$	421^{18}	$2^7 1^{10}$
$5^4 1^{10}$	$543^3 2^1 1^2$	$52^4 1^9$	$4^2 32^2 1^5$	41^{20}	$2^6 1^{12}$
$5^3 3^2 2$	$543^2 21^4$	$52^4 1^{11}$	$4^2 32^2 1^7$		$2^5 1^{14}$
$5^3 3^1 1^2$	$543^2 1^6$	$52^4 1^{13}$	$4^2 32^2 1^9$	3^9	$2^4 1^{16}$
$5^3 3^2 2^1$	$543^2 2^1 1$	$52^3 1^{15}$	$4^2 321^{11}$	$3^7 2^1$	$2^3 1^{18}$
$5^3 3^2 21^3$	$543^2 2^1 1^3$	521^{17}	$4^2 31^{13}$	$3^7 1^2$	$2^2 1^{20}$
$5^3 3^1 1^4$	$543^2 2^1 1^5$	51^{19}	$4^2 2^5$	$3^6 2^3$	21^{22}
$5^3 2^2 1$	$543^2 21^7$		$4^2 2^3 1^2$	$3^6 2^1 1^2$	
$5^3 2^2 1^2$	$543^2 1^9$	4^9	$4^2 2^1 1^4$	$3^6 21^4$	1^{24}
$5^3 2^2 1^4$	5432^3	$4^8 31$	$4^2 2^1 1^6$	$3^6 1^6$	
$5^3 2^2 1^6$	$5432^2 1^2$	$4^7 2^2$	$4^2 2^1 1^8$	$3^5 2^2 1$	

And the corresponding combinations (differently arranged) are—



The direct calculation of Π would involve the prior calculation of the equation in λ. But by combining Eulerian with Lagrangian functions, and introducing the resolvent product, we may obtain the symmetric product without forming the sextic.

17. In order to facilitate operations it will be convenient to replace the coefficient of the first term by unity, and to suppose the quintic to be deprived of its second term; that is, in effect, to deal with the equation under the form

$$(1, 0, c, d, e, f)(x, 1)^5 = 0.$$

Then, if we assume

$$f(\omega^m) = x_0 + \omega^m x_1 + \omega^{2m} x_2 + \omega^{3m} x_3 + \omega^{4m} x_4 = 5\beta_m,$$

where, as in former articles, ω is an unreal fifth root of unity, the following relation will obtain, viz.

$$x_{r+1} = \omega^r \beta_1 + \omega^{2r} \beta_2 + \omega^{3r} \beta_3 + \omega^r \beta_4,$$

r being 0, 1, 2, 3, or 4 indifferently. Developing and reducing by the known properties of ω, we find

$$\begin{aligned}
\Sigma x^3 &= 10(\beta_1\beta_4 + \beta_2\beta_3),\\
\Sigma x^3 &= 15(\beta_1^2\beta_3 + \beta_2^2\beta_1 + \beta_3^2\beta_4 + \beta_4^2\beta_2),\\
\Sigma x^4 &= 30(\beta_1^2\beta_4^2 + \beta_2^2\beta_3^2) + 20(\beta_1^2\beta_2\beta_3 + \beta_2^2\beta_3\beta_4 + \beta_3^2\beta_4\beta_1 + \beta_4^2\beta_1\beta_2) + 120\beta_1\beta_2\beta_3\beta_4,\\
\Sigma x^5 &= 5\Sigma\beta^5 + 100(\beta_1^2\beta_2\beta_4 + \beta_2^2\beta_1\beta_3 + \beta_3^2\beta_2\beta_1 + \beta_4^2\beta_3\beta_2)\\
&\quad + 150(\beta_1^2\beta_2^2\beta_4 + \beta_2^2\beta_3^2\beta_3 + \beta_3^2\beta_4^2\beta_1 + \beta_4^2\beta_1^2\beta_2).
\end{aligned}$$

And by the method of the limiting equation or otherwise,

$$\Sigma x^3 = -2c, \quad \Sigma x^3 = -3d, \quad \Sigma x^4 = 2c^2 - 4e, \quad \Sigma x^5 = 5cd - 5f.$$

Whence by comparison with the above,

$$\begin{aligned}
-c &= 5(\beta_1\beta_4 + \beta_2\beta_3),\\
-d &= 5(\beta_1^2\beta_3 + \beta_2^2\beta_1 + \beta_3^2\beta_2 + \beta_4^2\beta_4),\\
-e &= 5(\beta_1^2\beta_3 + \beta_2^2\beta_4 + \beta_3^2\beta_3 + \beta_4^2\beta_1) + 3\beta_1\beta_2\beta_3\beta_4 - \tfrac{1}{5}c^2,\\
-f &= \Sigma\beta^5 - 10(\beta_1^3\beta_4\beta_4 + \beta_2^3\beta_1\beta_3 + \beta_3^3\beta_2\beta_1 + \beta_4^3\beta_3\beta_2) + \tfrac{1}{5}cd,\\
&= \Sigma\beta^5 + 10(\beta_1^3\beta_2^2\beta_4 + \beta_2^3\beta_3^2\beta_3 + \beta_3^3\beta_4^2\beta_1 + \beta_4^3\beta_1^2\beta_2) - \tfrac{1}{5}cd\text{*}.
\end{aligned}$$

18. If now we suppose one of the constituents, say β_4, to vanish, and eliminate the remaining ones, the effect will be the same as if we supposed the symmetric product, expressed as a function of the coefficients, to vanish. For

$$5^4\beta_1\beta_2\beta_3\beta_4\beta_4 = \theta;$$

* The formulæ here exhibited are Euler's, or rather Euler's as simplified by Mr. Cockle. They indicate the connexion between the coefficients of the quintic and the constituents of its roots. Those constituents, by Lagrange's process, are expressed as rational functions of the roots. It is to be observed that Euler's functions are cyclical. In fact, applying Σ' to the cycle

$$\ldots 12431243\ldots$$

the relations may be written thus:—

$$\begin{aligned}
-2c &= 5\Sigma'\beta_1\beta_2, \quad -d = 5\Sigma'\beta_1^2\beta_2, \quad -e = 5\Sigma'\beta_1^2\beta_3 + 3\beta_1\beta_2\beta_3\beta_4 - \tfrac{1}{5}c^2,\\
-f &= \Sigma\beta^5 - 10\Sigma'\beta_1^3\beta_2\beta_4 + \tfrac{1}{5}cd = \Sigma\beta^5 + 10\Sigma'\beta_1^3\beta_2\beta_4 - \tfrac{1}{5}cd.
\end{aligned}$$

And by the working properties of Σ',

$$cd = 5^3\Sigma'\beta_1^3\beta_2(\beta_1\beta_4 + \beta_2\beta_3) = 5^3\Sigma'(\beta_1^3\beta_2\beta_4 + \beta_1^3\beta_2^2\beta_3).$$

so that the evanescence of β involves the evanescence of θ, and consequently of Π. Making β_4 vanish, the equations at the foot of the last article become

$$-c=5\beta_1\beta_2,$$
$$-d=5(\beta_1^2\beta_3+\beta_1\beta_2^2),$$
$$-e=5(\beta_1^2\beta_3+\beta_1\beta_2^2)-\tfrac{1}{2}c^2,$$
$$-f=\beta_1^2+\beta_2^2+\beta_3^2+2\beta_1\beta_2 c+\tfrac{1}{2}cd.$$

And the elimination of β_1, β_2, β_3 gives

$$\Pi=k\times$$

$+1\ c^{10}$	$-65\ c^8d$	$f-10\ c^7$	f^2+625c^3d
$-16\ c^{10}e$	$+560\ c^8de$	$+125\ c^6e$	$-3125cde$
$+2\ c^9d^2$	$-85\ c^7d^2$	$+150\ c^5d^2$	
$+96\ c^8e^2$	$-1340\ c^6de^2$	$-500\ c^4e^2$	
$-9\ c^7d^2e$	$+510\ c^5d^2e$	$-750\ c^3d^2e$	
$-1\ c^6d^4$	$-70\ c^4d^3$	$-125\ cd^4$	
$-266\ c^6e^3$	$+875\ c^3de^3$	$+625\ ce^3$	
$-19\ c^5d^2e^2$	$-175\ cd^3e^2$	$+625\ d^2e^2$	
$+23\ c^4d^4e$	$+50\ d^4e$		
$+336\ c^4e^4$	$-250\ de^4$		
$-2\ c^3d^6$			
$-58\ c^3d^2e^3$			
$+14\ c^2d^4e^2$			
$-160\ c^2e^5$			
$-7\ cd^5e$			
$+35\ cd^3e^4$			
$+1\ d^6$			
$-10\ d^4e^3$			
$+25\ e^6$			

$f^3=0.$

In order to determine k (a constant numerical factor, dropped in the course of calculation), let us take the particular equation

$$x^5+cx^3=0,$$

for which we have (art. 11)

$$\theta=5c^3,$$

and consequently

$$\Pi=5^6c^{12}.$$

Then since, in this case, the coefficients d, e, f severally vanish, the foregoing formula gives

$$\Pi=kc^{12};$$

and therefore

$$k=5^6\text{*}.$$

* The symmetric product for the quintic, deprived of its second term,

$$x^5-5Px^3-5Qx^2-5Rx+E=0,$$

was first calculated by Mr. Cockle. See his paper "On Equations of the Fifth Degree," published in the Appendix to the Lady's and Gentleman's Diary for 1858. In the second section of my original memoir I have verified his result by an independent calculation and supplied the constant numerical multiplier. Mr. Cockle presented the product in the form of a function of P, Q, S and E, S being given by

$$S=P^2+R.$$

Following the notation of my friend, I gave the product in the same form. But the passage to the

19. We have thus obtained the symmetric product for the quintic wanting in its second term; but it seems desirable on many grounds to calculate the product for the perfect form. A variety of methods of performing this calculation might be suggested. The reversal of the problem of transformation appears, at first sight, the most easy and practicable, and if in the foregoing expression for Π the following substitutions be made, viz.—

$$-\frac{2b^2}{5}+c \text{ for } c,$$

$$\frac{4b^3}{5^2}-\frac{3bc}{5}+d \text{ for } d,$$

$$-\frac{3b^4}{5^3}+\frac{3b^2c}{5^2}-\frac{2bd}{5}+e \text{ for } e,$$

$$\frac{4b^5}{5^4}-\frac{b^3c}{5^3}+\frac{b^2d}{5^2}-\frac{be}{5}+f \text{ for } f,$$

the result will be the symmetric product for the complete quintic

$$(1, b, c, d, e, f)(x, 1)^5=0.$$

But it will be found on trial that this process, though apparently simple, does in point of fact involve prodigious labour. Mr. Samuel Bills of Hawton, who kindly undertook to assist me in the calculation, communicated to me in the early part of the present year that portion of the expression into which f^3 enters. But the difficulties of the calculation and the want of means of verifying successive results have led him to abandon the work as impracticable. An equally effective and a much more expeditious process is supplied by the following considerations.

20. It occurred to Mr. Cockle that the symmetric product for the perfect quintic

$$x^5-5Mx^4-5Px^3-5Qx^2-5Rx+E=0$$

---

corresponding expression in P, Q, R and E is easily effected, and I find that the result (as yet unpublished) of the transformation is—

$$\Pi=5^{14}\times$$

+ 625 P^{12}	+325 P^8Q	$E+2\ P^7$	$E^2+1\ P^3Q$
+2000 $P^{10}R$	+560 P^6QR	+5 P^4R	+1 PQR
− 250 P^8Q^2	− 85 P^4Q^3	+6 P^4Q^2	E^3
+2400 P^8R^2	+268 P^4QR^2	+4 P^3R^2	
− 225 P^6Q^2R	−102 P^2Q^3R	+6 P^2Q^2R	
− 25 P^6Q^4	+ 14 P^2Q^5	+1 PQ^4	
+1330 P^6R^3	+ 35 P^2QR^3	+1 PR^3	
+ 95 $P^4Q^3R^2$	− 7 PQ^3R^2	+1 Q^2R^3	
− 115 P^4Q^4R	+ 2 Q^5R		
+ 336 P^4R^4	+ 2 QR^4		
+ 10 P^4Q^6			
− 58 $P^2Q^5R^2$			
+ 14 $P^2Q^4R^3$			
+ 32 P^2R^5			
− 7 PQ^6R			
− 7 PQ^3R^4			
+ 1 Q^8			
+ 2 Q^4R^3			
+ 1 R^6			

is given by the expression

$$\pi - D\pi.M + D^2\pi.\frac{M^2}{1.2} - D^3\pi.\frac{M^3}{1.2.3} + \&c.,$$

where π is the symmetric product for the imperfect form treated of in the foot-note under paragraph 18, D is the differential symbol

$$\frac{d}{dx_1} + \frac{d}{dx_2} + \frac{d}{dx_3} + \frac{d}{dx_4} + \frac{d}{dx_5},$$

and the relations

$$DP = D(-\tfrac{1}{2}\Sigma x_1 x_2) = -\tfrac{4}{2}\Sigma x = -4M,$$

and others corresponding to them, hold. Mr. COCKLE had further noticed that $D\pi$ is to be regarded as free from M, a condition substantially equivalent to the expunging of the portion

$$5\partial_b + 4\partial\partial_e$$

of the operator ∇ which will be presently considered. Although he had not accurately completed this process of derivation, yet he had made a near approach to its completion, when Mr. CAYLEY, to whom as well as to myself Mr. COCKLE had communicated it, showed that the same results might be more immediately and conveniently obtained by means of the quantical calculus, and in so doing he incidentally corrected an oversight which I had already pointed out to Mr. COCKLE. In a letter under date September 28, 1859, Mr. CAYLEY called my attention to the circumstance that the several coefficients of the resolvent equation of the quintic are leading coefficients of a covariant. Mr. COCKLE had previously suggested that the symmetric product Π, or the last coefficient, was such a term. The test that a function of the roots may be such a term is that it is reduced to zero by the operation

$$\partial_{\alpha^1} + \partial_{\alpha^2} + \partial_{\alpha^3} + \partial_{\alpha^4} + \partial_{\alpha^5}.$$

It is clear that this is the case with respect to each factor of the product

$$\theta_1 = f(\omega).f(\omega^2).f(\omega^3).f(\omega^4) = 5^4\beta_1\beta_2\beta_3\beta_4.$$

Therefore it is also the case with the product itself; and since the like is true with respect to the other five values of θ, it is also true with respect to any symmetrical function of the six values. Consequently each coefficient of the sextic in θ is the leading coefficient of a covariant. At present, however, we have only to deal with the last coefficient, that is, the symmetric product.

21. Π is a seminvariant* reduced to zero by the operation

* The term "Seminvariant" is due to Mr. CAYLEY, who in a letter to me dated March 22, 1860, says, "The meaning is a function which is reduced to zero by one only of the operators which reduce to zero an invariant. It is in fact the leading coefficient of a covariant. It may also be defined as a function of the coefficients which is not altered by the substitution of $x+h$ for x." Defined as functions of the coefficients which are not altered by the substitution of $x+h$ for x, seminvariants are what Mr. COCKLE (who discussed such functions some years ago in the third and concluding volume of the 'Mathematician,' and more recently in some of the other journals referred to in the foot-note under the first paragraph of this paper) calls "critical functions." I may add that some years since Mr. COCKLE pointed out that the factors of the resolvent product, and, consequently, the product itself, are critical functions.

$$\nabla = 5\partial_a + 4b\partial_b + 3c\partial_c + 2d\partial_d + e\partial_f;$$

and if we write

$$\Pi = \Pi_0 + \Pi_1 b + \Pi_2 b^2 + \ldots + \Pi_t b^t,$$

Π_0 is known, being what Π becomes when the quintic is deprived of its second term, and $\Pi_1, \Pi_2, \ldots \Pi_t$ may be found from it by means of the formulæ

$$\Pi_1 = -\frac{1}{5}\nabla'\Pi_0,$$

$$\Pi_2 = -\frac{1}{2.5}(\nabla'\Pi_1 + 4\partial_c\Pi_0),$$

. . . . . .

$$\Pi_t = -\frac{1}{5t}(\nabla'\Pi_{t-1} + 4\partial_c\Pi_{t-2}),$$

where

$$\nabla' = \nabla - (5\partial_a + 4b\partial_b) = 3c\partial_c + 2d\partial_d + e\partial_f.$$

Assume

$$\Pi_0 = A_0 + B_0 f + C_0 f^2 + D_0 f^3,$$

and

$$\nabla'' = \nabla' - e\partial f = 3c\partial_c + 2d\partial_d;$$

then

$$\begin{aligned} -5\Pi_1 = \nabla'\Pi_0 = \quad & \nabla''A_0 + B_0 e \\ & + f\,(\nabla''B_0 + 2C_0 e) \\ & + f^2(\nabla''C_0 + 3D_0 e) \\ & + f^3\,\nabla''D_0; \end{aligned}$$

and if we write

$$\Pi_1 = A_1 + B_1 f + C_1 f^2 + D_1 f^3,$$

then will

$$\begin{aligned} -2.5\Pi_2 = \nabla'\Pi_1 + 4\partial_c\Pi_0 = \quad & \nabla''A_1 + B_1 e + 4\partial_c A_0 \\ & + f\,(\nabla''B_1 + 2C_1 e + 4\partial_c B_0) \\ & + f^2(\nabla''C_1 + 3D_1 e + 4\partial_c C_0) \\ & + f^3(\nabla''D_1 \quad\quad + 4\partial_c D_0); \end{aligned}$$

so, in general, if

$$\Pi_t = A_t + B_t f + C_t f^2 + D_t f^3,$$

we shall have

$$\begin{aligned} -5(t+1)\Pi_{t+1} = \nabla'\Pi_t + 4\partial_c\Pi_{t-1} = \quad & \nabla''A_t + B_t e + 4\partial_c A_{t-1} \\ & + f\,(\nabla''B_t + 2C_t e + 4\partial_c B_{t-1}) \\ & + f^2(\nabla''C_t + 3D_t e + 4\partial_c C_{t-1}) \\ & + f^3(\nabla''D_t + 4\partial_c D_{t-1} \quad\quad). \end{aligned}$$

These formulæ will enable us with comparative ease and great rapidity to derive the symmetric product for the complete quintic from that for the quintic wanting in its second term.

22. But it is noteworthy that the complete value of a seminvariant or critical function may always be deduced from the result obtained when *any* coefficient (excepting only a the first) vanishes. Thus, for the equation

$$(a, b, c, d, e, f)(x, 1)^5=0,$$

the operator is

$$\nabla=5a\partial_b+4b\partial_c+3c\partial_d+2d\partial_e+e\partial_f;$$

and if *ex. gr.* we write

$$\mathrm{I}=\mathrm{I}_0+\mathrm{I}_1 d+\mathrm{I}_2 d^2+\ldots$$

and suppose I_0 given, all the other terms can be found. For, in this case, let

$$\nabla'=\nabla-(3c\partial_d+2d\partial_e)=5a\partial_b+4b\partial_c+e\partial_f;$$

then

$$\nabla\mathrm{I}=\left\{\begin{array}{l} \nabla'\mathrm{I}_0 \quad +d\nabla'\mathrm{I}_1+d^2\nabla\mathrm{I}_2+\ldots \\ +3c(\mathrm{I}_1 \quad +2d\mathrm{I}_2+3d^2\mathrm{I}_3+\ldots) \\ +2\ (d\partial_c\mathrm{I}_0+d^2\partial_c\mathrm{I}_1 \quad +\ldots) \end{array}\right\}=0,$$

and therefore

$$3c\mathrm{I}_1=-\nabla'\mathrm{I}_0,$$

$$6c\mathrm{I}_2=-\nabla'\mathrm{I}_1-2\partial_c\mathrm{I}_0,$$

$$9c\mathrm{I}_3=-\nabla'\mathrm{I}_2-2\partial_c\mathrm{I}_1, \text{ \&c.},$$

which give I_1, I_2, I_3, &c. And it is easy to see how to obtain the corresponding formulæ for the other cases, viz. when the given function is free from either c, e, or f. The extension of the process to equations of other degrees than the fifth presents no difficulty.

23. In applying the formulæ in art. 21, it will be convenient to omit the constant numerical factor 5^6. The calculation may be thus conducted:—

Terms in Π_0.

A_0		B_0		C_0		D_0	
c^{12}	+ 1	c^9d	− 65	c^7	− 10	c^3d	+ 625
$c^{10}e$	− 16	c^8de	+ 560	c^5e	+ 125	cde	− 3125
c^8d^2	+ 2	c^7d^3	− 85	c^4d^2	+ 150		
c^6e^2	+ 96	c^6de^2	− 1340	c^3e^2	− 500		
c^7d^2e	− 9	c^5d^3e	+ 510	c^2d^2e	− 750		
c^6d^4	− 1	c^4d^5	− 70	cd^4	− 125		
c^5e^3	− 266	c^4de^3	+ 875	ce^3	+ 625		
$c^4d^3e^2$	− 19	cd^3e^2	− 175	d^2e^2	+ 625		
c^4d^5e	+ 23	d^5e	+ 50				
c^5e^4	+ 336	de^4	− 250				
c^3d^6	− 2						
$c^3d^4e^2$	− 58						
$c^2d^4e^3$	+ 14						
c^2e^5	− 160						
cd^5e	− 7						
cd^3e^4	+ 35						
d^5	+ 1						
d^4e^3	− 10						
e^6	+ 25						

Calculation of Π_1.

$\nabla''A_0+B_0e$	$3c\partial_d \; 2d\partial_e$ A_0	B_0	e	$=$	$-5A_1$	A_1
$c^{10}d$	+ 12	− 32		=	− 20	+ 4
c^9de	− 54	+ 384	− 65		+ 265	− 53
c^7d^3	− 12	− 18			− 30	+ 6
c^8de^2	−114	−1596	+ 560		−1150	+230
c^6d^3e	+276	− 76	− 85		+ 115	− 23
c^4d^5	− 36	+ 46			+ 10	− 2
c^7de^3	−348	+2688	−1340		+1000	−200
$c^5d^3e^2$	+168	− 348	+ 510		+ 330	− 66
c^3d^5e	−126	+ 56	− 70		− 140	+ 28
c^5de^4	+210	−1600	+ 875		− 515	+103
cd^7	+ 24	− 14			+ 10	− 2
cd^5e^2	−120	+ 280	− 175		− 15	+ 3
d^3e^3		− 60	+ 50		− 10	+ 2
de^5		+ 300	− 250		+ 50	− 10

$\nabla''B_0+2C_0e$	$3c\partial_d \; 2d\partial_e$ B_0	$2e$ C_0	$=$	$-5B_1$	B_1
c^9	− 195		=	− 195	+ 39
c^7e	+1680	− 20		+1660	−332
c^5d^2	− 765	+1120		+ 355	− 71
c^7e^2	−4020	+ 250		−3770	+754
c^5d^2e	+4590	−5360	+ 300	− 470	+ 94
c^3d^4	−1050	+1020		− 30	+ 6
c^5e^3	+2625	−1000		+1625	−325
$c^3d^2e^2$	−1575	+5250	−1500	+2175	−435
cd^4e	+ 750	− 700	− 250	− 200	+ 40
c^3e^4	− 750	+1250		+ 500	−100
d^6	+ 100			+ 100	− 20
d^4e^3		−2000	+1250	− 750	+150

$\nabla''C_0+3D_0e$	$3c\partial_d \; 2d\partial_e$ C_0	$3e$ D_0	$=$	$-5C_1$	C_1
c^6d	+ 900	+ 250	=	+1150	−230
c^4de	−4500	−2000	+1875	−4625	+925
c^2d^3	−1500	−1500		−3000	+600
cde^3	+3750	+3750	−9375	−1875	+375
d^3e		+2500		+2500	−500

$\nabla''D_0$	$3c\partial_d \; 2d\partial_e$ D_0	$=$	$-5D_1$	D_1
c^4	+1875	=	+1875	− 375
c^2e	−9375		−9375	+1875
cd^2	−6250		−6250	+1250

Following the same process we obtain the successive developments of Π_2, Π_3, ... Π_{21}.

Then introducing the first coefficient (a) of the quintic and restoring the constant numerical factor (5^6), we have, for the complete form

$$(a, b, c, d, e, f)^5(x, 1)^5 = 0,$$

the symmetric product

$$\Pi = \frac{1}{a^{20}} \text{ multiplied into}$$

$5^5\times$

$-$ 125	$a^{16}cdef^3$
+ 25	$a^{15}ce^2f^3$
+ 25	$a^{15}d^2ef^3$
$-$ 10	$a^{15}def^4$
+ 1	$a^{14}f^6$

$+5^5\times$

+ 250	$a^{17}b^2def^3$
$-$ 50	$a^{17}b^2e^2f^3$
+ 375	$a^{17}bc^2ef^3$
+ 250	$a^{17}bcd^2f^3$
+ 75	$a^{17}bcde^2f^3$
$-$ 20	$a^{17}bce^3f^3$
$-$ 100	$a^{17}bd^2e^2f^3$
+ 30	$a^{17}bd^3ef^3$
$-$ 2	$a^{17}bde^3$
+ 125	$a^{17}c^2df^3$
$-$ 100	$a^{17}c^2e^2f^3$
$-$ 150	$a^{17}c^2d^2ef^3$
+ 175	$a^{17}c^2def^4$
$-$ 32	$a^{17}c^2e^5$
$-$ 25	$a^{17}cd^4f^3$
$-$ 35	$a^{17}cd^3e^2f^3$
+ 7	$a^{17}cd^2e^3$
+ 10	$a^{17}d^3ef$
$-$ 2	$a^{17}d^4e^2$

$+5^5\times$

$-$1250	$a^{16}b^3ceef^3$
$-$ 500	$a^{16}b^3d^2f^4$
$-$ 250	$a^{16}b^3de^2f^3$
+ 60	$a^{16}b^3e^4f$
$-$1875	$a^{16}b^2c^2df^3$
$-$ 75	$a^{16}b^2c^2e^2f^3$
+ 525	$a^{16}b^2cd^2ef^3$
$-$ 785	$a^{16}b^2cde^2f^4$
+ 141	$a^{16}b^2ce^3$
+ 150	$a^{16}b^2d^4f^3$
+ 80	$a^{16}b^2d^3e^2f$
$-$ 19	$a^{16}b^2d^2e^4$
$-$ 375	$a^{16}bc^4f^3$
+ 925	$a^{16}bc^3def^3$
$-$ 325	$a^{16}bc^3e^2f^3$
+ 600	$a^{16}bc^2d^3f^3$
$-$ 435	$a^{16}bc^2d^2ef$
+ 103	$a^{16}bc^2de^4$
+ 40	$a^{16}bcd^4ef$
+ 3	$a^{16}bcd^3e^3$
$-$ 20	$a^{16}bd^5f$
+ 2	$a^{16}bd^4e^3$

+ 125	$a^{16}c^3ef^3$
+ 150	$a^{16}c^2d^2f^3$
$-$ 1340	$a^{16}c^2def^4$
+ 336	$a^{16}c^2e^5$
+ 510	$a^{16}cd^3ef$
$-$ 58	$a^{16}cd^3e^3$
+ 70	$a^{16}cd^2f^4$
+ 14	$a^{16}cd^2e^2$
$-$ 7	$a^{16}cde^4$
+ 1	$a^{16}d^5$

$+5^5\times$

+ 1000	$a^{15}b^4ef^3$
+ 5125	$a^{15}b^3cdf^3$
+ 1275	$a^{15}b^3ce^2f^3$
+ 100	$a^{15}b^3d^2ef^3$
+ 790	$a^{15}b^3def^4$
$-$ 156	$a^{15}b^3e^5$
+ 3875	$a^{15}b^2c^2f^3$
$-$ 2775	$a^{15}b^2c^2def^3$
+ 2135	$a^{15}b^2c^2e^2f^3$
$-$ 2550	$a^{15}b^2cd^2f^4$
+ 2140	$a^{15}b^2cd^2ef^3$
$-$ 445	$a^{15}b^2cde^4$
$-$ 260	$a^{15}b^2d^4ef$
+ 20	$a^{15}b^2d^3e^3$
$-$ 2075	$a^{15}bc^4ef^3$
$-$ 4825	$a^{15}bc^3d^2f^3$
+12075	$a^{15}bc^3def^4$
$-$ 2680	$a^{15}bc^3e^5$
$-$ 3030	$a^{15}bc^2d^3ef^3$
+ 140	$a^{15}bc^2d^2e^2f^3$
+ 420	$a^{15}bc^2cd^3f$
$-$ 100	$a^{15}bc^2cd^4e^2$
+ 20	$a^{15}bc^2d^5e$
$-$ 1150	$a^{15}bc^2def^5$
+ 3770	$a^{15}bc^2e^2f^4$
+ 470	$a^{15}bc^2d^2ef$
$-$ 1000	$a^{15}bc^2de^5$
+ 30	$a^{15}bc^2d^4f^3$
$-$ 330	$a^{15}bc^2d^3e^2$
+ 140	$a^{15}bc^2d^2e$
$-$ 10	$a^{15}bcd^7$
$-$ 50	$a^{15}c^3f^3$
+ 2800	$a^{15}c^3def^3$
$-$ 1330	$a^{15}c^3e^5$
$-$ 425	$a^{15}c^2d^2f^3$
$-$ 95	$a^{15}c^2d^2e^2$
+ 115	$a^{15}cd^4e$
$-$ 10	$a^{15}cd^5$

$+5^5\times$

$-$ 750	$a^{14}b^4df^3$
$-$ 270	$a^{14}b^4e^2f^3$
$-$ 2475	$a^{14}b^3c^2f^3$
+ 45	$a^{14}b^3cdef^3$
$-$ 880	$a^{14}b^3ce^2f^3$
+ 400	$a^{14}b^3d^2f^3$
$-$ 540	$a^{14}b^3d^2ef^3$
+ 102	$a^{14}b^3de^4$
+ 1495	$a^{14}b^2c^2ef^3$
+ 4320	$a^{14}b^2c^2d^2f^3$
$-$ 8250	$a^{14}b^2c^2def^4$
+ 1568	$a^{14}b^2c^2e^5$
+ 1195	$a^{14}b^2cd^3ef$
+ 6	$a^{14}b^2cd^2e^3$
$-$ 58	$a^{14}b^2d^4f$
+ 27	$a^{14}b^2d^3e^2$
+ 3740	$a^{14}b^2cd^2f^3$
$-$ 7165	$a^{14}b^2c^2def^3$
+ 1635	$a^{14}b^2c^2de^2f^3$
$-$ 40	$a^{14}b^2c^2d^3f$
+ 490	$a^{14}b^2c^2d^2e^2$
$-$ 100	$a^{14}b^2cd^2e$
+ 485	$a^{14}b^2c^3f^3$
$-$ 8280	$a^{14}b^2c^3def^3$
+ 3115	$a^{14}b^2c^3e^2f^3$
+ 720	$a^{14}b^2c^3d^2f$
+ 1040	$a^{14}b^2c^3d^2e^2$
$-$ 265	$a^{14}b^2c^3d^3e$
+ 5	$a^{14}b^2c^3d^4$
$-$ 1660	$a^{14}bc^4ef$
$-$ 355	$a^{14}bc^4d^2f$
+ 1150	$a^{14}bc^4def^2$
$-$ 115	$a^{14}bc^4e^3f$
$-$ 10	$a^{14}bc^4d^2e$
$-$ 325	$a^{14}c^4df$
+ 480	$a^{14}c^4e^2$
$-$ 45	$a^{14}c^3d^2e$
$-$ 5	$a^{14}c^2d^3$

$+5^5\times$

+ 630	$a^{13}b^7ef^3$
+ 90	$a^{13}b^7def^3$
+ 116	$a^{13}b^7e^2f$
$-$ 355	$a^{13}b^6c^2ef^3$
$-$ 1275	$a^{13}b^6cd^2f^3$
+ 2481	$a^{13}b^6cdef^4$
$-$ 399	$a^{13}b^6ce^5$
$-$ 114	$a^{13}b^6d^3ef$
$-$ 10	$a^{13}b^6d^2e^3$
$-$ 3550	$a^{13}b^5c^3def^3$

+ 5456	$a^{13}b^5c^3e^2f^3$
+ 1149	$a^{13}b^5c^2d^2ef$
$-$ 958	$a^{13}b^5c^2de^3$
$-$ 48	$a^{13}b^5cd^3f$
$-$ 219	$a^{13}b^5cd^2e^2$
+ 14	$a^{13}b^5d^3e$
$-$ 1143	$a^{13}b^4c^3f^3$
+ 9717	$a^{13}b^4c^2def$
$-$ 3002	$a^{13}b^4c^2e^3$
$-$ 426	$a^{13}b^4cd^2f$
$-$ 1480	$a^{13}b^4c^2d^2e^2$
+ 138	$a^{13}b^4c^2d^4e$
+ 8	$a^{13}b^4cd^6$
+ 4690	$a^{13}b^3c^3ef$
+ 1240	$a^{13}b^3c^2d^2f$
$-$ 2950	$a^{13}b^3c^2de^2$
+ 40	$a^{13}b^3cd^4e$
+ 40	$a^{13}b^3c^2d^5$
+ 1585	$a^{13}b^2c^4f$
$-$ 1715	$a^{13}b^2c^3e$
$-$ 195	$a^{13}b^2c^2d^2e$
+ 50	$a^{13}b^2c^2d^4$
+ 195	$a^{13}bc^3f$
$-$ 265	$a^{13}bc^3de$
+ 30	$a^{13}bc^2d^3$
$-$ 80	$a^{13}c^5e$
+ 10	$a^{13}c^4d^2$

$+5^5\times$

$-$ 56	$a^{12}b^7f^3$
+ 17	$a^{12}b^6cef^3$
+ 123	$a^{12}b^6d^2f^3$
$-$ 270	$a^{12}b^6def^4$
+ 37	$a^{12}b^6e^5$
+ 1456	$a^{12}b^5c^2df^3$
$-$ 2063	$a^{12}b^5c^2e^2f$
$-$ 444	$a^{12}b^5cd^2ef$
+ 241	$a^{12}b^5cde^3$
+ 12	$a^{12}b^5d^4f$
+ 30	$a^{12}b^5d^3e^2$
+ 1117	$a^{12}b^4c^4f^3$
$-$ 5903	$a^{12}b^4c^3def$
+ 1515	$a^{12}b^4c^3e^3$
+ 113	$a^{12}b^4c^2d^2f$
+ 811	$a^{12}b^4c^2d^2e^2$
$-$ 13	$a^{12}b^4cd^4e$
+ 2	$a^{12}b^4d^6$
$-$ 5577	$a^{12}b^3c^3ef$
$-$ 1616	$a^{12}b^3c^2d^2f$
+ 3047	$a^{12}b^3c^2de^3$
+ 162	$a^{12}b^3cd^4e$
$-$ 36	$a^{12}b^3c^2d^5$
$-$ 3060	$a^{12}b^2c^4df$

+ 2609	$a^{12}b^2c^3e^3$
+ 744	$a^{12}b^2c^2d^2e$
$-$ 84	$a^{12}b^2cd^4$
$-$ 785	$a^{12}b^2c^4f$
+ 995	$a^{12}b^2c^3de$
$-$ 70	$a^{12}b^2c^2d^3$
+ 380	$a^{12}b^2c^5e$
$-$ 10	$a^{12}b^2c^4d^2$
+ 20	$a^{12}bc^{10}d$
+ 5	$a^{12}c^{12}$

$+5^5\times$

+ 2	$a^{11}b^{10}ef^2$
$-$ 276	$a^{11}b^9cdf^2$
+ 384	$a^{11}b^9ce^2f^2$
+ 58	$a^{11}b^9d^2ef$
$-$ 22	$a^{11}b^9de^3$
$-$ 556	$a^{11}b^8c^2f^2$
+ 1971	$a^{11}b^8cdef$
$-$ 421	$a^{11}b^8ce^3$
$-$ 4	$a^{11}b^8cd^2f$
$-$ 194	$a^{11}b^8c^2d^2e^2$
$-$ 2	$a^{11}b^8d^4e$
+ 3629	$a^{11}b^7c^2ef$
+ 1047	$a^{11}b^7c^2d^2f$
$-$ 1623	$a^{11}b^7c^2de^2$
$-$ 147	$a^{11}b^7c^2d^4e$
+ 10	$a^{11}b^7cd^6$
+ 3143	$a^{11}b^6c^3df$
$-$ 2206	$a^{11}b^6c^3e^2$
$-$ 897	$a^{11}b^6c^2d^2e$
+ 52	$a^{11}b^6c^2d^4$
+ 1372	$a^{11}b^5c^3f$
$-$ 1588	$a^{11}b^5c^3de$
+ 48	$a^{11}b^5c^2d^3$
$-$ 794	$a^{11}b^4c^4e$
$-$ 52	$a^{11}b^4c^3d^2$
$-$ 100	$a^{11}b^3c^4d$
$-$ 30	$a^{11}b^3c^{11}$

$+5^4\times$

+ 100	$a^{10}b^{11}df^3$
$-$ 140	$a^{10}b^{11}e^2f$
+ 750	$a^{10}b^{10}c^2f^3$
$-$ 1710	$a^{10}b^{10}cdef$
+ 305	$a^{10}b^{10}ce^3$
$-$ 10	$a^{10}b^{10}d^2f$
+ 85	$a^{10}b^{10}d^2e^2$
$-$ 6985	$a^{10}b^9c^2ef$
$-$ 1830	$a^{10}b^9c^2d^2f$
+ 2355	$a^{10}b^9c^2de^2$
+ 220	$a^{10}b^9cd^2e$

$-4\ a^{20}b^9c^3$	$+3555\ a^{17}b^{10}c^4df$	$-8735\ a^{14}b^{11}c^6f$	$+5^3\times$	$+5^3\times$
$-9550\ a^{19}b^8c^4df$	$-1016\ a^{17}b^{10}c^4e^2$	$+6965\ a^{14}b^{11}c^5de$	$-26\ a^{21}b^{10}df$	$-8\ a^{21}b^{10}f$
$+5670\ a^{19}b^8c^5e^2$	$-825\ a^{17}b^{10}c^5d^2e$	$+270\ a^{14}b^{11}c^6d^2$	$+9\ a^{21}b^{11}e^2$	$+52\ a^{21}b^{11}ce$
$+2640\ a^{19}b^9c^3d^2e$	$+2\ a^{17}b^{10}cd^3$	$+9620\ a^{13}b^{12}c^4e$	$-394\ a^{21}b^{12}cf$	$+6\ a^{21}b^{12}d^2$
$-60\ a^{19}b^9c^4d^4$	$+4232\ a^{17}b^9c^2f$	$+2410\ a^{13}b^{12}c^4d^2$	$+142\ a^{21}b^{12}de$	$+220\ a^{21}b^{12}c^2d$
$-6835\ a^{18}b^7c^4f$	$-3766\ a^{17}b^9c^2de$	$+5700\ a^{13}b^{13}c^3d$	$+4\ a^{21}b^{13}d^3$	$+690\ a^{21}b^{13}c^4$
$+7025\ a^{18}b^7c^5de$	$-112\ a^{17}b^9c^3e^2$	$+3450\ a^{13}b^{14}c^5$	$+792\ a^{21}b^{14}c^2e$	
$+30\ a^{18}b^7c^6d^3$	$-3716\ a^{17}b^9c^4e$		$+196\ a^{21}b^{14}c^3d^2$	$+5^3\times$
$+4800\ a^{18}b^8c^2e$	$-778\ a^{17}b^9c^5d^2$	$+5^3\times$	$+1140\ a^{21}b^{15}c^4d$	$-2\ a^{21}b^{20}e$
$+700\ a^{18}b^8c^3d^2$	$-1400\ a^{17}b^7c^4d$		$+1345\ a^{21}b^{16}c^5$	$-20\ a^{21}b^{19}cd$
$+1100\ a^{18}b^8c^4d$	$-650\ a^{17}b^9c^5$	$+52\ a^7b^{13}ef$		$-130\ a^{21}b^{18}c^2$
$+405\ a^{18}b^8c^5$		$+494\ a^7b^{14}cdf$	$+5^2\times$	
	$+5^3\times$	$-193\ a^7b^{14}ce^2$		$+5\times$
$+5^4\times$	$+30\ a^{21}b^{14}f^2$	$-42\ a^7b^{15}d^2e$	$+190\ a^{21}b^{17}cf$	$+4\ a^{21}b^{21}d$
$-105\ a^{21}b^{13}ef^2$	$-990\ a^{21}b^{15}cef$	$+2339\ a^7b^{15}c^2f$	$-34\ a^{21}b^{17}de$	$+81\ a^{21}b^{20}c^2$
$+120\ a^{21}b^{13}def$	$-120\ a^{21}b^{15}d^2f$	$-1265\ a^7b^{15}c^2de$	$-596\ a^{21}b^{16}c^2e$	
$-18\ a^{21}b^{13}e^3$	$+108\ a^{21}b^{15}de^2$	$-54\ a^7b^{15}cd^3$	$-118\ a^{21}b^{16}cd^2$	$+5\times$
$+1590\ a^{21}b^{11}c^2ef$	$-3985\ a^{21}b^{16}c^2df$	$-3374\ a^7b^{15}c^4e$	$+1400\ a^{21}b^{15}c^3d$	$(-6\ ab^{20}c)$
$+330\ a^{21}b^{11}cd^2f$	$+1772\ a^{21}b^{15}c^3e^2$	$-892\ a^7b^{15}c^3d^2$	$-2550\ a^{21}b^{14}c^5$	
$-354\ a^{21}b^{11}cde^2$	$+657\ a^{21}b^{15}cd^2e$	$-3100\ a^7b^{15}c^4d$		$+1\times$
$-22\ a^{21}b^{11}d^3e$	$+3\ a^{21}b^{15}d^4$	$-2550\ a^7b^{16}c^5$		$(+1\ b^{24})^*$

24. A partial verification of this value is afforded by supposing the last two coefficients of the quintic, viz. e and f, or (what in effect is the same thing) two of its roots, say x_4 and x_5, to vanish. We have then (art. 11 *et seq.*)

$$\theta_1=\theta_2=\frac{1}{a^3}(na+5dx_3),$$

$$\theta_3=\theta_5=\frac{1}{a^3}(na+5dx_2),$$

$$\theta_4=\theta_6=\frac{1}{a^3}(na+5dx_1),$$

where I have for the moment written a in place of

$$5a^3bd+5a^2c^2-5ab^2c+b^4.$$

And, consequently, in this case the product is

$$\frac{1}{a^{24}}\times\text{the square of}$$

$-5^3\ a^3d^4$	$-2^2.5^3\ a^{12}b^3c^3d$	$-11.5^2\ a^{21}b^6c^3$
$+5^3\ a^7bcd^3$	$-3.5^3\ a^{11}b^5c^2$	$+2.5^2\ a^{21}b^7d$
$+5^3\ a^7c^3d^2$	$+5^2\ a^{11}b^4d^2$	$+18.5^2\ a^{21}b^7c^2$
$+2.5^3\ a^6b^4d$	$+14.5^2\ a^{11}b^6c^2d$	$-3.5\ ab^{19}c$
$+5^3\ a^6c^5$	$+18.5^2\ a^{11}b^7c^4$	$+1\ b^{23}$
$-2^2.5^2\ a^5b^4cd^2$	$-2^2.5^2\ a^{11}b^7cd$	

Developing this expression, all the terms in Π not affected by e or f will be verified.

* It is noticeable that the following combinations do not enter into the above expression for Π, viz.

$a^{15}g^4$ $a^{15}b_6f^4$ $a^{15}g^3f^2$ $a^{15}b^2f^4$
$a^{15}b_6f^4$ $a^{15}c^2f^4$ $a^{17}b^2cf^3$ $a^{19}b^2g^2$.

25. But a more complete verification is obtained by taking the sum of all the numerical coefficients that enter into the expression for Π. That sum is

$$-4761104,$$

as it ought to be. For the roots of

$$(1, 1, 1, 1, 1, 1)(x, 1)^6=0$$

are

$$-1,\ \alpha,\ -\alpha,\ \alpha^2,\ -\alpha^2,$$

α being an unreal cube root of unity; and for this particular form we have

$$\theta_1=11-10\alpha,\quad \theta_2=\ 21+10\alpha,\quad \theta_3=1,$$
$$\theta_4=\ -29\ ,\quad \theta_5=-\ 4+20\alpha,\quad \theta_6=-24-20\alpha;$$

or

$$\theta_1\theta_2=331,\quad \theta_3\theta_5=-29,\quad \theta_4\theta_6=496.$$

Consequently

$$\Pi=\theta_1\theta_2\theta_3\theta_4\theta_5\theta_6=-4761104.$$

26. Another convenient verification is afforded by writing

$$a,\quad 5b,\quad 10c,\quad 10d,\quad 5e,\quad f$$

for

$$a,\quad b,\quad c,\quad d,\quad e,\quad f$$

respectively, when the sum of the numerical coefficients of the several powers of f should be zero*. And the transformed result will be worth having for its own sake, as belonging to Mr. CAYLEY's standard

$$(a, b, c, d, e, f)(x, 1)^5.$$

* Not only the sum of *all* the numerical coefficients, but the sum of the numerical coefficients of *each power of f*, is zero. The reason is, because the roots of

$$(1, 1, 1, 1, 1, f)(x, 1)^5=0$$

are included in the form

$$-1+\omega^m\sqrt[5]{1-f},$$

and therefore $\beta=0$. So that in this case the symmetric product vanishes identically.

In relation to the foregoing property, Mr. CAYLEY remarks as follows:—More generally the forms

$$(1, 0, 0, 0, 0, 1+f)(x, y)^5,\quad (1, 1, 1, 1, 1, f)(x', y')^5$$

are equivalent, the modulus of substitution being unity, as is at once seen by writing

$$x=x'+y'$$
$$y=\quad y';$$

and the leading coefficients of any covariant of the same two forms respectively are therefore absolutely identical. That is, any seminvariant of the form

$$(a, b, c, d, e, f)(x, y)^5$$

will have the same value, whether we write therein

$$(a, b, c, d, e, f)=(1, 0, 0, 0, 0, 1+f),$$

or

$$(a, b, c, d, e, f)=(1, 1, 1, 1, 1,\ f\ );$$

whence in particular a seminvariant which vanishes upon writing therein $(b, c, d, e)=(0, 0, 0, 0)$ will also vanish upon writing therein $(a, b, c, d, e)=(1, 1, 1, 1, 1)$; that is, in such a seminvariant the sum of the numerical coefficients of *each power* of f is zero.

That result, arranging the terms according to descending powers of f, is

$$\Pi = \frac{5^{14}}{a^{31}} \times$$

$$\left(\begin{array}{|rl|rl|} \hline -4 & a^{16}cde & -48 & a^{16}bc^4 \\ +4 & a^{17}b^2de & +8 & a^{16}b^5e \\ +12 & a^{17}bc^2e & +164 & a^{16}b^4cd \\ +16 & a^{17}bcd^2 & +248 & a^{16}b^3c^2 \\ +16 & a^{17}c^3d & -60 & a^{14}b^6d \\ -20 & a^{16}b^2ce & -396 & a^{14}b^5c \\ -16 & a^{16}b^2d^2 & +252 & a^{13}b^7e \\ -120 & a^{16}b^2c^2d & -56 & a^{13}b^9 \\ \hline \end{array} \right) f^3.$$

$$+\left(\begin{array}{|rl|rl|rl|rl|rl|} \hline +2 & a^{18}ce^2 & +168 & a^{15}b^2cd^2e & -1328 & a^{15}b^3c^4e & +6208 & a^{14}b^3c^5 & +50 & a^{11}b^{10}e \\ +4 & a^{18}d^2e^2 & +96 & a^{15}b^3d^4 & -6176 & a^{15}b^5c^2d^2 & +180 & a^{13}b^7de & -27600 & a^{11}b^8cd \\ -2 & a^{17}b^3e^3 & +592 & a^{15}b^6de & -2944 & a^{15}bc^4d & -1420 & a^{13}b^6c^2e & -111200 & a^{11}b^6c^3 \\ +12 & a^{17}bcde^3 & +768 & a^{15}bc^2d^3 & -256 & a^{15}c^7 & -10200 & a^{13}b^5cd^2 & +5000 & a^{10}b^{11}d \\ -32 & a^{17}b^2e^4 & +160 & a^{15}c^4e & -54 & a^{14}b^8e^3 & -56800 & a^{13}b^4c^2d & +75000 & a^{10}b^{10}e^2 \\ -32 & a^{17}c^3e^3 & +384 & a^{15}c^2d^2 & +36 & a^{14}b^7cde & -36576 & a^{13}b^4c^3 & -26250 & a^9b^{13}c \\ -96 & a^{17}c^3d^2e & +102 & a^{15}b^4ce^2 & +640 & a^{14}b^7d^2 & +170 & a^{13}b^6ce & +3750 & a^9b^{14} \\ -32 & a^{17}cd^4 & +16 & a^{15}b^4d^2e & +2392 & a^{14}b^4c^3e & +2460 & a^{12}b^8d^2 & & \\ -20 & a^{16}b^3de^2 & -888 & a^{14}b^5c^2de & +13824 & a^{14}b^4c^2d^2 & +58240 & a^{12}b^7c^2d & & \\ -12 & a^{16}b^3c^2e^3 & -1632 & a^{15}b^5cd^3 & +23936 & a^{14}b^4c^4d & +89360 & a^{12}b^6c^4 & & \\ \hline \end{array} \right) f^2,$$

$$+\left(\begin{array}{|rl|rl|rl|rl|rl|} \hline -4 & a^{18}de^4 & -832 & a^{15}b^3d^4e & -212480 & a^{14}b^5c^2e & -2361200 & a^{12}b^8c^3de & -38200000 & a^{10}b^8c^4d \\ -8 & a^{17}bce^4 & +38640 & a^{15}b^5c^3de^2 & -90880 & a^{14}b^6c^4d^2 & +90400 & a^{12}b^8c^2d^3 & -54680000 & a^{10}b^7c^5 \\ +24 & a^{17}bd^2e^3 & -19392 & a^{15}b^5c^2d^3e & -166400 & a^{14}c^6d & -4461600 & a^{12}b^6c^6e & +150000 & a^9b^{13}de \\ +280 & a^{17}c^2d^2e^3 & +5376 & a^{15}b^7cd^4 & +580 & a^{13}b^7c^3 & -2585600 & a^{12}b^6c^4d^2 & +3975000 & a^9b^{11}c^3e \\ -112 & a^{17}cd^3e^2 & +24128 & a^{15}bc^5e^3 & +49620 & a^{13}b^6cde^2 & -9792000 & a^{12}b^6c^5d & +1650000 & a^9b^{11}cd^2 \\ +64 & a^{17}d^5e & +6016 & a^{15}bc^4d^2e & -4560 & a^{13}b^6d^3e & -5024000 & a^{12}b^5c^6 & +35550000 & a^9b^{10}c^2d \\ +12 & a^{16}b^3e^4 & +768 & a^{15}bc^3d^4 & +218240 & a^{13}b^5c^3e^3 & +96000 & a^{11}b^7ce^2 & +85640000 & a^9b^9c^5 \\ -628 & a^{16}b^2cde^3 & +71680 & a^{14}c^6de & +91920 & a^{13}b^5c^2d^2e & +29000 & a^{11}b^7d^2e & -1237500 & a^8b^{13}ce \\ +128 & a^{16}b^2d^3e^2 & -21760 & a^{14}c^6d^3 & -7680 & a^{13}b^5cd^4 & +1971000 & a^{11}b^6c^3de & -300000 & a^8b^{13}d^2 \\ -520 & a^{16}bc^2e^3 & -1760 & a^{14}b^6ce^3 & +1554720 & a^{13}b^4c^4de & -8000 & a^{11}b^6cd^3 & -19925000 & a^8b^{12}c^2d \\ -1392 & a^{16}bc^2d^2e^2 & -2160 & a^{14}b^6d^3e^2 & -136320 & a^{13}b^4c^3d^3 & +7258000 & a^{11}b^5c^4e & -87550000 & a^8b^{11}c^4 \\ +256 & a^{16}bcd^4e & -66000 & a^{14}b^5c^2de^3 & +1500800 & a^{13}b^5c^5e & +4188000 & a^{11}b^5c^3d^2 & +162500 & a^7b^{13}e \\ -256 & a^{16}bd^6 & +19120 & a^{14}b^5cd^3e & +793600 & a^{13}b^5c^4d^2 & +25144000 & a^{11}b^4c^4d & +6175000 & a^7b^{12}cd \\ -8576 & a^{16}c^4de^3 & -1856 & a^{14}b^4d^5 & +2028800 & a^{13}b^5c^2d & +21952000 & a^{11}b^4c^5 & +58475000 & a^7b^{11}c^3 \\ +6528 & a^{16}c^3d^2e & -114640 & a^{14}b^7c^4e^2 & +499200 & a^{13}b^6c^3 & -17500 & a^{10}b^{11}e^3 & -812500 & a^6b^{14}d \\ -1792 & a^{16}c^2d^3 & -40320 & a^{14}b^7c^3d^2e & -13500 & a^{12}b^8d^2e^2 & -855000 & a^{10}b^{10}cde & -24625000 & a^6b^{13}c^2 \\ +316 & a^{16}b^4de^3 & -2560 & a^{14}b^7c^2d^4 & -206300 & a^{12}b^7c^3e^2 & -10000 & a^{10}b^{10}d^2 & +5937500 & a^6b^{12}c \\ +1708 & a^{15}b^3c^3e^3 & -529920 & a^{14}b^6c^4de & -88800 & a^{12}b^7cd^3e & -6985000 & a^{10}b^9c^3e & -625000 & a^4b^{19} \\ +3424 & a^{15}b^3cd^2e^2 & +92160 & a^{14}b^6c^4d^3 & +4800 & a^{12}b^7d^4 & -3660000 & a^{10}b^9c^2d^2 & & \\ \hline \end{array} \right) f,$$

$$
\begin{array}{rl|rl|rl}
+ & 1\ a^{18}e^{6} & - & 4720000\ a^{13}b^{3}c^{4}de^{3} & + & 112000000\ a^{10}b^{6}d^{3} \\
- & 4\ a^{17}bde^{6} & + & 128000\ a^{13}b^{3}c^{4}d^{3}e & + & 352000000\ a^{10}b^{5}c^{3}d \\
- & 128\ a^{17}c^{2}e^{5} & + & 256000\ a^{13}b^{2}c^{5}d^{3} & + & 259200000\ a^{10}b^{4}c^{10} \\
+ & 56\ a^{17}cd^{2}e^{4} & - & 5488000\ a^{13}b^{2}c^{5}e^{3} & - & 56250\ a^{9}b^{13}e^{3} \\
- & 32\ a^{17}d^{4}e^{3} & - & 1248000\ a^{13}b^{2}c^{4}d^{2}e & - & 4425000\ a^{9}b^{11}cde^{3} \\
+ & 282\ a^{16}b^{2}e^{6} & + & 640000\ a^{13}b^{2}c^{3}d^{4} & - & 550000\ a^{9}b^{11}d^{2}e \\
- & 76\ a^{16}b^{2}d^{2}e^{4} & - & 3392000\ a^{13}b^{2}c^{3}de & - & 45400000\ a^{9}b^{10}c^{2}e^{3} \\
+ & 824\ a^{16}bc^{2}de^{4} & + & 768000\ a^{13}b^{2}c^{2}d^{3} & - & 41250000\ a^{9}b^{10}c^{2}d^{2}e \\
+ & 48\ a^{16}bcd^{3}e^{3} & - & 2048000\ a^{13}c^{6}e & + & 200000\ a^{9}b^{10}cd^{4} \\
+ & 64\ a^{16}bd^{5}e^{2} & + & 512000\ a^{13}c^{5}d^{2} & - & 376600000\ a^{9}b^{9}c^{4}de \\
+ & 5376\ a^{15}c^{4}e^{4} & + & 4625\ a^{12}b^{8}e^{4} & - & 22400000\ a^{9}b^{9}c^{2}d^{3} \\
- & 1856\ a^{15}c^{3}d^{2}e^{3} & + & 120500\ a^{12}b^{7}cde^{3} & - & 743200000\ a^{9}b^{8}e \\
+ & 896\ a^{15}c^{3}d^{4}e^{2} & + & 30000\ a^{12}b^{7}d^{3}e^{2} & - & 311200000\ a^{9}b^{8}c^{2}d \\
- & 896\ a^{15}cd^{6}e & + & 1515000\ a^{12}b^{6}c^{3}e^{3} & - & 1120000000\ a^{9}b^{7}c^{4}d \\
+ & 256\ a^{15}d^{8} & + & 1622000\ a^{12}b^{6}c^{3}d^{2}e^{2} & - & 1040000000\ a^{9}b^{6}c^{6} \\
- & 156\ a^{14}b^{4}e^{6} & - & 52000\ a^{12}b^{6}cd^{4}e & + & 675000\ a^{8}b^{13}de^{3} \\
- & 1780\ a^{14}b^{3}cde^{4} & - & 16000\ a^{12}b^{6}d^{6} & + & 22150000\ a^{8}b^{12}c^{2}e^{3} \\
+ & 160\ a^{14}b^{3}d^{3}e^{3} & + & 12188000\ a^{12}b^{5}c^{4}de^{3} & + & 16425000\ a^{8}b^{12}cd^{2}e \\
- & 21440\ a^{14}b^{2}c^{2}e^{4} & + & 1296000\ a^{12}b^{5}c^{3}d^{3}e & + & 150000\ a^{8}b^{12}d^{4} \\
+ & 2240\ a^{14}b^{2}c^{2}d^{2}e^{3} & - & 576000\ a^{12}b^{5}c^{2}d^{5} & + & 313250000\ a^{8}b^{11}c^{3}de \\
- & 3200\ a^{14}b^{2}cd^{4}e^{2} & + & 20872000\ a^{12}b^{4}c^{5}e^{3} & + & 27000000\ a^{8}b^{11}c^{2}d^{3} \\
+ & 1280\ a^{14}b^{2}d^{6}e & + & 11904000\ a^{12}b^{4}c^{4}d^{2}e & + & 962000000\ a^{8}b^{10}c^{6}e \\
- & 32000\ a^{14}bc^{4}de^{3} & - & 2688000\ a^{12}b^{4}c^{4}d^{4} & + & 482000000\ a^{8}b^{10}c^{4}d^{3} \\
- & 21120\ a^{14}bc^{3}d^{2}e^{2} & + & 31840000\ a^{12}b^{3}c^{7}de & + & 2280000000\ a^{8}b^{9}c^{6}d \\
+ & 17920\ a^{14}bc^{3}d^{4}e & - & 4480000\ a^{12}b^{3}c^{6}d^{3} & + & 2760000000\ a^{8}b^{8}c^{8} \\
- & 2560\ a^{14}bcd^{7} & + & 24320000\ a^{12}b^{2}c^{8}e & - & 6031250\ a^{7}b^{14}ce^{3} \\
- & 85120\ a^{13}c^{6}e^{3} & - & 1280000\ a^{12}b^{2}c^{6}d^{2}e & - & 2625000\ a^{7}b^{14}d^{2}e \\
+(& - 12160\ a^{13}c^{5}d^{2}e^{2} & + & 5120000\ a^{12}bc^{10}d & - & 158125000\ a^{7}b^{13}c^{2}de \\
+ & 29440\ a^{13}c^{4}d^{4}e & + & 2560000\ a^{12}c^{12} & - & 13500000\ a^{7}b^{13}cd^{3} \\
- & 5120\ a^{13}c^{3}d^{6} & - & 27500\ a^{11}b^{9}de^{3} & - & 843500000\ a^{7}b^{12}c^{3}e \\
+ & 1020\ a^{14}b^{3}d^{2}e^{4} & - & 1052500\ a^{11}b^{8}c^{2}e^{3} & - & 446000000\ a^{7}b^{12}c^{2}d^{2} \\
+ & 31360\ a^{14}b^{3}c^{2}e^{4} & - & 970000\ a^{11}b^{8}cd^{2}e^{2} & - & 3100000000\ a^{7}b^{11}c^{4}d \\
+ & 240\ a^{14}b^{3}cd^{2}e^{3} & - & 20000\ a^{11}b^{8}d^{4}e & - & 5100000000\ a^{7}b^{10}c^{7} \\
+ & 2160\ a^{14}b^{3}d^{4}e^{2} & - & 1623000\ a^{11}b^{7}c^{3}de^{3} & + & 703125\ a^{6}b^{15}e^{3} \\
+ & 130800\ a^{14}b^{2}c^{3}d^{2}e^{2} & - & 2940000\ a^{11}b^{7}c^{3}d^{3}e & + & 44375000\ a^{6}b^{14}cde \\
+ & 78400\ a^{14}b^{2}c^{2}d^{4}e & + & 400000\ a^{11}b^{7}cd^{5} & + & 2500000\ a^{6}b^{14}d^{3} \\
- & 32000\ a^{14}b^{2}cd^{4}e & - & 44120000\ a^{11}b^{6}c^{4}e^{3} & + & 495000000\ a^{6}b^{13}c^{3}e \\
+ & 498400\ a^{14}b^{2}c^{6}e^{3} & - & 35880000\ a^{11}b^{6}c^{4}d^{2}e & + & 245000000\ a^{6}b^{13}c^{2}d^{3} \\
+ & 332800\ a^{14}b^{2}c^{4}d^{3}e^{2} & + & 4160000\ a^{11}b^{6}c^{3}d^{4} & + & 2850000000\ a^{6}b^{12}c^{4}d \\
- & 169600\ a^{14}b^{2}c^{3}d^{5}e & - & 127040000\ a^{11}b^{5}c^{6}de & + & 6725000000\ a^{6}b^{12}c^{6} \\
+ & 6400\ a^{14}b^{2}c^{2}d^{6} & + & 7680000\ a^{11}b^{5}c^{5}d^{3} & - & 5312500\ a^{5}b^{17}de \\
+ & 736000\ a^{14}b^{6}d^{2}e^{2} & - & 127040000\ a^{11}b^{4}c^{8}e & - & 186250000\ a^{5}b^{16}e^{2} \\
- & 147200\ a^{14}b^{6}cd^{3}e & - & 16640000\ a^{11}b^{4}c^{7}d^{2} & - & 73750000\ a^{5}b^{15}cd^{2} \\
- & 25600\ a^{14}b^{6}d^{5} & - & 64000000\ a^{11}b^{3}c^{9}d & - & 1750000000\ a^{5}b^{14}c^{3}d \\
+ & 614400\ a^{14}c^{8}e^{3} & - & 38400000\ a^{11}b^{3}c^{11} & - & 6375000000\ a^{5}b^{13}c^{5} \\
- & 115200\ a^{14}c^{7}d^{2}e & + & 381250\ a^{10}b^{10}ce^{3} & + & 40625000\ a^{4}b^{18}ce \\
- & 25600\ a^{14}c^{6}d^{4} & + & 212500\ a^{10}b^{10}d^{2}e^{2} & + & 9375000\ a^{4}b^{18}d^{2} \\
- & 19950\ a^{13}b^{8}ce^{4} & + & 11775000\ a^{10}b^{9}c^{2}de^{2} & + & 687500000\ a^{4}b^{17}c^{2}d \\
- & 1000\ a^{13}b^{8}d^{2}e^{3} & + & 2200000\ a^{10}b^{9}cd^{3}e & + & 4312500000\ a^{4}b^{16}c^{4} \\
- & 191600\ a^{13}b^{7}c^{3}de^{3} & - & 80000\ a^{10}b^{9}d^{5} & - & 3906250\ a^{3}b^{20}e \\
- & 87600\ a^{13}b^{7}cd^{2}e^{2} & + & 56700000\ a^{10}b^{8}c^{4}e^{3} & - & 156250000\ a^{3}b^{19}cd \\
+ & 11200\ a^{13}b^{7}d^{3}e & + & 52800000\ a^{10}b^{8}c^{3}d^{2}e & - & 2031250000\ a^{3}b^{18}c^{3} \\
- & 1200800\ a^{13}b^{6}c^{4}e^{3} & - & 2400000\ a^{10}b^{8}c^{2}d^{4} & + & 15625000\ a^{2}b^{21}d \\
- & 1184000\ a^{13}b^{6}c^{3}d^{2}e^{2} & + & 281000000\ a^{10}b^{7}c^{5}de & + & 632812500\ a^{2}b^{20}c^{2} \\
+ & 220800\ a^{13}b^{6}c^{2}d^{4}e & + & 2400000\ a^{10}b^{7}c^{4}d^{3} & - & 117187500\ ab^{22}e \\
+ & 25600\ a^{13}b^{6}cd^{6} & + & 384000000\ a^{10}b^{6}c^{7}e & + & 9765625\ b^{24}
\end{array}
$$

XVII. *On the Double Tangents of a Curve of the Fourth Order.*

By ARTHUR CAYLEY, *Esq.*, *F.R.S.*

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THE present memoir is intended to be supplementary to that "On the Double Tangents of a Plane Curve."* I take the opportunity of correcting an error which I have there fallen into, and which is rather a misleading one, viz. the emanants $U_1, U_2 \ldots$ were numerically determined in such manner as to become equal to U on putting (x_1, y_1, z_1) equal to (x, y, z); the numerical determination should have been (and in the latter part of the memoir is assumed to be) such as to render H_1, H_2, &c. equal to H, on making the substitution in question; that is, in the place of the formulæ

$$U_1 = \frac{1}{n}(x_1\partial_x + y_1\partial_y + z_1\partial_z)U,$$

$$U_2 = \frac{1}{n(n-1)}(x_1\partial_x + y_1\partial_y + z_1\partial_z)^2 U, \text{ &c.},$$

there ought to have been

$$U_1 = \frac{1}{(n-2)}(x_1\partial_x + y_1\partial_y + z_1\partial_z)U,$$

$$U_2 = \frac{1}{(n-2)(n-3)}(x_1\partial_x + y_1\partial_y + z_1\partial_z)^2 U, \text{ &c.}$$

The points of contact of the double tangents of the curve of the fourth order or quartic $U=0$, are given as the intersections of the curve with a curve of the fourteenth order $\Pi=0$; the last-mentioned curve is not absolutely determinate, since instead of $\Pi=0$, we may, it is clear, write $\Pi+MU=0$, where M is an arbitrary function of the tenth order. I have in the memoir spoken of HESSE's original form (say $\Pi_1=0$) of the curve of the fourteenth order obtained by him in 1850, and of his transformed form (say $\Pi_2=0$) obtained in 1856. The method in the memoir itself (Mr. SALMON's method) gives, in the case in question of a quartic curve, a third form, say $\Pi_3=0$. It appears by his paper "On the Determination of the Points of Contact of Double Tangents to an Algebraic Curve,"† that Mr. SALMON has verified by algebraic transformations the equivalence of the last-mentioned form with those of HESSE; but the process is not given. The object of the present memoir is to demonstrate the equivalence in question, viz. that of the equation $\Pi_3=0$ with the one or other of the equations $\Pi_1=0$, $\Pi_2=0$, in virtue of the equation $U=0$. The transformation depends, 1st, on a theorem used by HESSE for

* Philosophical Transactions, vol. cxix. (1859) pp. 198–212.

† Quart. Math. Journ. vol. iii. p. 317 (1859).

the deduction of his second form $\Pi_2=0$ from the original form $\Pi_1=0$, which theorem is given in his paper "Transformation der Gleichung der Curven 14ten Grades welche eine gegebene Curve 4ten Grades in den Berührungspuncten ihrer Doppeltangenten schneiden," *Crelle*, t. lii. pp. 97–103 (1856), containing the transformation in question; I prove this theorem in a different and (as it appears to me) more simple manner; 2nd, on a theorem relating to a cubic curve proved incidentally in my memoir "On the Conic of Five-pointic Contact at any point of a Plane Curve",* the cubic curve being in the present case any first emanant of the given quartic curve: the demonstration occupies only a single paragraph, and it is here reproduced; and I reproduce also Hesse's demonstration of the equivalence of the two forms $\Pi_1=0$ and $\Pi_2=0$.

Let $U=(*)(x, y, z)^4$ be a quartic function of (x, y, z); (a, b, c, f, g, h) its second differential coefficients; (A, B, C, F, G, H) the reciprocal system

$$(bc-f^2,\ ca-g^2,\ ab-h^2,\ gh-af,\ hf-bg,\ fg-ch).$$

And let H be the Hessian of U, or determinant $abc-af^2-bg^2-ch^2+2fgh$ (H is of course a sextic function of x, y, z); (a', b', c', f', g', h') the second differential coefficients of H; (A', B', C', F', G', H') the reciprocal system

$$(b'c'-f'^2,\ c'a'-g'^2,\ a'b'-h'^2,\ g'h'-a'f',\ h'f'-b'g',\ f'g'-c'h').$$

Then $U=0$ being the equation of a quartic curve, the equation of the curve of the fourteenth order which by its intersections determines the points of contact of the double tangents of the quartic curve, may be taken to be (Hesse's original form)

$$\Pi_1=(A, B, C, F, G, H)(\partial_x H, \partial_y H, \partial_z H)^3-3H(A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2 H=0\dagger.$$

Or it may be taken to be (Hesse's transformed form)

$$\Pi_2=5(A, B, C, F, G, H)(\partial_x H, \partial_y H, \partial_z H)^3-3(A', B', C', F', G', H')(\partial_x U, \partial_y U, \partial_z U)^3=0.$$

And moreover, if $U_1=\frac{1}{3}(x_1\partial_x+y_1\partial_y+z_1\partial_z)U$, and if H_1 be the Hessian of U_1, and $(a'', b'', c'', f'', g'', h'')$ the second differential coefficients of $H-3H_1$, where in the differentiations (x_1, y_1, z_1) are treated as constants but after the differentiations are effected they are replaced by (x, y, z), and if $(A'', B'', C'', F'', G'', H'')$ be the reciprocal system

$$(b''c''-f''^2,\ c''a''-g''^2,\ a''b''-h''^2,\ g''h''-a''f'',\ h''f''-b''g'',\ f''g''-c''h''),$$

then the equation of the curve of the fourteenth order may be taken to be (Salmon's form)

$$\Pi_3=(A'', B'', C'', F'', G'', H'')(\partial_x U, \partial_y U, \partial_z U)^3=0.$$

I have preferred to write the three equations in the foregoing forms; but it is clear that the terms

$$(A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^3 H;\ (A', B', C', F', G', H')(\partial_x, \partial_y, \partial_z)^3 U$$

might also have been written

$$(A, B, C, F, G, H)(a', b', c', 2f', 2g', 2h');\ (A', B', C', F', G', H')(a, b, c, 2f, 2g, 2h).$$

* Philosophical Transactions, vol. cxlix. (1859), see p. 385.

† In quoting this formula in my former memoir, the numerical factor 3 is by mistake omitted.

As already noticed, it has been shown by HESSE (and his demonstration is to be here reproduced) that the two forms $\Pi_1=0$ and $\Pi_2=0$ are equivalent to each other. And the object of the memoir is to show that the third form $\Pi_3=0$ is equivalent to the other two. The equivalences in question subsist in virtue of the equation $U=0$, that is, the functions Π_1, Π_2, Π_3 are not identical, but differ from each other by multiples of U.

Demonstration of HESSE'S *Theorem.*

Let (a, b, c, f, g, h), (a', b', c', f', g', h') be any systems of coefficients of a ternary quadratic function; (A, B, C, F, G, H), (A', B', C', F', G', H') the reciprocal systems as above, (x, y, z) arbitrary quantities. Consider the function

$$\square=(a, b, c, f, g, h)(x, y, z)^2.(A', B', C', F', G', H')(a, b, c, 2f, 2g, 2h)$$
$$-(A', B', C', F', G', H')(ax+hy+gz, hx+by+fz, gx+fy+cz)^2.$$

The term involving A' is

$$a(a, b, c, f, g, h)(x, y, z)^2-(ax+hy+gz)^2,$$

which is

$$=(ab-h^2)y^2+(ac-g^2)z^2+2(af-gh)yz$$
$$=Cy^2+Bz^2-2Fyz;$$

and the term involving $2F'$ is

$$f(a, b, c, f, g, h)(x, y, z)^2-(hx+by+fz)(gx+fy+cz),$$

which is

$$=(af-gh)x^2+(f^2-bc)yz+(fg-ch)zx+(hf-bg)xy$$
$$=-Fx^2-Ayz+Hzx+Gxy;$$

and the entire expression for $\square$ is thus

$$\begin{aligned}&A'(Cy^2+Bz^2-2Fyz)\\&+B'(Ax^2+Cz^2-2Gzx)\\&+C'(Bx^2+Ay^2-2Hxy)\\&+2F'(-Fx^2-Ayz+Hzx+Gxy)\\&+2G'(-Gy^2-Bzx+Fxy+Hyz)\\&+2H'(-Hz^2-Cxy+Gyz+Fzx);\end{aligned}$$

or what is the same thing,

$$\square=(BC'+B'C-2FF',\ CA'+C'A-2GG',\ AB'+A'B-2HH',$$
$$GH'+G'H-AF'-A'F,\ HF'+H'F-BG'-B'G,\ FG'+F'G-CH'-C'H)(x, y, z)^2,$$

which is really the fundamental theorem. It is however used as follows; viz. the right-hand side being symmetrical in regard to the two systems

$$(a, b, c, f, g, h),\quad (a', b', c', f', g', h'),$$

the left-hand side, which is not in form symmetrical as regards the two systems, must be so in reality; or if $\square'$ is what $\square$ becomes by interchanging the two systems, then $\square'=\square$;

or substituting for $\square$ and $\square'$ their values, we have

$$(a, b, c, f, g, h)(x, y, z)^2.(A', B', C', F', G', H')(a, b, c, 2f, 2g, 2h)$$
$$-(A', B', C', F', G', H')(ax+hy+gz,\ hx+by+fz,\ gx+fy+cz)^2$$
$$=(a', b', c', f', g', h')(x, y, z)^2.(A, B, C, F, G, H)(a', b', c', 2f', 2g', 2h')$$
$$-(A, B, C, F, G, H)(a'x+h'y+g'z,\ h'x+b'y+f'z,\ g'x+f'y+c'z)^2,$$

which is HESSE'S theorem.

If in particular (a, b, c, f, g, h) are the second differential coefficients of a function $u=(\bullet)(x, y, z)^p$, and (a', b', c', f', g', h') the second differential coefficients of a function $u'=(\bullet)(x, y, z)^{p'}$, then the equation becomes

$$p(p-1)u.(A', B', C', F', G', H')(\partial_x, \partial_y, \partial_z)^2u-(p-1)^2(A', B', C', F', G', H')(\partial_x u, \partial_y u, \partial_z u)^2$$
$$=p'(p'-1)u'.(A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2u'-(p'-1)^2(A, B, C, F, G, H)(\partial_x u', \partial_y u', \partial_z u')^2;$$

and if for u, u' we take the quartic function U and the sextic function H, its Hessian, we have

$$12U.(A', B', C', F', G', H')(\partial_x, \partial_y, \partial_z)^2U-\ 9(A', B', C', F', G', H')(\partial_x U, \partial_y U, \partial_z U)^2$$
$$=30H.(A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2H-25(A, B, C, F, G, H)(\partial_x H, \partial_y H, \partial_z H)^2;$$

and if in this identical equation we write $U=0$, then from the resulting equation and the equation

$$\Pi_1=-3H(A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2H+(A, B, C, F, G, H)(\partial_x H, \partial_y H, \partial_z H)^2$$

we may eliminate any one of the three terms

$$(A', B', C', F', G', H')(\partial_x U, \partial_y U, \partial_z U)^2,$$
$$H.(A, B, C, F, G, H)(\partial_x\ \ , \partial_y\ \ , \partial_z)^2H,$$
$$(A, B, C, F, G, H)(\partial_x H, \partial_y H, \partial_z H)^2;$$

and in particular if the second term be eliminated, we obtain the equation

$$\Pi_2=5(A, B, C, F, G, H)(\partial_x H, \partial_y H, \partial_z H)^2-3(A', B', C', F', G', H')(\partial_x U, \partial_y U, \partial_z U)^2,$$

and the equivalence of the two forms $\Pi_1=0$ and $\Pi_2=0$ is thus established.

But HESSE'S theorem leads also to the demonstration of the equivalence of the third form $\Pi_3=0$. To use it for this purpose, I remark that if $(a'', b'', c'', f'', g'', h'')$ are the second differential coefficients of $H-3H_1$, where after the differentiations x_1, y_1, z_1 are to be replaced by (x, y, z), then the theorem gives

$$12U.(A'', B'', C'', F'', G'', H'')(\partial_x, \partial_y, \partial_z)^2U-9(A'', B'', C'', F'', G'', H'')(\partial_x U, \partial_y U, \partial_z U)^2$$
$$=(a'', b'', c'', f'', g'', h'')(x, y, z)^2.(A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2(H-3H_1)$$
$$-(A, B, C, F, G, H)(a''x+h''y+g''z,\ h''x+b''y+f''z,\ g''x+f''y+c''z)^2.$$

But on putting (x, y, z) for (x_1, y_1, z_1) we have (since H is a homogeneous function of the order 6, and H_1 before the change is a homogeneous function of the order 3 in (x, y, z)) $a''x+h''y+g''z=5\partial_x H-3.2\partial_x H_1=5\partial_x H-3\partial_x H$ (since, on making the substi-

tution, $H_1=H$, but $\partial_x H_1=\frac{1}{4}\partial_x H)=2\partial_x H$; and thus

$$(a''x+h''y+g''z,\ h''x+b''y+f''z,\ g''x+f''y+c''z)=(2\partial_x H,\ 2\partial_y H,\ 2\partial_z H);$$

and similarly, on making the substitution,

$$(a'',\ b'',\ c'',\ f'',\ g'',\ h'')(x,\ y,\ z)^3=6.5H-3.3.2H_1=(30-18)H=12H.$$

Hence writing therein $U=0$, the foregoing equation becomes

$$\begin{aligned}-9(A'', B'', C'', F'', G'', H'')(\partial_x U,\ \partial_y U,\ \partial_z U)^2\\ =12H.(A, B, C, F, G, H)(\partial_x\ ,\ \partial_y\ ,\ \partial_z)^4(H-3H_1)\\ -4(A, B, C, F, G, H)(\partial_x U,\ \partial_y U,\ \partial_z U)^3,\end{aligned}$$

which may also be written

$$\begin{aligned}-9(A'', B'', C'', F'', G'', H'')(\partial_x U,\ \partial_y U,\ \partial_z U)^2\\ =12H.(A, B, C, F, G, H)(\partial_x\ ,\ \partial_y\ ,\ \partial_z)^2H\\ -36H.(A, B, C, F, G, H)(\partial_x\ ,\ \partial_y\ ,\ \partial_z)^2H_1\\ -4\quad(A, B, C, F, G, H)(\partial_x U,\ \partial_y U,\ \partial_z U)^3,\end{aligned}$$

where (x_1, y_1, z_1) are ultimately to be replaced by (x, y, z). The second line in fact vanishes, which I show as follows:—

Demonstration of my Theorem for a Cubic Curve.

Let $U=(*)(x, y, z)^3$ be a cubic function; it may by a linear transformation of the coordinates be reduced to the canonical form $x^3+y^3+z^3+6lxyz$, and we then have

$$\begin{aligned}(A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2H\div 6^2\\ =\quad (yz-l^2x^2).-6l^2x\\ +\ (zx-l^2y^2).-6l^2y\\ +\ (xy-l^2z^2).-6l^2z\\ +2(l^2yz-lx^2).(1+2l^3)x\\ +2(l^2zx-ly^2).(1+2l^3)y\\ +2(l^2xy-lz^2).(1+2l^3)z\\ =-\quad 18l^2xyz\quad +6l^4(x^3+y^3+z^3)\\ +6l^2(1+2l^3)xyz-2l(1+2l^3)(x^3+y^3+z^3)\\ =(-12l^2+12l^5)xyz+(-2l+2l^4)(x^3+y^3+z^3)\\ =\quad 2(-l+l^4)(x^3+y^3+z^3+6lxyz).\end{aligned}$$

Or since $-l+l^4$ is equal to the quartinvariant S, and the equation is an invariantive one, we have for any cubic function whatever

$$(A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2H\div 6^2=2S.U,$$

which is the theorem in question. There is a difference of notation, and consequently a

different numerical factor in the theorem, as stated in the memoir on the conic of five-pointic contact, referred to above.

If, as above, U is a quartic function $(*)(x, y, z)^4$, and $U_1=\frac{1}{4}(x_1\partial_x+y_1\partial_y+z_1\partial_z)U$, then U_1 is a cubic function, and we have

$$(A_1, B_1, C_1, F_1, G_1, H_1)(\partial_x, \partial_y, \partial_z)^2H_1 \div 6^2=2S_1 . U_1,$$

where it is to be noticed that S_1 denotes a quartic function in the coefficients of U_1, and consequently a quartic function in (x_1, y_1, z_1), the coefficients being quartic functions of the coefficients of U. On writing (x, y, z) in the place of (x_1, y_1, z_1), S_1 becomes a quartic function of (x, y, z), which is in fact a quarticovariant quartic of U.

If in the foregoing equation we write (x, y, z) in the place of (x_1, y_1, z_1), then U_1 becomes equal to 2U; and consequently, if U=0, the right-hand side of the equation vanishes. Moreover $(a_1, b_1, c_1, f_1, g_1, h_1)$ (the second differential coefficients of U_1) become equal to (a, b, c, f, g, h), and consequently the coefficients $(A_1, B_1, C_1, F_1, G_1, H_1)$ become equal to (A, B, C, F, G, H). Hence, assuming always that U=0, the equation becomes

$$(A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2H_1=0,$$

where after the differentiations (x_1, y_1, z_1) are replaced by (x, y, z). This is the form which is required for the present purpose.

Returning to the foregoing expression of $-9(A'', B'', C'', F'', G'', H'')(\partial_x U, \partial_y U, \partial_z U)^2$, this now becomes

$$\begin{aligned}-9\Pi_2 &= -9(A'', B'', C'', F'', G'', H'')(\partial_x U, \partial_y U, \partial_z U)^2 \\ &= 4\{3H . (A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2H-(A, B, C, F, G, H)(\partial_x U, \partial_y U, \partial_z U)^2\},\end{aligned}$$

so that the equation $\Pi_2=0$ gives

$$\Pi_1=(A, B, C, F, G, H)(\partial_x U, \partial_y U, \partial_z U)^2-3H . (A, B, C, F, G, H)(\partial_x, \partial_y, \partial_z)^2H=0,$$

and the equivalence of the equations $\Pi_1=0$ and $\Pi_2=0$ is thus established.

XVIII. *Electro-Physiological Researches.*—Eleventh Series. *On the Secondary Electromotor Power of Nerves, and its Application to the Explanation of certain Electro-Physiological Phenomena.* By Signor CARLO MATTEUCCI. *Communicated by* General SABINE, *Treas. and V.P.R.S.*

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THE object of this memoir is to describe experiments which prove that whenever a nerve is traversed by an electric current, it acquires in all its points a secondary electromotor power, and consequently becomes capable of producing in a conducting homogeneous circuit, whose extremities touch any two points whatever of that nerve, an electric current in a contrary direction to that of the current which we shall call the exciting current.

This property of nerves, which, as we shall see, is independent of their vital faculties, is nevertheless connected with their structure, and ceases when the integrity of that structure is impaired. All porous bodies, whether organic or inorganic, when saturated with a conducting liquid, are capable of acquiring a secondary electromotor power, so as to become a sort of secondary pile of RITTER; but I do not enter into an examination of these phenomena, which have been studied in their generality by other physicists, my principal aim being to determine exactly the conditions of the secondary electromotor power of nerves, in order to make a due application of these conditions to the explanation of the electro-physiological phenomena which are awakened at the opening of the voltaic circuit.

I shall begin by giving such a minute description of the experimental arrangements which I have followed as may enable others to repeat my experiments with ease, and furnish, as I believe, an unfailing method for conducting electro-physiological researches in general.

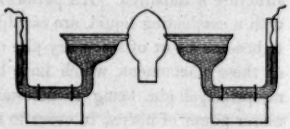
There is but one special instrument required in these researches, namely, a very delicate galvanometer, of from 24,000 to 30,000 coils of fine wire.

Many years ago I introduced in electro-physiological experiments the use of amalgamated zinc plates as extremities of the galvanometer, which may be employed much more easily and surely than the plates of distilled zinc proposed by JULES REGNAULT. I used also two small glasses, inside of which were fixed two thick strips composed of a great many layers of unsized paper or of flannel, forming a sort of cushion bent horizontally over the rim of each glass, like the cushions employed by DU BOIS REYMOND: these glasses were filled with a saturated solution of sulphate of zinc, into which were plunged the plates of amalgamated zinc. Before beginning the experiment, the two glasses were brought near to each other, so that the cushions were put in contact. If the slightest

sign of current was indicated by the galvanometer, the plates of zinc were amalgamated afresh, the cushions were carefully washed by agitating the glasses in a solution of sulphate of zinc, and the solution in the glasses was renewed. These preliminary arrangements having been made, the piece of muscle or nerve, the electromotor power of which was to be examined, was laid on a flat handle-shaped piece of gutta percha, and brought into contact with the two extremities of the cushions. After a certain number of experiments, between each of which it is necessary to ascertain that no current is developed when the cushions are put in immediate contact, it frequently happened that a certain alteration began to manifest itself; so that before the experiment could be continued, the zinc plates had to be amalgamated afresh, the cushions washed, and the solution in the glasses renewed as above described.

I have latterly succeeded in introducing some useful modifications in this method of experimenting, by means of which much trouble is saved, and the experiments are so simplified as to be executed rapidly, and at the same time with exactness.

Instead of two glasses, I now employ two tubes bent in the form of a U, one branch or arm of which is much larger than the other, and terminates like a funnel furnished with a broad flattened beak. These tubes are nearly filled with an amalgam of zinc, so dense as to be almost solid. A copper wire united to the galvanometer is inserted into the smaller branch of each tube and immersed in the amalgam. The large wide-mouthed branches of the tubes are filled up to the brim with the usual saturated neutral solution of zinc, so that the liquid extends in a very thin stratum over their flattened beaks. It is easy to lay on these beaks a single stratum of unsized paper, which becomes instantly soaked, and can be renewed without the least difficulty. Thanks to this improvement in the way of operating, I have been able to carry on a series of experiments for months together without any sign of the currents which used so frequently to be produced between the liquids in the glasses; there is no longer any need of amalgamating afresh the zinc plates, and when the solution in the tube requires renewal the operation is quickly performed.

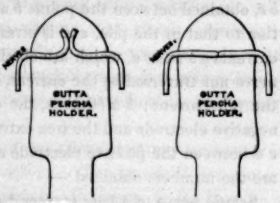


I shall describe, finally, an important part of the method pursued in these researches for comparing the electromotor power of different animal parts. This method (already well known, and which I have always followed in my electro-physiological researches*) is independent of the resistance of the elements themselves, and of the influence of time on animal structures; it consists in opposing two electromotor elements, and in observing the direction and intensity of the differential currents thus obtained. The two elements are laid on the gutta-percha holder, so as to place the two poles of the same name in contact. If, for instance, a comparison is to be made between the electromotor power of two pieces of frogs' thighs, a double pile is formed by bringing into contact the two transverse sections and closing the circuit, or *vice versâ* by touching the corresponding

* Philosophical Transactions, 1845.

extremities of the external surface of the two muscles with the extremities of the galvanometer. Thus it is found that the electromotor power of eight or ten elements or pieces of nerve of the same length is equal to that of a piece of muscle of the same length taken from the same animal.

I shall now describe the principal experiment made with the view of proving the development of secondary polarities in a nerve. Before commencing the experiment, I ascertained that no sign of current was obtained by touching any two points of a nervous filament, equidistant from its extremities, with the extremities of the galvanometer. I also ascertained that two sciatic nerves of a fowl or a rabbit, joined together either by bringing into contact the two distal extremities or the two proximal extremities, gave no sign whatever of current if these two nerves had not previously been subjected to the passage of electricity, and if the experiment had been properly prepared. By due preparation of the experiment, I mean it to be understood that the contact between the two pieces of nerve ought to be either between the two surfaces or the two transverse sections of these nerves, and never between the surface of the one and the transverse section of the other. In order to ensure this result, the two pieces of nerve should be laid on the gutta-percha holder, so as to establish the contact either of their surfaces or of their transverse sections on the middle of the holder, while the two opposite extremities of the nerves hang down over the two opposite sides of the holder.

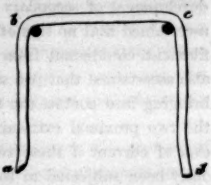


The leading experiment is performed either by laying a nervous cord upon two platinum wires, or by placing this cord upon two cushions of flannel or paper soaked with spring water, communicating with the poles of a pile. The experiment may also be made by preparing a fowl and a frog so as to leave their limbs united to the two sciatic nerves, and these nerves to a portion of the spine. An electric current from a pile of eight or ten elements is then sent through the nerves for a longer or shorter time. After the nerves have been thus traversed by the current, they are laid on the gutta-percha holder and put into communication with the galvanometer. The needle deviates, and indicates that the nerve is traversed, in the portion which was placed between the two electrodes, by a current the direction of which is opposite to that of the pile, and which lasts a certain time.

Signs of secondary current are also obtained by touching with the extremites of the galvanometer those portions of the nerve which have not been traversed by the current, that is, between the points touched by the electrodes and the points of the nerve which hang outside of the circuit. It is important to observe that the direction of the currents thus obtained is the same as that of the pile-current between the electrodes. I have constantly remarked that the current thus obtained between the points in proximity to the negative electrode and the external portion of the nerve on that side, is much

stronger than the current obtained between the points touched by the positive electrode and the external portion of the nerve towards this electrode.

In order to render this result more evident, I give the numbers obtained in several experiments in which the nerve had been subjected to the passage of the current, being laid upon two cushions. For the sake of brevity, let *a*, *b*, *c*, *d* be the nervous cord laid on the electrodes of a pile, these electrodes being indicated by *b* and *c*. After the passage of the current, I touch the extremities of the galvanometer with the points *b c*, or *b a*, or *c d* of the nerve. In all these cases the secondary current *b c*, obtained between the points *b* and *c*, has a contrary direction to that of the pile, and is invariably the strongest. The currents *a b* and *c d*, which are obtained in the portions of the nerve not traversed by the current, have the same direction as the pile-current; *b a* (that is, the current obtained between the points touched by the negative electrode and the free extremity of the nerve) is always greater than the current *c d* between the positive electrode and the other extremity of the nerve. The following are the numbers obtained:—



Sciatic nerve of a frog traversed during 60″ by a current of eight small elements (zinc, carbon, and salt water):—

Current *b c*	45°
a b	10
c d	4

Sciatic nerve of a sheep with a pile of eight elements during 60″:—

Current *b c*	72°
a b	37
c d	4

Sciatic nerve of a rabbit with a pile of eight elements for 60″:—

Current *b c*	70° to 80°
a b	35
c d	10

I give also the numbers obtained in experiments in which, instead of cushions as extremities of the galvanometer, I employed two platinum wires carefully depolarized between each experiment.

Sciatic nerve of a fowl with a pile of eight elements for 2 minutes:—

Current *b c*	25°
a b	16
c d	6

Sciatic nerve of a sheep in the same conditions as the preceding experiment:—

Current *b c*	90°
a b	38
c d	6

I shall not, as already said, enter here into particular considerations as to the production of secondary polarity in certain bodies more or less analogous to nerves, and which physically may be regarded as a solid porous mass the cavities of which are full of a conducting liquid. The general conditions required for the development of the secondary electromotor power of nerves leave no doubt as to the interpretation of this phenomenon: in the points of a nerve touched by the electrodes of a pile, the products of electrolysation are accumulated, and from thence spread through the tissue more or less, according to differences of its structure and chemical composition. The direction of the secondary current in a nerve is the same as that which is obtained after having sent a current through a strip of paper or flannel steeped in a weak saline solution, or, still more simply, by wetting two points of this strip, namely that which corresponds to the negative pole with an acid, the other, corresponding to the positive pole, with an alkaline solution, and then closing the circuit by touching with the extremities of the galvanometer either two intermediate points, or two points outside of those traversed by the current.

The object of this memoir is, I repeat, the study of secondary electromotor power in nerves, with a view to its application to electro-physiology. The principal experiment succeeds perfectly on the entire nerve of a living animal. It is easy to lay bare on a rabbit or fowl a long piece of the sciatic nerve, and to subject this nerve to the passage of a current; when the points of the nerve which have been traversed by the current are put in contact with the cushions of the galvanometer, currents owing to secondary polarities are immediately obtained.

By employing the differential method already described, I have also been enabled to prove that the secondary electromotor power of nerves is independent of their state of vitality. Thus I have compared two sciatic nerves of fowls, the one taken the instant the animal is killed, the other four days after death. These nerves, after being cut exactly of the same length, were placed one after another in the same conditions, and traversed by the same current for an equal time, after which they were laid on the gutta-percha holder and in opposition: no sign of differential currents was obtained.

I take the sciatic nerve of a sheep, measuring 210 millims., and lay its extremities on the electrodes of the pile. It is easy to imagine, without the aid of a figure, how a commutator may be employed in this experiment in order to close the circuit upon the nerve, either with the pile or with the galvanometer, by a rapid movement of the instrument. With the aid of this commutator, in a very small fraction of a second I have sent a current of from eight to ten of GROVE'S elements through this long nerve, and this has sufficed to develope the secondary electromotor power in all points of the nerve. I have frequently repeated this experiment, touching successively different points of a long nerve with the extremities of the galvanometer kept at an equal distance. I obtained a secondary current in all these points, but as to intensity the results were not constant.

As far back as the time of my first experiments, I had observed that the secondary current obtained between two given points of the nerve differed according to whether

the experiment was made before or after having touched the nerve in other points; consequently in all my subsequent experiments I have never employed any other method than the differential method which I have already described.

I prepare on a fowl two sciatic nerves, and lay them side by side on the two cushions of glasses filled with spring water, in which I plunge the electrodes of a pile of ten small elements (zinc, charcoal, and salt water). The current divides itself equally between the two nerves. I leave the circuit closed for a time, varying from five to twenty minutes. I then lay the two nerves in opposition on the gutta-percha holder, and bring them into communication with the galvanometer. No trace of differential current is to be detected, while each of these nerves gives a secondary current of from 40° to 50°.

I shall now describe briefly the results obtained by studying the influence exercised by various physical and chemical conditions, to which the nerve was subject, on its electromotor power.

I prepare in the usual way two sciatic nerves of a fowl. I put one of these nerves into a glass tube, which is left for fifteen minutes in a refrigerating mixture; at the end of this time the nerve thus cooled is exposed to the air until it acquires the same temperature as the other nerve. Both nerves are then disposed so as to be traversed by the same current, after which they are reunited in opposition and tested at the galvanometer. I find that the secondary electromotor power of the nerve which had been cooled had suffered great diminution.

I take two similar sciatic nerves, and immerse one of them in water at +50° or 60° C., leaving the other nerve intact. I send a current, as in the former experiment, through both nerves, and find the secondary electromotor power strongest in the nerve which was not heated.

I prepare on another fowl two other sciatic nerves; I crush or compress one, and leave the other intact. After these two nerves have been simultaneously subjected to the passage of the current, the secondary electromotor power is greatly weakened in the nerve which has been crushed.

Portions of sciatic nerve which have been held for a few minutes in an alkaline solution containing $\frac{1}{1000}$ weight of potassa, lose completely their secondary electromotor power. Immersion of the nerve in alcohol produces the same result. If the nerve is kept for a very long time in distilled water, the secondary electromotor power is weakened. It is remarkable that the time which may elapse after the passage of the electric current in the nerve and the washing of the nerve in water, exercises no influence on the secondary electromotor power of the nerve. If two sciatic nerves are exposed to the same current, but successively, no difference is found between them even when there has been an interval of from fifteen to twenty minutes between the electrolysation of the two nerves. After having subjected two similar nerves to the passage of the current, if one of them is afterwards held for a few seconds in distilled water, its secondary electromotor power is unaltered.

A nerve subjected to the passage of the current, first in one direction and then in

the contrary direction, for an equal time, acquires in the second case a weaker secondary electromotor power than it would have acquired had it been taken in its natural condition.

The duration of the passage of the current increases, within certain limits, the secondary electromotor power of a nerve. I have proved the influence of the intensity of the current, by putting successively in the circuit a single nerve, and then two nerves joined together, so that the intensity of the current was always half for each of the latter. This experiment was made with two, six, and twelve elements of GROVE. After each experiment, I sought the differential current by confronting the nerve traversed by the whole with that traversed by the half current. The differential currents were 3°, 28°, and 38°.

I have also proved that neither the size nor the number of the nerves united by superposition exercised any influence on the secondary electromotor power. After having subjected four similar nerves to the same current, I made a differential pile, setting three of these nerves superposed in opposition to one; and no differential current was produced. I have sent the same current through the nerve of a frog, that of a lamb, and of a fowl. These nerves were of the same length; and in order to avoid the desiccation of the nerve of the frog especially, they were placed under a moistened glass bell. After they had been electrolysed, I opposed successively the nerve of the frog to that of the fowl, the nerve of the fowl to that of the lamb, and so on: I found no differential current. Yet each of these nerves, taken separately, gave a current of from 40° to 50°, due to secondary polarity.

I would again recall attention to the result obtained in studying the influence of the length of nerves. In whatever way the experiments were made, whatever might be the nerves employed, a strong differential current was constantly found in the direction of the longest piece of nerve. We shall afterwards see how this result is modified, according to whether the two species of nerve of unequal lengths to be compared have been taken near the positive or near the negative electrode; these differences do not, however, alter the general result already referred to. The experiment consists in sending the current through a long nerve, like the sciatic nerve of a lamb. This nerve is electrolysed and divided into four equal parts, three of which are left disposed as they were during the passage of the current, and the remaining one is opposed to these three. This experiment was performed by alternating the position of the pieces; a differential current of from 30° to 40° was constantly obtained, owing to the longest piece. The same effect is obtained from nerves of frogs and fowls, whether the operation of cutting the nerve has been made after being electrolysed or before. This result cannot be understood unless we admit that the secondary electromotor power, which originally is greatest in contact with the electrodes, extends successively to all the parts of a nerve traversed by the current.

I come now to the fact which I consider as most important in the application of secondary electromotor power to the explanation of certain electro-physiological phenomena. This fact is resumed in the following proposition:—

The secondary electromotor power of a nerve is not equal in all points of the nerve,

being much stronger in the portion of the nerve near the positive electrode, than in the portion near the negative electrode: this difference is greater in a nerve traversed by the current in the direction contrary to that of its ramification, than in a nerve traversed by the current in the direction of its ramification.

I have verified this proposition by experiments on sciatic nerves of fowls, frogs, and rabbits. I begin by laying two sciatic nerves of equal length on the cushions of two glasses filled with spring water. These nerves are disposed in the same way as to their ramification, that is, the voltaic current in both nerves must be either direct or inverse. After the passage of the current, the two nerves, each of which has been traversed by one half of the current, are opposed to each other in the usual way on the gutta-percha holder. I never found any indication of differential current, although each of these nerves, taken separately, gave a very strong secondary current, having the same direction whatever might be the position of the two points of the nerve touched by the extremities of the galvanometer.

In order to show the difference of secondary electomotor power taken at points of the nerve nearest to the two poles, I cut an electrolysed nerve into two equal parts, and oppose these parts to each other; a differential current of 25 to 30 and more degrees is constantly obtained from the portion of nerve nearest to the positive pole. This experiment was made on a long sciatic nerve of a lamb. After having electrolysed this nerve, I cut it into a certain number of equal parts, which I opposed to each other, and I found constantly a differential current of 25° to 30° and more from the portion of nerve near the positive electrode.

The experiment was again made on a long sciatic nerve of a lamb. This nerve, after having been electrolysed, was cut into a certain number of equal parts, which were then set in opposition two by two and tested with the galvanometer. The differential current obtained for each couple was always determined by the portion of nerve near the positive electrode; and this intensity increased with the distance which separated the two pieces of nerve considered in relation to their natural position.

The influence exercised by the direction of the current which developes secondary electromotor power in a nerve, relatively to its ramification, may be demonstrated by repeating the experiment, already so often described, upon two sciatic nerves taken from a fowl or other animal, with this difference, that the two nerves (laid side by side) must be disposed so that the current may traverse one nerve in the direction of its ramification, and the other nerve in the opposite direction. As the current is divided in half, if these nerves, after having been electrolysed and then set in opposition, present a differential current, this current must be attributed to a difference of effect corresponding to the direction of the current through the nerves relatively to their ramification. This result is obtained either by opposing the two entire nerves, or, still better, by dividing one of these nerves into two equal parts and opposing these halves.

In a great number of similar experiments performed on nerves of different animals, I have constantly found a differential current, by which it is proved that a nerve traversed

by the current in a direction contrary to that of its ramification (*inversely*, as it is called in electro-physiology) had acquired a stronger electromotor power than that acquired by a nerve traversed in the direction of its ramification. Naturally, owing to a difference in the length, and consequently in the resistance of the nerve, this differential current must be stronger when the experiment is made on the two halves of a nerve than on two entire nerves. The same result may be obtained with different arrangements of the experiment. One of these arrangements consists in taking rapidly from a fowl the two nerves of the thigh, and in disposing these nerves one after another as they were in the living animal, so that the current traverses the one in the same direction as the ramification, and the other in the opposite direction. After being traversed by the current, the nerves are put in opposition: a differential current is determined by the nerve which, for the sake of brevity, we shall call *inverse*.

Here, once for all, I observe that in making these comparative experiments it is necessary to have a galvanometer and a rheostat in the circuit, in order to obtain constantly the same pile-current.

Another analogous way of performing this experiment is to employ a prepared animal such as is used in the electro-physiological experiment which demonstrates the influence exercised by the direction of a continuous current on the irritability of a nerve. As is well known, this preparation consists of the two limbs of an animal united by the two nerves of the thighs connected with a portion of the spine. The extremities of the limbs are immersed in water contained in two glasses, together with the electrodes of the pile; and thus the current goes from one limb to the other, traversing the two nerves, inversely in the nerve next the positive electrode, and directly in the other nerve, next the negative electrode. This experiment may be made on living animals, that is, with the entire trunk, and on animals such as frogs, fowls, and rabbits recently killed, by prolonging the passage of the current from a few seconds to twenty or thirty minutes, according to its intensity. Both the nerves acquire a strong secondary polarity; but the *inverse* nerve acquires a stronger secondary electromotor power than the *direct*, and in both nerves the secondary electromotor power is greater in the portion near the positive electrode than in that which is near the negative electrode.

The object of these researches was not, as we have said, to study the production of secondary electromotor power in nerves rather than in other porous and humid bodies of various structure and chemical composition. Under this point of view it is evident that the phenomenon is complex and its analysation difficult. In the present state of science, therefore, we are unable to account for the differences presented by a nerve in its different points, according to their proximity to one pole or the other, and according to the direction in which the nerve is traversed by the current. It is possible that similar differences will present themselves in other bodies which are not organized, or taken from living animals. It is sufficient for my present object to have proved that the secondary electromotor power of a nerve requires for its development the integrity of structure of the nerve itself, but not the excitability of the living animal, and to have determined

rigorously those differences of this power which have led me to ground the explanation of the electro-physiological phenomena which take place on the opening of the circuit on a fundamental physical fact.

The arrangement which I have for a long time adopted in the study of these phenomena is well known, and has been described in several works on physics and physiology.

This arrangement consists of a frog prepared after the usual manner of GALVANI, and then cut in half at the symphysis of the pelvis. If a continuous current is passed from one limb to another for fifteen, twenty, or thirty seconds, according to the force of the current, it is known that the opening of the circuit is accompanied by violent contractions of the limb traversed by the inverse current. These contractions depend, as I showed many years ago*, on a particular state of the nerve; and in fact the contractions are obtained and continue when the circuit is interrupted by cutting the nerve near the spine, but they are no longer produced if the nerve is cut near its insertion in the muscles of the leg.

My object in this memoir has been to prove that the particular state of the nerve above described consists of secondary electromotor power, that is, in a well-known physical phenomenon.

I complete this demonstration by a very simple experiment. After having passed a current through two lumbar nerves of a fowl in the way already described, I lay the nerve of a galvanoscopic frog on different points of these nerves, precisely at the instant in which I open the circuit: I employ several galvanoscopic frogs in order to test with their nerves different points of the nerves of the fowl, and I also vary the direction of the nerves of the galvanoscopic frogs upon the nerves of the fowl. The instant that I open the circuit, the galvanoscopic frogs contract; these contractions are also produced on touching the nerves of the fowl with the galvanoscopic frogs some instants after the opening of the circuit. Thus the secondary current, the existence of which is demonstrated by the galvanometer, and which is *direct* for the nerve that has been traversed by the *inverse* current, is also demonstrated by the contractions of the galvanoscopic frog: this direction of the secondary current explains, according to the known laws of electro-physiology, the effects produced by it on the opening of the circuit.

The differences of electromotor power found in various points of the electrolysed nerve, the prevalence of this power in the portions of the nerve near the positive electrode, very probably also the different degree of this secondary electromotor power in the various *strata* which compose the interior and the envelope of the nerve, explain sufficiently the secondary current which takes place in the nerve at the opening of the circuit, and which is *direct* and most intense in the nerve which has already been traversed by the inverse current in proximity to the positive electrode.

In conclusion, in order to explain the physiological effects which accompany the opening of a circuit, we must henceforth recur to the secondary electromotor power which is developed in nerves, and to the laws of this phenomenon.

* Electro-Physiological Researches, Fifth Series, Part II.—Phil. Trans. for 1847, pp. 235, 236.

XIX. *On Liquid Transpiration in relation to Chemical Composition.*

By THOMAS GRAHAM, *F.R.S., Master of the Mint.*

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THE passage of liquids under pressure through a capillary tube is here spoken of as liquid transpiration, in accordance with the analogy of gaseous transpiration. The subject owes the development which it has already acquired chiefly to the investigations of the late Dr. POISEUILLE*. The precision of the results attainable by the mode of experimenting pursued by that physicist has been remarked on by every one who has followed him in the inquiry. The observations we owe to M. POISEUILLE and other inquirers are very numerous, but have not, so far as I am aware, been connected hitherto with any speculative views of the chemical or molecular constitution of liquids.

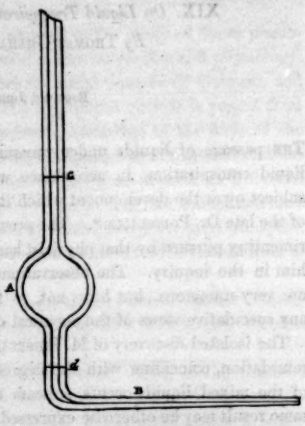
The isolated discovery of M. POISEUILLE, that diluted alcohol has a point of maximum retardation, coinciding with the degree of dilution at which the greatest condensation of the mixed liquids occurs, appears to offer a starting-point for new inquiries. The same result may be otherwise expressed, by saying that the definite compound of 1 equiv. of alcohol with 6 equivs. of water, $C_4H_6O_2+6HO$†, is more retarded than alcohol containing either a greater or a smaller proportion of water. The rate of transpiration appears here to depend upon chemical composition, and to afford an indication of it. A new physical property may thus become available for the determination of the chemical constitution of substances. Methylic alcohol being found to exhibit the same remarkable feature in its transpiration, although the 6-hydrate of that alcohol is not distinguished by extraordinary condensation of volume, the inquiry was extended to the hydrated acids. The results obtained with the latter substances give a certain degree of generality to the relation subsisting between the transpirability and chemical composition of liquids.

The apparatus employed was very similar to that of M. POISEUILLE. It consisted of a small but rather stout glass bulb, A (see figure), about two-thirds of an inch in diameter, having a capacity of from 4 to 8 cub. cent., blown upon a thick glass tube, with a bore of about 2 millimetres. A scratch (*c*) was made upon the glass tube above, and another (*d*) below the bulb, to indicate the available capacity of the instrument. The lower tube was bent at a right angle to the upper, and a fine capillary tube, B, from 3 to 4 inches in length, was sealed to the curved extremity of the tube. The bulb and capillary were always held immersed in a vessel of water during the experiment, in order to secure uniformity of temperature. The force employed to impel the liquid through the

* Mém. Savans Étrangers, tom. ix. p. 433.

† Halving the equivalent of alcohol, the hydrate of greatest retardation becomes C_2H_3O+3HO.

capillary was the weight of one atmosphere of 760 millimetres of mercury, and was obtained from compressed air contained in a large reservoir provided with a mercurial gauge, as in POISEUILLE'S experiments. The time was noted in seconds which the level of the liquid in the bulb took to fall from the mark *c* to the mark *d*. This time varied from about 300 to 900 seconds in different liquids. In successive experiments made upon the same liquid, the variation in the time, or error of observation, did not exceed one or two seconds. The experiment was always repeated two or three times, and a mean taken. The temperature of the liquid transpired was 20° (68° F.), when not otherwise stated.



The liquid may be introduced into the bulb through the open upper tube by means of a tube-funnel; but it was found more convenient in practice, although requiring a much longer time, to fill the bulb by aspiration through the capillary. With this view the compressed air was shut off by a stop-cock, and the upper tube of the bulb was then allowed to communicate with the receiver of an air-pump, instead, by which exhaustion was produced, while the open end of the capillary was immersed in a portion of the liquid. The liquid which entered the bulb in this manner was sure to be free from any solid matter which could cause obstruction in the capillary during the subsequent passage of the liquid outwards, while the disconnecting of the bulb from the rest of the apparatus, for the purpose of filling the former, was also avoided.

Nitric Acid.

A bulb provided with a capillary tube, distinguished as capillary C, was used in the transpiration of nitric acid and of several other liquids. The dimensions of this bulb C were as follows:—Capacity of bulb, 8·075 cub. cent.; length of capillary tube, 28 millims.; diameter of bore, 0·0942 millim. The time of passage of water through the tube, under the pressure of one atmosphere and at the fixed temperature of 20°, was 348 seconds. The time of the passage of the most highly concentrated nitric acid through the same capillary was found to be 344·5 seconds, or slightly less than the time of water. This is the protohydrate of nitric acid, $HO . NO_5$ or NHO_6. With the addition of water to the acid, the transpiration of equal volumes of liquid becomes gradually slower; till as much as three additional equivalents of water were added, when the transpiration-time rose to its maximum, 732 seconds. The last hydrate is the well-known definite compound NHO_6+3HO, having the specific gravity 1·4, and which possesses the highest boiling-point of any compound of nitric acid and water. Diluted beyond this point

nitric acid begins to pass more freely, and the transpiration-time approaches again to that of water. With the addition of twice its weight of water, or about 7 equivalents, the acid passed through the capillary in 472 seconds.

The experiments made upon nitric acid are recorded in the following Table. It will be observed that the retardation is considerable for a certain distance on both sides of the maximum point. No unusual retardation appears to occur with the proportions of water corresponding to 2 and 4 equivalents. The specific gravity of the acid liquid is added in the last column of the Table, whenever that property was observed.

TABLE I.—Transpiration of Nitric Acid, at 20° C., by Capillary C*.

(Transpiration-time of water, 348 seconds.)

Water added to 100 acid (NHO_6).	Water, per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water = 1.	
0	0	344·5	0·9899	1·5046
25·47	20·38	692	1·9885	1·4358
28·56 ... 2 eqs. HO	21·43	705	2·0258	
30	23·07	712	2·0459	
40	28·50	725	2·0833	
42·85 ... 3 eqs. HO	29·99	732	2·1034	1·3978
45	31·03	730	2·0977	
50	33·33	728·5	2·0919	1·3816
55	35·48	718	2·0632	
57·12 ... 4 eqs. HO	36·35	712	2·0459	
60	37·50	709·5	2·0387	1·3598
70	41·17	683	1·9626	1·3407
80	44·44	661	1·8994	1·3239
90	47·36	635·5	1·8261	
100	50·00	593	1·7040	1·2943
200	66·66	472	1·3563	

It appears, then, that a certain hydrate of nitric acid is marked out by its low transpirability so distinctly, that nitric acid could be identified by that physical property. Such a property may prove to be typical of a class of acids to which nitric acid belongs. The hydration of nitric acid probably advances by three equivalents at a time, NHO_6+3HO, as it does in the magnesian nitrates, $NMO_6+3HO+3HO$. The transpiration of the assumed second hydrate of nitric acid was not made the subject of experiment. A certain steadiness is observed in the transpiration of this acid on either side of the point of maximum retardation.

* In the following Tables, the particular capillary employed is in each case designated by a particular letter. Capillary C, which was more employed than any other, became reduced in length during the course of the experiments, the end being ground off on several occasions on account of the choking of the tube. This capillary is then described as C shortened. It did not seem requisite to give in every case the dimensions of the bulb and capillary tube, as all the experiments were conducted on the same plan, and the transpiration of water is in every case given as a standard of comparison. Direct experiments were also made, which proved that the transpiration-times were sensibly inversely proportional to the effective pressure applied to the liquid, as found by POISEUILLE; which indicates that the capillaries offered sufficient resistance to the passage of the liquid.

Sulphuric Acid.

TABLE II.—Transpiration of Sulphuric Acid, at 20°, by Capillary G.
(Transpiration-time of water, 109 seconds.)

Water added to 100 acid (SHO_4).	Water, per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water=1.	
0	0	2360	21·6514	1·8456
2·5	2·43	2412	22·1284	1·8398
5	4·76	2451	22·4862	1·8346
10	9·09	2516	23·0825	1·8120
12·5	11·11	2548	23·3761	1·7976
15	13·04	2587	23·7340	1·7800
17·5	14·89	2591	23·7706	
18·36 ... 1 eq. HO	15·13	2466	22·6238	1·7590
20	16·66	2398	22·0000	1·7473
30	23·07	1523	13·9724	1·6700
36·73 ... 2 eqs. HO	26·86	1189	10·9090	1·6335
40	28·50	1056	9·6880	1·6146
50	33·33	810	7·4302	1·5600
60	37·50	626	5·7431	1·5118
70	41·17	535	4·9082	
80	44·44	480	4·1284	
100	50·00	382	3·5045	
120	54·54	332	3·0458	
140	58·33	290	2·6605	
160	61·53	260	2·3889	
180	64·28	241	2·2110	
200	66·66	227	2·0825	

The transpiration of sulphuric acid is very slow, being twenty-four times less rapid than that of water, as might be expected from the viscous quality of the acid fluid. It is surprising, however, that the first additions of water do not promote the transpiration, although they lessen in a sensible degree the viscosity of the liquid. The transpiration-time increases from 2360 to 2591 seconds, and then attains the maximum, when 17·5 parts of water have been added to 100 parts of oil of vitriol. The proportion of water named approaches closely to 1 equivalent (18·36 parts). Indeed it is quite possible that the acid mixture which exhibits the least transpirability might have contained a full equivalent of water, for a portion of aqueous vapour may have been absorbed from the air during the process of filling the bulb. That the crystallizable hydrate of sulphuric acid, SHO_4+HO, is the liquid of least transpirability is, I believe, the proper inference from these observations. With increasing proportions of water the transpiration-time rapidly diminishes, till the time is reduced to 227 seconds in a mixture of oil of vitriol with twice its weight of water.

A more minute examination than has been attempted would be required to show whether the existence of other definite hydrates of sulphuric acid may be indicated by a perceptible retardation in the time of transpiration.

Acetic Acid.

TABLE III.—Transpiration of Acetic Acid, at 20°, by Capillary C.

(Transpiration-time of water, 348 seconds.)

Water added to 100 acid ($C_4H_4O_4$).	Water, per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water = 1.	
0·8	0·8	445·5	1·2801	
15 ... 1 eq. HO	13·04	890	2·5574	1·0735
20	16·66	921·5	2·6480	1·0742
25	20·00	931	2·6753	
27·5	21·56	933	2·6810	
30 ... 2 eqs. HO	23·07	941	2·7040	1·0752
32·5	24·52	934	2·6839	1·0746
35	25·92	928	2·6666	
40	28·50	912	2·6207	
45	31·04	895	2·5718	
50	33·33	882	2·5344	1·0720
60 ... 4 eqs. HO	37·50	852	2·4482	1·0700
90 ... 6 eqs. HO	47·36	769	2·2098	

The glacial acetic acid made use of in these experiments still retained 0·8 per cent. of water. Its transpiration-time was 445·5 seconds. With the addition of 1 equiv. of water the time rose to 890 seconds; and with 2 equivs. of water to 941 seconds, when it attained its maximum. This last is the characteristic hydrate of acetic acid, $C_4H_4O_4+2HO$. It is marked out with great precision in these transpiration experiments. The times rise very gradually on either side, and appear to culminate exactly at that point. It is also the compound of water and acetic acid of maximum density, as is well known. The transpiration-time of the hydrate referred to is so much as 2·7 times longer than that of pure water. With 6 equivalents of water acetic acid is still transpired 2·2 times more slowly than water.

Butyric Acid.

TABLE IV.—Transpiration of Butyric Acid, $C_8H_8O_4$, at 20°, by Capillary C shortened.

(Transpiration-time of water, 290 seconds.)

Water added to 100 acid ($C_8H_8O_4$).	Water, per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water = 1.	
0	0	454	1·565	·9740
10·22 ... 1 eq. HO	9·27	828	2·855	·9901
20·45 ... 2 eqs. HO	16·98	951	3·279	·9975
30·67 ... 3 eqs. HO	23·47	969	3·341	
38·69 ... 4·8 eqs. HO	27·85	863	2·975	

In the transpirability of its hydrates butyric acid presents a considerable analogy to acetic acid, as might be expected from the relation of these acids in composition. The time of the acid ($C_8H_8O_4$) is 1·565, referred to that of water as 1, and it rises to 2·855 by the addition of 1 equivalent of water. By a second equivalent of water the time is increased to 3·279. Here, however, the progression does not immediately turn, as with

acetic acid, but the time rises to 3·341 with 3 equivalents of water. With 3·8 equivalents of water the time is 2·975, and has accordingly very sensibly receded, the maximum point being passed. It is conceivable that the relation to acetic acid is slightly modified in butyric acid by the interference of some other physical property, such as unctuosity, that is unequally developed in the two acids.

Valerianic Acid.

The hydration of this acid cannot be carried beyond 2 equivalents, but up to that point the transpiration is retarded by every addition of water, as in acetic and butyric acids. While the basic hydrate ($C_{10}H_{10}O_4$) is transpired in 2·155 times the water period, the time increases to 3·634 with 1 equivalent of water added, and to 3·839 with 2 equivalents.

TABLE V.—Transpiration of Valerianic Acid, at 20° C., by Capillary C shortened.

(Transpiration-time of water, 290 seconds.)

Water added to 100 acid ($C_{10}H_{10}O_4$).	Water per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water = 1.	
0	0	625·2	2·155	·9350
8·82 ... 1 eq. HO	8·10	1054	3·634	·9484
17·64 ... 2 eqs. HO	15·84	1113·5	3·839	·9519

Formic Acid.

Formic acid appears to diverge considerably from the other members of the acetic acid series in certain physical and chemical characters. While the acetic hydrate is lighter than water, and is increased in density by the addition of water, the formic hydrate has a higher density than water, and has its density uniformly lowered by dilution, as will be seen in the Table which follows. The transpiration-time of formic acid is also highest in a concentrated state, and diminishes with dilution in the same regular manner as the density, showing no evidence of the acetic maximum at the point of 2 equivalents of water. Indeed, formic acid does not appear to affect that particular degree of hydration so characteristic of the acetic acid series. Hence it is, also, that we have no subformiate of lead corresponding with the subacetate of lead, and have occasion to remark a general absence of basic formiates. The physical properties of liquid formic acid are more suggestive of hydrochloric acid than they are of acetic acid.

The most concentrated formic acid that could be prepared still contained 3·6 per cent. of water. The transpiration-time of that liquid, it will be seen, is 1·718 referred to water as 1; and of the 2-hydrate 1·486. There is evidence of retardation between the points of 3 and 4 equivalents of water, but it is difficult to say with which of these two hydrates the retardation should be connected. More numerous and minute observations would be required to settle the point. We can only draw the negative conclusion from the Table, that the maximum retardation does not coincide with the 2-hydrate as in acetic acid.

TABLE VI.—Transpiration of Formic Acid, at 20°, by Capillary C shortened.
(Transpiration-time of water, 293 seconds.)

Water added to 100 acid ($C_2H_2O_4$).	Water, per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water = 1.	
3·73	3·6	503·5	1·718	1·2265
19·56 ... 1 eq.	16·35	484·5	1·653	1·2019
39·13 ... 2 eqs.	20·93	435·5	1·486	1·1765
58·69 ... 3 eqs.	36·98	411	1·402	1·1524
68·47 ... 3·5 eqs.	40·64	401·5	1·368	1·1466
78·26 ... 4 eqs.	43·90	402·5	1·372	1·1408
97·82 ... 5 eqs.	49·44	388·5	1·325	1·1275
117·35 ... 6 eqs.	53·99	376·5	1·284	1·1203
136·95 ... 7 eqs.	57·79	359	1·225	1·1062

Hydrochloric Acid.

The most concentrated form of this acid that was dealt with, acid of sp. gr. 1·1553, contained already upwards of 8 equivalents of water. Its transpiration-time was 1·7356, referred to the time of water as 1. With further dilution the time diminished, till at the proportion of 12 equivalents of water the time had fallen to 1·5287. About this point the rate of diminution is reduced, and the transpiration-time even becomes stationary for a short portion of the range of hydration. The retardation observed appears to coincide with the formation of a 12-hydrate of hydrochloric acid. The existence of such a compound is further supported by the fact that solutions of hydrochloric acid tend to the same composition by evaporation at the atmospheric temperature. The degree of hydration of most stability at high temperatures, and having the highest boiling-point, is known to be at or near the proportion of the 16-hydrate. The existence, however, of the latter hydrate, at the ordinary temperature, is not supported by the transpiration experiments now recorded, conducted as these were at a low temperature.

TABLE VII.—Transpiration of Hydrochloric Acid, at 20°, by Capillary C.
(Transpiration-time of water, 348 seconds.)

Water added to 100 acid (HCl).	Water, per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water = 1.	
221·8	69·23	604	1·7356	1·1553
250	71·42	569	1·6336	1·1411
280	73·67	536	1·5404	1·1303
290	74·36	532	1·5287	
295·89 ... 12 eqs. HO	74·74	532	1·5287	1·1246
300	75·00	520	1·4942	
310	75·60	516	1·4827	1·1202
380	79·20	486	1·3965	1·1021
394 ... 16 eqs. HO	79·97	479	1·3764	1·0992
410	80·39	469	1·3476	1·0961

Alcohol.

The fundamental discovery made by Poiseuille of a point of maximum retardation in the transpiration of diluted alcohol is fully confirmed in the following series of observations. The transpiration-time rises from that of absolute alcohol, 1·1957 (water being 1), to 2·7872, when the alcohol is united with 6 equivalents of water, and then falls off again by further additions of water.

Table VIII.—Transpiration of Alcohol, at 20°, by Capillary D.
(Transpiration-time of water, 470 seconds.)

Water added to 100 Alcohol.	Water, per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water = 1.	
0	0	562	1·1957	
1	0·99	578	1·2297	·7069
3	2·91	615	1·3085	·8030
5	4·76	650	1·3829	·8083
7	6·54	695	1·4787	
10	9·09	734	1·5617	
20	16·66	851	1·8106	·8396
30	23·07	950	2·0212	·8557
40	28·50	1029	2·1893	·8683
50	33·33	1093	2·3253	·8800
60	37·50	1152	2·4510	·8897
70	41·17	1213	2·5808	·8983
72·5	42·02	1230	2·6170	·9003
75	42·85	1231	2·6191	·9021
78·26 ... 4 eqs. HO	43·94	1239	2·6361	·9045
80	44·44	1238	2·6340	·9058
82·5	45·20	1242	2·6425	·9073
85	45·94	1244	2·6468	·9088
90	47·36	1256	2·6723	·9120
100	50·00	1268	2·6978	·9183
110	52·38	1282	2·7276	·9235
112·5	52·94	1287	2·7382	·9249
115	53·49	1298	2·7617	·9255
117·39 ... 6 eqs. HO	54·04	1310	2·7872	·9271
120	54·54	1307	2·7808	·9288
122	55·05	1300	2·7659	·9292
125	55·55	1297	2·7595	·9304
130	56·52	1297	2·7595	·9328
140	58·33	1295	2·7553	·9363
150	60·00	1280	2·7234	·9396
160	61·53	1255	2·6702	·9430
170	62·92	1250	2·6595	·9451
180	64·28	1246	2·6510	·9482
190	65·51	1240	2·6382	·9500
200	66·66	1235	2·6276	·9521
250	71·42	1165	2·4787	·9601
300	75·00	1094	2·3276	·9652
350	77·77	1026	2·1829	·9689
400	80·00	973	2·0702	·9716
450	81·80	934	1·9872	·9738
500	83·33	908	1·9319	·9759

It will be observed that after attaining its maximum the transpiration-time falls off in a very gradual manner, till another equivalent at least of water has been added. With still further dilution the shortening of the transpiration-time is considerably more rapid. The Table appears to indicate a slight retardation at the proportion of four equi-

valents of water; but this would require confirmation. It is remarkable that hydrated liquid compounds appear in general to show only one decided transpiration maximum, as with the 1-hydrate in sulphuric acid, the 2-hydrate in acetic acid, the 3-hydrate in nitric acid, the 6-hydrate in alcohol, and the 12-hydrate in hydrochloric acid.

A considerable number of experiments were made upon specimens of *methylic alcohol* prepared at different times, with some discrepancy in the results. Although always derived from crystallized methylic oxalic ether, the liquid varied sensibly in transpirability. As the cause of this variation has not yet been ascertained, I shall confine myself at present to one statement, namely, that a particular specimen of methylic alcohol gave 0·63 as the transpiration-time of the anhydrous substance (water being 1), and 1·8021 as the time of the 6-hydrate, $C_2 H_4 O_2 + 6HO$, and that for a considerable distance on either side of that point of hydration the transpiration was slightly less and nearly constant, as it is in vinic alcohol. It may be inferred, therefore, with some probability, that alcohols have a maximum of retardation at the same stage of dilution.

Three alcohols in a state of purity were transpired through the same capillary, with water for comparison, at 20°. The time of water was 297 seconds.

TABLE IX.—Transpiration of Alcohols, at 20°.

	Transpiration-time.		Specific gravity, at 15°.	Boiling-point.
	In seconds.	Water = 1.		
Methylic alcohol	187·25	0·630	·7973	66° C.
Vinic alcohol.....	355·1	1·195	·7947	78·5
Amylic alcohol	1084	3·649	·8204	132

It will be remarked that the transpiration-time of an alcohol increases with the elevation of its temperature of ebullition. A similar observation applies to the transpiration of ethers.

TABLE X.—Transpiration of Ethers, at 20°, by Capillary C shortened.
(Transpiration-time of water, 290 seconds.)

	Transpiration-time.		Specific gravity, at 15°.	Boiling-point.
	In seconds.	Water = 1.		
Formiate of ethyl	148·2	0·511	·9174	55°·5
Acetate of ethyl	160·5	0·553	·8853	74
Butyrate of ethyl	217·5	0·750	·8490	114
Valerianate of ethyl	237·5	0·827	·8750	133·5

The transpiration-times of the homologous acids, previously observed, appear also to follow in progression.

Transpiration of Acids, at 20°.

	Acid.	Acid + 2HO.
Acetic acid	1·2801	2·740
Butyric acid	1·565	3·279
Valerianic acid	2·155	3·839

The increase of the transpiration-time of an alcohol, ether, and acid, as each rises in its series, may be connected with the increasing weight of their molecule.

Acetone.

The transpiration of this liquid is remarkably rapid. It is also greatly retarded by the addition of water. The time will be found to rise from 0·401, that of anhydrous acetone, to 1·604, the time of the 12-hydrate, taking the equivalent of acetone as $C_6H_6O_2$, or of the 6-hydrate with the equivalent C_3H_3O.

TABLE XI.—Transpiration of Acetone, at 20°, by Capillary C.

(Transpiration-time of water, 348 seconds.)

Water added to 100 acetone ($C_6H_6O_2$).	Water, per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water = 1.	
0	0	139·6	0·401	·7943
15·51 ... 1 eq.	13·42	212·5	0·610	·8384
31·03 ... 2 eqs.	23·68	283·5	0·814	·8604
46·55 ... 3 „	31·76	355·5	1·021	·8850
62·06 ... 4 „	38·29	457	1·313	·8990
77·58 ... 5 „	43·68	464	1·333	·9123
85·34 ... 5·5 „	46·04	469	1·347	·9173
93·10 ... 6 „	48·21	482	1·385	·9219
100	50·00	500	1·436	·9251
108·61 ... 7 „	52·06	515·5	1·479	·9300
124·13 ... 8 „	55·33	531·5	1·527	·9320
139·65 ... 9 „	57·85	537·7	1·543	·9413
155·16 ... 10 „	60·81	552·7	1·586	·9468
170·67 ... 11 „	63·05	555·5	1·594	·9504
186·18 ... 12 „	65·05	558·5	1·604	·9526
201·71 ... 13 „	66·85	556·5	1·599	·9563
217·24 ... 14 „	68·41	557	1·600	·9588
232·75 ... 15 „	69·94	553·5	1·590	·9608
248·27 ... 16 „	71·28	549	1·577	·9632
263·79 ... 17 „	72·23	547	1·571	·9649
279·31 ... 18 „	73·63	546	1·568	·9662
294·82 ... 19 „	74·67	539·5	1·550	·9676
372·24 ... 24 „	78·82	519	1·491	·9736

The transpiration-time of acetone attains a maximum at what is represented in the Table as the compound with 12 equivalents of water. The time is nearly stationary for some distance on either side of that point, the range from 10 to 15 equivalents of water being 1·586 to 1·590, with 1·604 as a maximum for the intermediate twelfth equivalent.

Glycerine.

This liquid is too viscid in a state of purity to be transpired by means of the bulb and capillaries employed in these experiments. The observations to be recorded were confined to diluted solutions of glycerine approaching in composition to the 18-hydrate, $C_8H_8O_6+18HO$. It was imagined that glycerine as a triatomic alcohol might affect combination with water in the proportion named.

TABLE XII.—Transpiration of Glycerine, at 20°, by Capillary C.

(Transpiration-time of water at the same temperature, 348 seconds.)

Water added to 100 Glycerine ($C_3H_8O_3$).	Water, per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water = 1.	
170	62·96	1199	3·445	1·1010
176·07 ... 18 eqs.	63·77	1160	3·333	1·0980
180	64·28	1131·5	3·251	1·0960
190	65·51	1068·5	3·070	1·0934
192	65·75	1054	3·031	1·0927
195	66·10	1049	3·014	1·0914
197	66·32	1039	2·977	1·0912
200	66·66	1026	2·948	1·0905

The transpiration-time of 18-hydrate is 3·333, referred to water as 1. There is no indication of a maximum at that point, but the numbers descend according to their place in the Table without any interruption.

The idea having suggested itself that the viscous property of glycerine solutions might overpower or conceal the expected deviation, the transpiration was repeated at a higher temperature, when the solutions possess greater fluidity.

TABLE XIII.—Transpiration of Glycerine, at 60°, by Capillary C.

(Transpiration-time of water at the same temperature, 186 seconds.)

Water added to 100 Glycerine ($C_3H_8O_3$).	Water, per cent.	Transpiration-time.		Specific gravity, at 15°.
		In seconds.	Water = 1.	
170	62·96	435·5	2·341	1·1010
172·5	63·30	432	2·322	1·0999
175	63·63	428	2·301	1·0980
176·08 ... 18 eqs.	63·77	425	2·284	1·0976
177	63·96	422·5	2·271	1·0970
180	64·22	420	2·258	1·0960

Still no retardation appears at the point of 18 equivalents, but the time continues to shorten as the proportion of water is increased, according to a pretty uniform progression. The information respecting the constitution of glycerine which transpiration affords is therefore of a negative character.

The existence of a relation between the transpirability of liquids and their chemical composition appears to be established. It is a relation analogous in character to that subsisting between the boiling-point and composition, so well defined by M. KOPP. Perhaps the most interesting part of the present subject to develope would be the transpiration of homologous series of substances. Judging from the limited observations on the alcohols, ethers, and acids, the order of succession of individual substances in any series would be indicated by the degree of transpirability of these substances, as clearly as it is by their comparative volatility. In carrying out the inquiry, it would probably be

found advantageous to operate at a fixed temperature, which is somewhat elevated. A large number of substances are liquid at 100°, of which the transpiration-time could be easily obtained.

In hydrated substances transpiration also affords a manifestation of definite combination at once striking and precise. I need only refer to the manner in which the "constitutional" hydrate of sulphuric acid SHO_4+HO, of acetic acid $C_4H_4O_4+2HO$, of nitric acid NHO_6+3HO, and of alcohol $C_4H_6O_2+6HO$ is each indicated by its maximum transpiration-time. The indication of the alcohol-hydrate is particularly distinct, although that hydrate must be a comparatively feeble compound. Indeed the extent to which transpiration is affected by the annexation of constitutional water appears to be by no means in proportion to the intensity of combination.

The increased resistance to transpiration observed in these definite hydrates may be connected with their larger molecules. But another speculative view of the retardation can be suggested, in which the phenomenon is referred to a physical agency. When one of these definite hydrates, say the 6-hydrate of alcohol, is being forced through the capillary, it may be imagined that a small portion of the hydrated compound is molecularily decomposed by the friction. A certain portion of the impelling force would thereby be lost, being converted into the latent heat which alcohol and water require to assume when separated from each other, and the transpiration be consequently retarded; for as alcohol and water evolve heat on combining, so they must absorb heat when their union is dissolved by any cause. But the change of temperature representing the lost force appears to be too small to be rendered sensible to observation. It would be capable of raising the temperature of the transpired liquid not more than about one forty-third part of a degree, according to an accurate estimate for which I am indebted to Professor Stokes. In consequence of this circumstance the physical hypothesis now suggested has neither been verified nor disproved.

To this paper are appended two series of observations made on transpiration at different temperatures, the first series being the transpiration of water, and the second that of absolute alcohol. Each series of experiments is repeated with two capillary tubes, one having nearly double the resistance of the other. The numbers from the two capillaries exhibit a fair amount of agreement. The times given are those actually observed, no correction being made for the small variation of the capillary in diameter at different temperatures.

The dimensions of Capillary D were as follows:—Capacity of bulb, 4·135 cub. cent.; length of capillary tube, 37·5 millims.; diameter of bore, 0·10325 millim. Time of passage of water, at 20°, under pressure of one atmosphere, 470 seconds.

The dimensions of Capillary E were as follows:—Capacity of bulb, 3·725 cub. cent.; length of capillary, 53 millims.; diameter of bore, 0·0858 millim. Time of passage of water, at 20°, under pressure of one atmosphere, 913 seconds.

TABLE XIV.—Transpiration of Water at different Temperatures.

Temperature.	By capillary tube D.					By capillary tube E.				
	Time in seconds.	Time and velocity of water at 20° = 1.		Time and velocity of water at 0° = 1.		Time in seconds.	Time and velocity of water at 20° = 1.		Time and velocity of water at 0° = 1.	
		Time.	Velocity.	Time.	Velocity.		Time.	Velocity.	Time.	Velocity.
°										
0	840	1·7872	0·5593	1·	1·	1629	1·7872	0·5604	1·	1·
1	792	1·6851	0·5934	0·9428	1·0606	1568	1·7174	0·5829	0·9625	1·0389
2	770·5	1·6391	0·6099	0·9172	1·	1514	1·6582	0·6050	0·9294	1·
3	749	1·5936	0·6275	0·8917	1·1216	1461	1·6002	0·6249	0·8975	1·1449
4	727	1·5468	0·6465	0·8654	1·					
5	709	1·5085	0·6629	0·8440	1·1857	1382	1·5136	0·6606	0·8483	1·1787
7	669	1·4234	0·7025	0·7964	1·2556	1289	1·4118	0·7083	0·7912	1·2638
10	618	1·3148	0·7605	0·7357	1·3592	1188	1·3012	0·7685	0·7293	1·3717
14	548	1·1659	0·8576	0·6423	1·5328					
15	533	1·1340	0·8818	0·6345	1·5759	1037	1·1358	0·8804	0·6366	1·5709
16	521	1·1085	0·9021	0·6202	1·6122					
20	470	1·	1·	0·5595	1·7872	913	1·	1·	0·5604	1·7842
25	414	0·8808	1·1352	0·4928	2·0189	823	0·9014	1·0904	0·5052	1·9793
30	375·5	0·7989	1·2516	0·4470	2·2371	743	0·8138	1·2288	0·4501	2·1924
35	338	0·7191	1·3905	0·4023	2·4852	670	0·7338	1·3626	0·4113	2·4313
40	309·5	0·6508	1·5185	0·3684	2·7108	602	0·6593	1·5166	0·3695	2·7059
45	284·5	0·6053	1·6520	0·3386	2·9525	553	0·6056	1·6509	0·3394	2·9459
50	261	0·5553	1·8007	0·3107	3·2184	505	0·5531	1·8079	0·3100	3·2257
55	243	0·5170	1·9341	0·2892	3·4979	475	0·5202	1·9221	0·2916	3·4294
60	228	0·4851	2·0614	0·2714	3·6842	438	0·4797	2·0844	0·2689	3·7191
65	214	0·4553	2·1967	0·2547	3·9252	400	0·4381	2·2825	0·2455	4·0725
70	200	0·4155	2·3500	0·2380	4·2000	378	0·4140	2·4153	0·2321	4·3122

TABLE XV.—Transpiration of Alcohol at different Temperatures.

Temperature.	By capillary tube D.					By capillary tube E.				
	Time in seconds.	Time and velocity of water at 20° = 1.		Time and velocity of water at 0° = 1.		Time in seconds.	Time and velocity of water at 20° = 1.		Time and velocity of water at 0° = 1.	
		Time.	Velocity.	Time.	Velocity.		Time.	Velocity.	Time.	Velocity.
0°C.	860	1·8297	0·5465	1·0238	0·9767	1642	1·7984	0·5560	1·0079	0·9920
1	840	1·7893	0·5588	1·0012	0·9988	1601	1·7535	0·5702	0·9828	1·0174
3	807	1·7170	0·5824	0·9607	1·0409	1537	1·6834	0·5940	0·9435	1·0598
5	772	1·6425	0·6089	0·9190	1·0880	1473	1·6133	0·6198	0·9042	1·1059
7	738	1·5702	0·6368	0·8785	1·1382	1410	1·5443	0·6475	0·8656	1·1553
10	700	1·4893	0·6714	0·8333	1·2000	1350	1·4786	0·6755	0·8226	1·2066
15	624	1·3276	0·7532	0·7428	1·3461	1213	1·3285	0·7526	0·7446	1·3429
20	562	1·1957	0·8362	0·6690	1·4946	1092	1·1960	0·8360	0·6703	1·4917
25	520	1·1063	0·9038	0·6190	1·6154	1001	1·0963	0·9120	0·6145	1·6273
30	476	1·0127	0·9873	0·5566	1·7646	915	1·0022	0·9978	0·5617	1·7803
35	428	0·9106	1·0981	0·5095	1·9626	843	0·9233	1·0830	0·5175	1·9323
40	391	0·8319	1·2020	0·4654	2·1483	772	0·8455	1·1826	0·4739	2·1101
45	360	0·7659	1·3055	0·4285	2·3333	707	0·7743	1·2913	0·4340	2·3041
50	331·5	0·7053	1·4177	0·3946	2·5339	645	0·7064	1·4155	0·3959	2·5100
55	307	0·6531	1·5309	0·3654	2·7301	592	0·6484	1·5422	0·3634	2·7517
60	285	0·6063	1·6491	0·3392	2·9473	551	0·6035	1·6569	0·3382	2·9564
65	262	0·5574	1·8005	0·3119	3·2061	510	0·5585	1·7901	0·3130	3·1941
70	241	0·5127	1·9502	0·2869	3·4854	468	0·5125	1·9465	0·2873	3·4807

XX. *On the Structure and Growth of the Tooth of* Echinus. *By* S. James A. Salter, *M.B. Lond., F.L.S., F.G.S. Communicated by* Thomas Bell, *Esq., F.R.S.*

Received March 5,—Read March 21, 1861.

The researches upon which this memoir is based were prosecuted more than four years since, and the illustrations which accompany it were mostly executed at nearly as distant a date—before I was aware that this interesting and obscure subject had ever been investigated by competent observers. And though it appears that some of my own observations (independent as they were) have been anticipated, there is so much of this ground of inquiry untrodden, and the accounts published of the structures in question are so very imperfect and often incorrect, that I have been induced to put together in this paper the results of my own more extended investigations.

The *literature* of this subject is confined to very narrow limits; for though comparative anatomists, from the time of Aristotle, have not failed to describe the curious apparatus of teeth and jaws in the Echinus, I am not aware that any observer had investigated the structure and growth of the teeth themselves, or published any account of their intrinsic anatomy, before the Essay on the genus *Echinus* was written by Valentin as one portion of a general monograph on the Echinodermata published by Agassiz*, and which appeared in 1841.

The illustrations of Valentin's paper are many of them exceedingly good, as far as they refer to the anatomy of the Echinus-tooth; but this distinguished anatomist signally failed to interpret correctly the appearances he has figured, and to associate in the mature tooth-structure the elementary parts with the position they there occupy.

Valentin has figured correctly and fairly described the earliest growth of the plumule of the tooth (tab. 6, figs. 113 and 114) with its two rows of triangular plates; but he subsequently speaks of the confluent apices of these plates as constituting the keel of the matured tooth, thus inverting their direction of growth and placing them in a portion of the tooth which they never approach (figs. 115 and 116), and this too while he figures most accurately, and in an admirable illustration, the relationship of the enamel rods, the keel fibres and plates; but viewing the latter in vertical section and not knowing the previous position of the plates and plan of their arrangement, he mistakes their line-like section for fibres, and so designates them (see fig. 110).

Valentin gives no correct account or figures of the ultimate histology of the Echinus-tooth; but by some accident he represents as a portion of a tooth a very highly magni-

* Anatomie des Echinodermes. Première Monographie: Anatomie du Genre *Echinus*, par G. Valentin. Neuchatel, 1841.

fied specimen of what appears to be a piece of shell or alveolus. I am quite unacquainted with any appearances in the Echinus-tooth resembling this figure.

Professor QUEKETT in 1854 published his lectures on histology*. He there simply alludes to the mature structure of the tooth without reference to its general anatomy or mode of development.

Respecting the structure of the compact tissue, Professor QUEKETT remarks, at page 235, that the teeth "contain numerous branching tubuli very like those of dentine; many of the tubuli are so much dilated at certain points as to form lacunæ, or bone-cells, but in others the branches are extremely minute, and run in parallel lines precisely like the tubuli of dentine." This account is quite correct, but it should be observed that the dentine-like appearance is displayed by transverse section; the bone-like appearance by vertical section.

Professor QUEKETT further observes that he has failed to discover any walls to the tubular structure of the Echinus-tooth.

Dr. CARPENTER, in his work on the microscope†, published in 1856, adds no further information respecting the structure of the Echinus-tooth; but, as qualifying the implied opinion of Professor QUEKETT, he remarks that he "is disposed to think that the structure of the teeth is essentially the same as that of the shell, save in the interspaces of the network being much narrower; and that the appearance of tubuli (in which Mr. QUEKETT has not been able to make out distinct walls) is due merely to the elongation of these interspaces."—Page 519.

Professor WILLIAMSON, in December 1856, published a valuable and original paper‡ on the anatomy and structure of the Echinus-tooth in the 'British Journal of Dental Science;' but from its appearing in an obscure professional periodical, it has unfortunately been lost to science nearly altogether; the author of this memoir, indeed, only becoming accidentally aware of its existence more than a year after its publication.

Professor WILLIAMSON'S paper is one of great excellence. He does not, however, appear to have been aware how far he had been anticipated by VALENTIN, especially in the illustrations of his monograph, to which, indeed, he makes no allusion. This paper contains a good account of the minute structure of the mature tooth, fuller and more particular than that of Professor QUEKETT; but its author does not seem to have succeeded in associating in the fully-formed organ the previously seen elementary parts with their then position, or the method in which they build up this curious and complicated fabric. He observes, "It is wholly impossible by verbal descriptions to convey any idea of the intricate arrangements of plates and rods which compose this singular structure." The difficulty, however, is not in describing but in making out what that arrangement is.

* Lectures on Histology. By J. QUEKETT. London, 1854.

† The Microscope and its Revelations. London, 1856.

‡ "On the Histology of Dental and Allied Dermal Tissues of Vertebrate and Invertebrate Animals," by W. C. WILLIAMSON, F.R.S., in the 'British Journal of Dental Science.' London, 1856, page 163.

In the anatomical details of this memoir I shall have occasion hereafter to allude to Professor WILLIAMSON's paper, and I will only now add that its important new points are—the distinct assertion that the tissues of the tooth, cell-like as they are when matured, are formed without cells, being (to use his own expression) produced "by the calcification of the intercellular fluid;" and, secondly, the discovery of the little calcareous discs by which the elementary parts of the tooth are united together; for though I differ from him entirely as to the anatomical relations of these bodies, he has certainly given the first published account of them*.

It is not my intention to enter upon any description of the many structures concerned in the complicated tooth-apparatus of the Echinus; I confine myself strictly to the structure and elaboration of the teeth themselves; the other elementary parts of the apparatus being already described in works treating of the comparative anatomy of Invertebrata. I may, however, premise that the internal growing extremity of the tooth is enclosed in a little membranous shut sac, containing numerous pigmentary cells and a clear aqueous fluid. The cells do not in any way enter into the formation of the tooth-structures which are free in the fluid of the sac.

The tooth of the Echinus is a slender calcareous rod, of a definite and constant form. It may be divided for the convenience of description (though the division is artificial) into the *shaft* and "*plumule*†," the shaft being the consolidated structure, the plumule the soft growing extremity. The shaft of the tooth is nearly straight, being very slightly curved, the concavity looking (when the tooth is in its natural position) towards the alimentary canal. The plumule is more strongly bent, and forms a little ringlet at the internal extremity of the organ (see Plate VI. fig. 1). In viewing the figure referred to, it will be seen that, in this lateral aspect, the plumule is much narrower than the shaft of the tooth; and further, that in tracing the plumule into the shaft it is found to be continuous only with that portion of the latter which forms the convex part of the tooth. It is important to bear this in mind, as it assists in understanding what part of the mature tooth is composed of those structural elements which constitute the plumule, and having this knowledge the observer can more readily associate the supplemental elements with their own position in the organ when completely elaborated. The general outline of the body of the tooth is displayed by a transverse section. Before considering this, however, there is one point which is best seen in the lateral view of the entire organ and its slightly magnified vertical section, namely, the apex or external point of the tooth. Both QUEKETT and WILLIAMSON have figured and described the apex of the tooth as consisting of a sharp cutting external *edge*—chisel-shaped, like the incisor of a rodent. Such a description, however, is anatomically incorrect, and such a form is physically impossible. Truly the apex of the Echinus-tooth is retained in a definite form by the friction wear of elementary parts of different density, as a rodent's

* Unless indeed VALENTIN's figure (tab. 6, fig. 105 *a*) refers to them: if so, their regular linear arrangement, as represented by him, is incorrect.

† The term "plumule" is VALENTIN's.

is; but the arrangement of the dense and softer parts is different in the two, and consequently the resultant form of the worn extremities is different. The central condensed axis is the hardest portion of the Echinus-tooth, and this, and not its external border, suffers the least from wear. From this axis the worn surface passes obliquely downwards and outwards, and also downwards and inwards (see Plate VI. fig. 2, *a*, *b*, *d*). It not only differs from the apex of the rodent-tooth when seen in this aspect, but, when viewed on the dorsal *face*, it is seen that the extremity is a point and not a cutting border; for, from the dense central axis, the body of the tooth slopes downwards and outwards obliquely on either side. The apex of the worn tooth not only corresponds with the relative hardness of its parts, but is coincident with lines of superimposition of its component elements; and, by fracturing the tooth in its centre, where those elements are as yet not firmly soldered together, they sever, leaving the broken point similar to that produced by friction wear; indeed the worn surface seems to show that attrition separates and removes the imbricated elements of the tooth in layers, their adhesion to one another being weaker than the cohesion of their own intrinsic structure.

A transverse section of the entire tooth exhibits an outline like the letter T, the cross line constituting the *body* of the tooth, the single vertical line the *keel*. The dorsal surface of the body of the tooth exhibits an undulating outline, with a central depression, each half of the body being symmetrical: the extremities of the body slightly bulge inwards at the enteric face. The keel, which springs backwards from the confluent halves of the body in the centre of the tooth, becomes somewhat bulbous or dilated at its free edge (Plate VI. fig. 4, *a*).

Mode of examination.—The great difficulty in successfully investigating the structure of the tooth of *Echinus* has arisen from the physical peculiarities of its different parts—the loose incoherence of its developing end in comparison with the great hardness of the other extremity, and the different density of the several parts of the tooth when fully formed, that is, at the same portion of the tooth in its maturity. The relation of the forming parts to the formed tooth can only be made out by carefully examining the growing end of the organ where its parts are soft, thin, and sufficiently transparent to be viewed microscopically in mutual relationship; by scrutinizing the fractures of the tooth and the fractured surfaces in its intermediate condition of softness and hardness, where it is too dense to be examined microscopically as a transparent object, and too lax to admit of being ground down into sections, and then by comparing these results with the appearances presented by sections, vertical and transverse, of the mature tooth as seen with the microscope.

The making of the sections of the matured parts of the tooth is another difficulty: the body of the organ is so dense and the keel so soft, that it requires the utmost care and no little dexterity, so to apply pressure and grinding action, as to render the body of the tooth sufficiently transparent without grinding entirely away the soft keel. The hard sections, when ground down sufficiently, should be mounted in Canada balsam.

The examination of the soft end of the growing tooth requires considerable adroitness. After removing the whole of the dental apparatus from a very fresh animal, the tooth, where it emerges internally from the alveoli, may be detached with the point of a knife, and itself freed from surrounding soft parts by means of fine-pointed scissors. When the growing end of the tooth is thus detached, it should be carefully drawn along the surface of a slightly moistened glass slide, just sufficient to extend it, the observer noting from the first whether it is the dorsal or enteric surface of the tooth which is uppermost and exposed to the objective. The specimen should then be treated with Liquor Sodæ, which has the effect of completely clarifying the soft tissues accidentally attached, so that nothing is observed but the plates, fibres, &c.—the different tooth-elements, sharply defined and *prononcé*. At the same time it will be found that dissection of the tooth, where its structure has become complicated, is very difficult; and it is only (in such regions of the organ) by many repeated examinations, and by the close and constant scrutiny of every detached portion of the disintegrated teeth, that happy and illustrative specimens can be obtained.

One important element of the tooth-structure, the "Soldering Particles," to be described hereafter, can be best studied in those portions of the shaft about midway between its two extremities; at a point, that is, immediately before they unite the several elements into a coherent mass. Little portions may be detached with needle-points, which display these particles adherent to the plates and fibres which they unite together; and often parts of several plates and fibres may be seen thus united.

Sections of the mature tooth are best examined by transmitted light—this ultimate histology with high powers; the general structural arrangement with low. The soft growing end of the tooth should also be examined by transmitted light. In those central portions of the tooth, where very instructive fractures may be produced by slipping apart the as-yet loosely attached imbricated elements, the broken ends should be viewed with reflected light and a very low power, as a hand-lens.

I have found that the nuclei and concentric rings in the soldering particles, and the incremental lines in the more advanced plates, can be best seen by a modification of transmitted light—by employing bright sunlight, but so adjusting the reflector as to direct the light just out of the limit of the field: in this partial darkness the appearances in question are most conspicuous.

Before entering on a minute description of the histology and growth of the Echinus-tooth, I would premise a few general remarks which will render my subsequent explanations more intelligible.

The Echinus-tooth consists of an aggregation of *plates* and *fibres* of carbonate of lime, arranged in most curious complexity, but on a constant and definite plan.

The tooth commences at the internal soft growing end by the formation of two series of thin triangular imbricated plates. These are physiologically, though not mathematically, the axis of the tooth, extending from end to end of the organ, and ultimately forming an important part of the body of the matured structure: upon these plates,

dependent on them and attached to them, are all the supplemental elements which enter into the construction of the tooth. These are four in number, namely, certain *Secondary Plates*, *Cylindrical Fibres*, *Flabelliform Processes*, and *Enamel Rods*.

The *Secondary plates* are lappets of the same thin calcareous sheet as constitutes the primary plates from whose outer edge they fold inwards towards the enteric region.

The form of the primary and secondary plates relatively, and their section, may be seen in the Woodcut diagram II., while the position of these tooth-elements in a transverse section of the tooth itself is diagrammatically represented in Woodcut I. *a*, *b*.

The *Cylindrical fibres* are cylindrical only in their early form: they are certain rods of carbonate of lime, which, stretching inwards and downwards from the upper angle and outer edge of the primary plates, constitute the keel of the tooth (Woodcut I. *c*, *c'*).

The *Flabelliform processes* are reticulated growths with fan-shaped extremities, of the same nature as the cylindrical fibres: they pass outwards and laterally *between* the plates from whose upper edge they take their origin. In their course they contribute considerably to the bulk and solidity of the body of the tooth, and, reaching beyond the edges of the plates, they terminate in the coarse reticulated structure which skirts the enteric border of the body of the tooth (Plate VI. fig. 4, *b*, *b*; and Woodcut I. *d*).

The *Enamel rods* are short stout cylinders of carbonate of lime, which grow backwards from the lower edge of the dorsal surface of the primary plates. They form, by fusing together, the white enamel which clothes, in a thin layer, the posterior aspect of the tooth. The structure, when completely formed, has small interstitial tubes, and is almost strictly horizontal, that is, at right angles to the long axis of the tooth.

All these supplemental parts are attached to the primary plates at their point of origin, but are elsewhere free: thus the secondary plates are attached to the outer edge of the primary plates (Plate VII. fig. 2; also Woodcut II. *a*). The cylindrical fibres are attached to the upper angle and outer edge of the plates (Plate VII. fig. 3); the flabelliform processes to the same region (Plate VII. fig. 1), and the enamel rods to the posterior surface of the lower margin of the plates (Plate VII. fig. 4). When all these parts are formed, they are free except at the points just indicated, and each set of elements is placed over its successor in a loose imbrication.

The next change consists in the development, all over the plates and fibres, of countless multitudes of minute excrescences of carbonate of lime, which, by their vertical growth, solder together the contiguous elements of the tooth, and by their lateral increase of size so diminish the intervals between them as to produce a compact though canaliculated structure.

The development of the tooth divides itself not unnaturally into three stages,—that of the primary plates, the supplemental elements, and, finally, the general consolidation.

Let it be recollected then that the plates (primary and secondary) constitute the main portion of the body of the tooth, the bulk being increased by the reticulation of the flabelliform processes between the plates; that the cylindrical fibres constitute the keel,

the enlarged extremities of the flabelliform processes the skirtings, and the enamel rods the white coating on the outer surface of the dorsum of the body of the tooth. These data being recollected, the particular direction and arrangement of the tubular interspaces, indeed the whole course and character of the histology of the mature structure which I am about to describe, become easily intelligible.

Histology.—The ultimate structure of the mature tooth of the Echinus is remarkable as presenting appearances similar to the hard structures of Vertebrata and certain Mollusca—*bone*, *dentine*, and *shell*, and exhibiting also a curious inconsistency in the aspects which it displays in different lines of section, vertical or transverse. The vertical section from before backwards resembles very closely the bone of fishes, while the vertical section of the tooth from side to side presents appearances closely simulating the dentine of a mammalian tooth. A transverse section exhibits a large area of the structure exceedingly like an oblique section of some molluscous shells, such as *Pinna*, with circumscribed regions of minute parallel tubes very like mammal ivory. Further, some portions of the tooth, the "skirtings," closely resemble the *shell* of echinoderms generally. A vertical section from within outwards of the compact consolidated portion of an Echinus-tooth, when viewed by moderate magnifying power, exhibits a general laminated structure, the laminations being parallel, and running in two directions from the axis of the tooth obliquely downwards and outwards, and downwards and inwards (Plate VI. figs. 2 & 3): these laminæ are commonly straight in their course. Professor WILLIAMSON speaks of the latter, those of the keel, as having a double sigmoid course. This, however, appears to vary with the species: I have not met with it in the teeth of the small variety of *E. sphæra*, among which I have mostly prosecuted my researches, while I have found it very conspicuous in the large teeth of *E. Flemingii*. Interspersed among the laminæ, but still more between them, are minute opake spaces (Plate VIII. fig. 7), which with higher powers are seen to resemble very closely the lacunæ of true vertebrate bone, especially that of fishes, in the angularity of the lacunæ and the sharp bends of the canaliculi (Plate VIII. fig. 8). The central portion or axis of the tooth is seen to be very transparent, presenting an indistinct and latent lamination, but destitute of opake vacuities. The difficulty of reconciling the appearances of the transverse and vertical sections (Plate VII. fig. 5, *a*, *b*, and Plate VIII. figs. 7 & 8) depends partly on the form and partly on the arrangement of the interspaces in the tissue, especially as regards the bone-like lacunæ. Now the lacunæ have a certain length, about $\frac{1}{1000}$th of an inch, and a breadth of about half that measurement; but they are so compressed laterally, that when seen cut across as in a transverse section they present but a minute vacuity, scarcely more than the canaliculi themselves. In arrangement the lacunæ are so disposed in the tooth that their long axis is not far from vertical—in the keel obliquely from above downwards and inwards; in the body of the tooth from above downwards and outwards. Their extreme breadth is at right angles to their length, and from before backwards in the tooth. Thus it is in a transverse section the lacunæ are seen at their minimum, whereas in a vertical section (from before backwards, not from side to side)

they are displayed at their maximum. In the body of the tooth, where this vertical section does not absolutely correspond in parallelism with the disposition of the elementary parts, as it does in the keel, the lacunæ are not shown in their longest axis nor in their extreme breadth—they are displayed somewhat obliquely, and are proportionally reduced in size.

The canaliculi run in greatest numbers and in general direction from the sides of the lacunæ more than from their extremities—at right angles, that is, to the general distribution of the lacunæ, at right angles to their long axis, and to the general fibrous or lamelliform structure of the particular part in which they are observed. This is especially obvious in vertical sections of the keel (Plate VIII. figs. 7 & 8), and it heightens the semblance of the tissue to true vertebrate bone—the fibres and plates looking like bone-laminæ of the Haversian systems, or of the external compact tissue, the lacunæ being dispersed mostly between the laminæ, while the canaliculi run outwards and inwards to the nutritional surfaces; and doubtless the anatomical arrangement of the tubular canals and reservoir cavities is, in both instances (vertebrate bone and Echinus-tooth), equally in accordance with their functional office

The dentine-like structure of the Echinus-tooth, as seen in transverse section, is confined to the lateral regions of the body of the tooth—the portion formed by the plates, and the flabelliform processes between the plates, the tubes being the parallel intervals which are left by the imperfect adhesion of these contiguous elements. That this is absolutely the case is easily shown by examining a rather thick section, when, by altering the focus of the microscope, the successive tubes, one above another, are recognized and are traced nearer to, or further from, the edge of the tooth, in accordance with the oblique direction in which the plates are disposed. The tube-like intervals remind the observer closely of the dentinal tubes of a human tooth, and the cavities have about the same diameter. There are moreover many fine branchings like what are seen especially towards the peripheral extremity of the true dentinal tubes of a mammalian tooth.

Very similar appearances are to be observed in the same region of the *body* of the tooth, that is within the outer edge and not quite in the centre, *in a vertical section from side to side*—not from before backwards. In the absolute centre there is a lateral parallel linear arrangement, but there are no tubular intervals.

Forming the outer limit of the dorsal surface of the body of the tooth is a structure to which I have applied the term *enamel*, and this rather from its whiteness and its anatomical position than from its histological character. This is the only matured structure of the Echinus-tooth which presents precisely the same aspect, whether seen in vertical or transverse section. The enamel is a very thin layer, only reaching the $\frac{1}{300}$th or $\frac{1}{400}$th of an inch in thickness even in large teeth, and that too in its thickest part—the lateral extremities and the dorsal convexities of the body of the tooth (see Plate VI. fig. 4, *c*, *c*).

The arrangement of this structure is a series of cylindrical tubes running from within outwards in an otherwise apparently homogeneous mass of carbonate of lime. Their

direction is nearly horizontal, that is at right angles to the axis of the tooth, but not strictly so, as in their course from within towards the surface they point slightly downwards (see Plate VI. fig. 3, *c*).

The tubes are cylindrical and unbranched: they extend from the surface to their inner limit in a nearly straight course, and then divide all equidistant from the exterior: their divisions are almost uniformly dichotomous, the double tubes inosculating with the other channels of the less superficial portions of the tooth. The average diameter of these tubes is about $\frac{1}{4000}$th of an inch.

A good example of a transverse section of the enamel, magnified 400 diameters, is seen at Plate VIII. fig. 6, in which some of the tubes are sharply defined, a few containing air-bubbles, while others, more deeply seated, are looming indistinctly out of focus. The tubes appear to be open at their outer extremities.

The portion of the tooth of Echinus which resembles in ultimate structure a section of molluscous shell, is the keel, as seen in transverse section (Plate VII. fig. 5). It is not precisely similar to any section *in plane* of the structure to which I have compared it, but it is like an obliquely transverse section proceeding from one part to another in an increasing curved obliquity.

A transverse section of the columnar particles of such a shell as that of *Pinna* presents an aggregation of polyhedral outlines, all of about the same aspect, whereas in the keel of the Echinus-tooth, as seen in similar section, the outlines of the cut prisms, which, close to the body of the tooth, are nearly equilateral pentagons or hexagons, become progressively elongated towards the enteric margin of the keel, so as to approach in form narrow oblongs with modified extremities. This depends, not on any alteration of the obliquity of the cut across the calcareous rods, but upon the change of their form towards their free ends, as will be shown hereafter (see Woodcut III.). The prismatic sections look as if composed of cylinders so partially compressed, that the angles, where three or more lines meet, leave interspaces far larger than between the sides where the compression has apparently occurred. The similitude which these outlines have to the adaptation of compressible particles is of course only apparent, as the structure is composed entirely of hard calcareous rods; but the change from true but small cylinders, which they are at first, to large rods, would involve, in their mutual adaptation, as they approached or came in contact, the same modification of form.

It is the large interspaces, above spoken of, as seen in transverse section at the point where the outlines of some three or more prisms meet, which coincide with the rows of lacunæ as seen in vertical section, and which determine their linear arrangement. Compare fig. 5, Plate VII. with figs. 7 & 8, Plate VIII.

There is one other portion of the tooth peculiar and histologically characteristic; that part of the body which faces the alimentary canal and the sides of the keel near the body of the tooth. The minute anatomy of this region, as seen in transverse section, is the nearest approach to the ordinary shell and sclerous echinoderm structure which any of the elements of the tooth exhibit. It consists of a loose open aggregation of tubes

without any definite arrangement, like segments of the ambulacral discs, portions of the spines, alveoli, &c.

The *continuous development* of the tooth, by the incessant reproduction at its growing end of the elementary parts which constitute it, affords the anatomist an opportunity of examining the nature and form of those elementary parts, and of observing the synthetical process by which they combine to build up this curious and complicated structure.

I purpose now to describe these elements in detail—in the order of their appearance and their value in combination.

Primary Plates.—The tooth originates at its soft growing extremity by the development of two sets of triangular plates (Plate VI. fig. 5). Valentin saw, correctly figured and described, the early growth of the first elementary parts of the Echinus-tooth, though he failed altogether to understand the relation of these parts to the entire organ. These plates commence as mere spots, so minute as to be of no definable form. They constitute two parallel series of equal numbers, at first quite distinct and separated by a clear interval, but afterwards, by the general enlargement of their area by marginal growth, they approximate each other in the mesial line, and at about the 50th to the 70th plate they begin to intersect in a perfectly regular alternation up the centre—the overlapping increasing more and more as the size of the plates increases up to a certain point, beyond which the further enlargement appears to take place almost wholly towards the outer margins.

The form of the plates is a modified triangle, but the sides, instead of being straight, are more or less convex or waved. The superior internal (enteric) and the inferior internal angles are mucronated, the former more than the latter. The mucronation consists in a thickened point which projects beyond the plate, and passing back upon its surface is lost in a gradual thinning expansion.

The form of the plates is constantly undergoing modification from their earliest appearance till the latest time at which they are capable of isolation and separate inspection. At first the aberration from a nearly-equilateral triangle consists in a prolongation of the outer and inferior angle: as the plate enlarges this becomes less conspicuous, and then the inner *side* of the triangle advances towards its fellow of the opposite series, in an irregular wavy line, at the same time the sharp mucronated angles become rounded and lost, and the thickened lines on the face of the plate also disappear. In the further development of the plates their outline is entirely altered, the triangle is lost, and, principally by prolongation of the outer inferior angle, the plate is converted into a broad wavy band. Compare fig. 5, Plate VI., figs. 1 & 4, Plate VII. It is very difficult to obtain the plates when they have assumed this latter form, as this change only occurs in a condition of very advanced development, when the plates have usually become more or less united with the contiguous elements of the tooth.

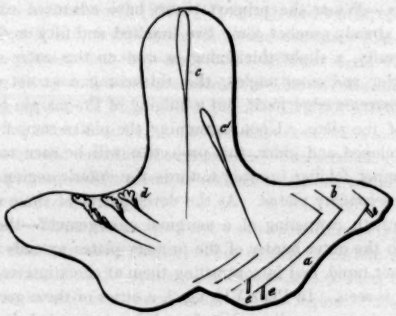
While the plates are increasing in area they become denser, thicker, and there now appear upon them, when viewed in a suitable light, certain lines having the same contour as the outlines of the plates themselves: these are indications of progressive growth,

are, in fact, *incremental lines*, and they are chiefly conspicuous at the outer angle, where the most rapid change has of late been going on (see Plate VII. fig. 4). As has been already observed, these lines are best seen by employing bright sunlight and directing the reflector of the microscope obliquely to one side of the field of vision.

The primary plates require very careful observation; they are, as I have before remarked, the *physiological* axis of the tooth, and dependent on them and attached to them are all the supplemental elements which enter into the construction of the organ.

The two series of primary plates have a definite and constant arrangement. As they are free in the little aqueous sac that contains them, merely imbricated one over the other without any attachment, it is difficult so to place them under the microscope as to avoid derangement of their relations. Careful observation, however, shows that, as regards the *mathematical* axis of the tooth, they are doubly oblique. Vertically they are oblique downwards and outwards, *i. e.* from the superior enteric angle to the inferior dorsal angle; and the second obliquity—the horizontal—arises from the internal inferior angle being further removed from the enteric region than the external. This will be best understood by referring to Plate VI. figs. 2 & 3, *b*, which represent the line of vertical obliquity, and the Woodcut I. *a*, which indicates the horizontal obliquity.

Woodcut I.*



In examining the soft growing end of the tooth, care should be taken to ascertain which face (enteric or dorsal) is opposed to the objective, and the thin glass which is imposed upon the specimen should be placed upon it lightly and without pressure. Much pressure may flatten and spread out the plates so as to be seen only face-wise, while traction downwards of the upper angle may present them to view edgewise, as in the case of VALENTIN's figure. A long series of observations will enable the micro-

* Diagram indicating, in a transverse section of a tooth, the position of *a*, Primary Plates; *b*, Secondary Plates; *d*, Flabelliform Processes; *cc'*, Fibres of the Keel; *eee*, Enamel Rods.

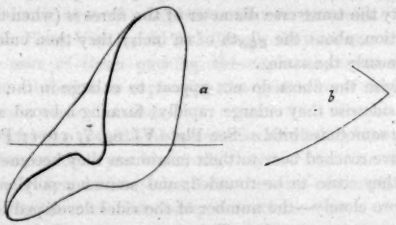
scopist to form an approximate idea of their position: a fracture of the tooth where it has become coherent and at the same time not absolutely consolidated, where one series of the tooth-elements may be detached from those imbricated upon it without their displacement, will determine their position definitively. The upper plates in figure 5, Plate VI. are very nearly shown in that position of obliquity which is normal, and which they maintain when in combination with the other elements of the tooth. The degree of imbrication of the plates, the amount covered and the amount left uncovered by each plate as it lies imposed upon its neighbour, will be best understood by examining their dorsal aspect when *in situ*. This is seen in Plate VII. fig. 1, where they are little disturbed; and it should be borne in mind, as the uncovered portion in the dorsal aspect is the exact limit of the attachment of one of the secondary elements (the enamel fibres) to the primary plates, which will hereafter be shown.

I have never succeeded in getting a view of a fractured plate edgewise so as to ascertain its thickness: it is, however, very thin, and its area increases rapidly in proportion to the accumulating density in its progressive growth. While the outline of the plates, when entire, is marked by *curves*, their fracture is always *angular* and *crystalline*. Portions of the plates, and indeed *all* the elements of the tooth, break up with a rhomboidal fracture, and, when the plates are disintegrated under the microscope, they present the appearance of a multitude of the rhombic plate-crystals of cholesterine.

Secondary Plates.—When the primary plates have advanced considerably in their development, and already number some two hundred and fifty or three hundred from the growing extremity, a slight thickening is seen on the outer edge about midway between the superior and outer angles: this thickening does not consist of a further growth upon the extreme edge itself, but a bulging of the margin towards the internal or enteric aspect of the plate. Upon examining the plates some five or ten further in advance, more developed and older, this projection will be seen to have grown into a small valve-like lappet folding inwards towards the enteric region. This is the commencement of the secondary plates. As the development of these supplemental plates progresses—the growth consisting of a marginal enlargement—their form alters and their attachment to the outer border of the primary plates extends itself. The growth of these plates is not rapid, and in contrasting them at short intervals of their advancement little change is seen. In Plate VII. fig. 2, a series of these growths is represented at intervals of fives, each succeeding plate from below upwards being the fifth from the previous one. By examining this illustration, the slow progress of development (in some instances not distinguishable between the contiguous fifth plate) will be seen and the altered form they gradually assume. The most remarkable feature of this altered form, when it has arrived at maturity, is the mamilliform process which grows from the middle of the free edge, and is ultimately much longer than any represented in this plate. This form is so characteristic that the existence and position of the secondary plates may always be recognized in the most advanced and complicated condition in which the tooth is susceptible of examination by transmitted light.

The secondary plates are developed from the outer margin of each series of primary plates on either side, and in their growth project towards the enteric region and towards the mesial line, approaching their fellows of the opposite side. When seen *in situ* and in their complete development, the mamilliform processes present a beautiful even series, between which the rods of the keel pass inwards. This is displayed in Plate VII. fig. 3.

Woodcut II.*



The angle at which the secondary bends away from the primary plate can be best estimated by contemplating a section through their centre—the average presented in a transverse section of a mature tooth (Plate VI. fig. 4), and which is diagrammatically represented in Woodcut I. *a*, *b*, and Woodcut II. The secondary plates are adherent to the outer edge of the primary, but they easily break off from them, coming away entire—the fracture being sharp and complete along the line of attachment. It is very common, in breaking up a tooth with needle points under the microscope, to see the secondary plates floating about free, but themselves unbroken: they may always be recognized by their characteristic mamilliform processes. The secondary plates are of the same nature as the primary—thin sheets of carbonate of lime, and they break up with exactly the same crystalline fracture†.

Keel Rods or Fibres.—These elements of the tooth constitute a very large proportion of its bulk—larger than any other singly. They consist of certain fibres which originate from the superior angle of the primary plates, attached to their extreme edge, and projecting inwards loose and free towards the enteric region. The source of these fibres, commencing in the centre where the two sets of plates interlock, gradually extends laterally,—the central fibres being the oldest and longest, those proceeding outwards in their attachment, shorter and shorter. The central fibres pass straight down-

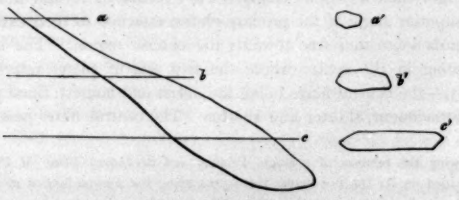
* Diagram showing the relation of a single Primary and Secondary Plate. *a*, Primary Plate with Secondary Plate folded on it; the line across them presenting the section figured at *b*. The latter has been turned round so as to coincide with the sectional lines *a* and *b* in Woodcut I.

† It is remarkable that neither Valentin nor Williamson appears to have been aware of the existence of these secondary plates, though they are sufficiently conspicuous, and constitute very important items in building up the fabric of the tooth.

wards and inwards to the enteric region: the lateral fibres converge on either side to join them (see Plate VII. fig. 3). The fibres originate in minute acicular threads, passing inwards from the extreme edge of the plates. They increase rapidly in length and slowly in circumference. They remain cylindrical and even in diameter till about the $\frac{1}{100}$th of an inch in length: they then commence enlarging gradually from the proximal to the distal extremity, and this alteration in shape progresses till they are fully formed, when they assume an attenuated club-shape. Up to within about a fourth of their distal extremity the transverse diameter of the fibres is (when thus fully developed) equal in each direction, about the $\frac{1}{800}$th of an inch; they then enlarge vertically, their breadth remaining nearly the same.

When seen edgewise the fibres do not appear to enlarge in the last quarter of their length: when seen sidewise they enlarge rapidly, forming a broad sword-shaped termination. The end is sometimes bifid. See Plate VI. fig. 7, *a* to *i*; Plate VII. figs. 3 & 5. When the fibres have reached near to their maximum they become altered in form (as before mentioned), they cease to be rounded, and assume a polyhedral shape, so as to adapt themselves more closely—the number of the sides developed being variously 4, 5, or 6, the two latter most commonly. This is seen in transverse section of the keel, which, though cutting the fibres themselves with obliquity on account of their slanting position in the keel, shows their superficial form with sufficient distinctness. In figure 5, *a*, *b*, Plate VII., this may be observed, the altered form of the polyhedral sections of the fibres in passing outwards towards the extremity of the keel depending on this progressive vertical enlargement. Thus it is that the appearances remind the observer of an oblique section of the shell of Pinna; but, as has been remarked, to produce the sectional elongation, it would be necessary, in the case of the mollusk shell, that the obliquity of the section should be progressively increased, which here is not the case—the altered form of the pentagons and hexagons in the keel of the Echinus-tooth depending on the increased vertical growth of the fibres. This will be better understood by referring to the diagrammatic illustration of Woodcut III.

Woodcut III.*



By a fortunate fracture in a tooth, hardly sufficiently consolidated to admit of being

* Diagram of a keel-fibre, indicating at *a*, *b*, *c* three planes of section, and at *a'*, *b'*, *c'* the resultant cut surfaces.

ground into section, exhibited in the illustration, Plate VII. fig. 5, *b*, the individuality of the fibres is displayed in their lateral narrowness, and their crystalline fracture is also seen. This latter circumstance is more distinctly shown in a vertical section of a much larger tooth (also imperfectly consolidated), magnified twice as many diameters (400), at Plate VIII. fig. 8, *b*; and the rhombic character of the fracture is here clearly traceable.

The fibres, like the plates, ultimately become united together by the soldering particles, but their adhesion is never so complete as that of the plates. It will be seen in the sections figured in Plate VII. fig. 5, that though they assume a more or less polyhedral form for the sake of closer packing, the angles are never sharp and pointed. This has already been alluded to as explaining the position and the linear arrangement of the lacunæ, as seen in vertical section.

The keel fibres are free at their distal extremities until they become consolidated by the soldering particles; at their proximal ends they are from the first united *inter se*: numbers of them may be detached together from the edge of the plate upon which they hang without disturbing their relation among themselves.

In large teeth the keel fibres sometimes attain the $\frac{1}{10}$th or $\frac{1}{12}$th of an inch in length: one, the $\frac{1}{18}$th of an inch, is represented at *h*, fig. 7, Plate VI.

The description I have given applies to keel fibres in the teeth of the small variety of *E. sphæra*, upon which my researches were made: in *E. Flemingii* I have found that the fibres slowly and evenly enlarge in proceeding from the proximal to the distal extremity, instead of swelling out club-shaped, towards their free ends.

Flabelliform Processes.—Also attached to the upper edge of the primary plates, and usually commencing to be formed before the appearance either of the secondary plates or the keel rods, there is developed towards the superior enteric angles a sprouting of moss-like reticulations (Plate VI. fig. 6, and Plate VII. fig. 1). These reticulations gradually but not rapidly enlarge, spreading on either side in a direction downwards and outwards. They do not stand away at an angle from the plates, like the keel rods, but pass sidewise between them, thus separating each plate from the next in previous contact with it. It is this elaborate reticulation intervening between the plates that so often masks the laminated structure of the body of the tooth in the region of the primary plates when seen in transverse section; and this is especially the case in the centre of the tooth, where these reticulations are most complicated, and where the attenuated angle of the plate in its most advanced state of development favours the obscuration of the layer-like appearance.

At first this moss-like growth consists principally of narrow fibres, but soon the extremities spread out into broad flattened expansions, club-shaped, fan-shaped, and of infinite diversity of form (Plate VI. fig. 7, *k*, *l*, *n*, *p*). It is these expansions at the extremity of the reticulations, extending beyond the margin of the plates, that constitute the coarse tubular tissue at the sides of the keel near the body of the tooth, and at the enteric margin of the body itself.

Enamel Rods.—The most difficult point of investigation in the structure of the Echinus-tooth is the method of development of the enamel, and its relation to the other elementary parts of the organ. This arises from two causes—the smallness of the amount of the tissue itself, and the lateness of its appearance on the tooth, after the organ has become very dense and opake.

Upon viewing the dorsal aspect of a set of primary plates, it will be seen that the face of each plate is covered by its predecessor, excepting for a small extent of its basal margin (Plate VII. fig. 1), and it is from this small extent of exposed plate that the rods originate which constitute the enamel. When, by a fortunate fracture, some of the plates on which this growth has commenced are isolated and present their dorsal aspect to the observer, they are seen studded, for just that amount of their margin which is naturally exposed, with minute projections of carbonate of lime closely resembling the early formation of the soldering particles, but, unlike them, they are limited to a particular and defined boundary. They differ, moreover, as will be seen by examining the more advanced of them, in becoming elongated, whereas the soldering particles as they grow increase in size mainly by circumferential enlargement.

In figure 4, Plate VII. are represented fifteen primary plates, exhibiting their dorsal aspect with the enamel rods just commencing to be formed; and as some of them are partially dislocated and pushed aside, it will be seen how circumscribed is the development of these enamel-rod growths, and how it is limited to just so much of the back surface of the plates as would be uncovered when in undisturbed position—those portions which, by the imbrication of the previous plate, or by the overlapping at the central line of junction of the plates of the opposite side, would be covered up, being free from these developments. The fibres, which constitute the enamel, grow backwards nearly horizontally, and by their lateral union among themselves form a compact structure, leaving simple and narrow tubular intervals.

Soldering Particles.—Up to a certain limit in the progress of development, the primary plates and the supplemental elements that enter into the structure of the tooth are free, excepting that the latter are attached to the former by their points of origin—the keel fibres, the flabelliform processes, and the enamel rods adhering to the primary plates by their proximal extremities. All the elementary parts themselves, excepting these attachments, though packed in close contiguity and entangled together, are free and floating in the circumambient fluid of the developmental sac.

A fresh formation now occurs of a very remarkable nature.

Scattered over the surface of all the elementary parts of the tooth there appear countless multitudes of minute points of carbonate of lime; at first these are the smallest microscopic specks, such as are depicted on the keel rod at Plate VI. fig. 7, *h*. These particles adhere firmly to the surface upon which they grow, and increase both in circumference and thickness, the former, however, more than the latter. The increase of growth of these calcareous points continues till the contiguous elements of the tooth are by them soldered together. It is from this circumstance, in accordance with the

function of these developments, that I have named them "Soldering particles." These bodies were first described by Professor WILLIAMSON as the instruments by which the separate elements of the forming tooth are united into a coherent mass, the vacuities between them constituting the cavities and tubes of the matured tissue. Though the interstitial nature of the tubes, lacunæ, and canaliculi was pointed out by QUEKETT, and in this much Professor WILLIAMSON was anticipated by him, still the method by which this character of the tissue is produced had never been before described.

Professor WILLIAMSON was, however, quite wrong in some important points, both in the anatomy and anatomical relationship of the soldering particles. He asserts that they are *free*, that they are unconnected with each other, and that they never become anchylosed to one another: in all three particulars he is mistaken.

The soldering particles are always attached firmly, and in fact united to the elements of the tooth on which they are seen, and cannot be removed without violence. Indeed it is by this absolute adhesion that the structures of the tooth are soldered together and cease to be separable. The mere development of these granules among the other elements of the tooth, if still free, would only add to the complication of the structures without contributing to their cohesion. The nature of these particles can be best investigated by examining the plates when studded with them and in a state of considerable advancement of growth, as is represented in Plate VIII. fig. 1. Portions of the plates may be broken up about midway between the two extremities of the tooth with the points of needles, the soldering particles remaining firmly attached to their surface: the particles can occasionally be swept off the plates by force; but it usually happens that, when once formed, they hold together as one, and that, however the plate breaks, its fractured edge corresponds with the distribution of the soldering particles.

The specimen figured at Plate VIII. fig. 1 consists of portions of four plates held together by the soldering particles: the relation of the particles to one another is here seen, and the characteristic appearance which they present when viewed in face. By a happy fracture of the most superficial plate, it exposes to view the reticulations of the fiabelliform processes, passing out between that plate and the next; and on them is also seen a profusion of soldering particles whose function it is to unite them with the contiguous plates.

But the soldering particles are not only adherent to the surface on which they are formed; they have a special connexion *inter se*. It occasionally happens that a whole sheet of the particles will become detached from the surface of a plate more or less completely, and yet retain their relative position as respects each other (see Plate VIII. figs. 3, 4, & 5, *b*, *d*). That this mutual attachment of the contiguous soldering particles is over and above their adhesion to the plate on which they rest, may be most conclusively demonstrated in such a specimen as is represented at fig. 3 of Plate VIII., in which a number of the soldering particles remain upon and adherent to the surface of a portion of broken plate, while others of the same set extend beyond its margin—the latter retaining their relative position *inter se* just as undisturbedly as the former.

It is sometimes very difficult to discover why the detached particles thus maintain their mutual relationship, while at others one is able distinctly to see a film of extreme tenuity holding them together. It is almost impossible to see this film as long as these particles are adherent to and resting upon the plates; but when detached, it may generally be made out, and it is especially intelligible when it projects beyond the margin of one of the particles (Plate VIII. figs. 4 & 5, *d*).

I had long much difficulty in determining the precise character of this film, but I am now entirely satisfied that it is of the same chemical nature as the particles themselves, and that it is double, passing from each face of each particle to each face of the contiguous ones; thus converting the intervals between them into canals, and constituting, when thus viewed in face, an upper and a lower wall to those canals. Now, though this film is of the same nature as the plates themselves, and ultimately becomes indissolubly united with them, still, as long as the particles can be detached *en masse* and in unbroken relationship, with the film entire, it cannot be said that the intervals between them are mere interstitial vacuities destitute of walls, as Professor WILLIAMSON asserts; the truth being that the *tubular system* is, by means of the soldering particles and their connecting films, introduced among—*interpolated between*—the other previously-existing elements of the tooth, and that it has an existence, as a tubular system, before the indissoluble adhesion of the elements has occurred.

How early the soldering particles are held together by the calcareous film that ultimately connects them, I am unable to say; but as far as observation goes the particles appear quite separate at first. Neither can I speak with certainty of the universality of this connecting medium, though from its usual appearance, when properly sought for, I should infer that it is always, or at least generally, formed in the advanced state of the tooth's development.

The soldering particles themselves vary in diameter indefinitely from the minutest microscopic point to a disc of the $\frac{1}{500}$th of an inch. Their average may perhaps be stated at the $\frac{1}{1500}$th or $\frac{1}{1200}$th of an inch, and their thickness from one-fourth to one-sixth of their diameter.

Their form is usually circular or oval at first, but as they increase in size and approach each other they become polyhedral and angular. Each soldering particle has a more or less conspicuous nucleus, around which are seen series of concentric rings—incremental lines of progressive enlargement. Contrary to Professor WILLIAMSON's statement, the neighbouring particles frequently fuse together, and their original distinct centres are easily recognized (see Plate VI. fig. 5, *b*, *c*); indeed it is by this complete and general fusion in the centre of the tooth that the compact hard axis is produced in which no interspaces remain.

Thus is built up this curiously complex and, at the same time, definitely planned fabric. It has many points in common with all the skeleton structures of the Echinodermata—the repetition of similar primary elements, and the sprouting from their surface of secondary elementary parts, and the further progressive enlargement of all by

mere surface growth. Again, like them, it exhibits the combination, both in its whole and in the anatomy of its parts, of organised form and the simplest chemical composition—shapes elaborated under vital forces composed of a material which physical and chemical actions scarcely pronounce to be other than inorganic.

The intimate anatomy of the Echinus-tooth is in no way to be removed in essential character from the rest of the Echinus-skeleton. Though the plan upon which its minute structure is formed is more symmetrical and orderly, and is more elaborate, yet I still concur entirely in Dr. CARPENTER'S generalization, "that the structure of the teeth is essentially the same as that of the shell, save in the interspaces of the network being narrower." Certain portions of the transverse section of the tooth (the *skirtings*) present coarse reticulations very closely resembling the shell-structure of the Echinus; but it is by examining the humbler teeth in another Echinoderm that the absolute anatomical truth of this generalization is established. In the little denticles—"*pala angularis*" of J. MÜLLER—the teeth of Ophiocoma, truly homologous with those of Echinus, the base of each exhibits an anatomical structure identical with that of the Echinus-shell, but gradually passing to its distal end into a tissue as entirely resembling the enamel of the Echinus-tooth, or the tubes between the keel-fibres when cut parallel to their axis—the broad reticulated interspaces in the former merging into the narrow tubules of the latter.

DESCRIPTION OF PLATES.

PLATE VI.

Fig. 1. Echinus-tooth, natural size and form: *a*, enteric or ventral aspect; *b*, external or dorsal aspect.

Fig. 2. Vertical section of portion of tooth, magnified 10 diameters. The form of the apex of the tooth is shown, as produced by wear and retained by the relative hardness of its elementary parts: *a*, the clear, condensed axis of the tooth; *b*, the body formed of plates; *c*, the "enamel;" and *d*, the keel.

Fig. 3. A diagram showing the *lines of direction* displayed by a vertical section of—*b*, the primary plates; *c*, the enamel rods; and *d*, the keel-fibres.

Fig. 4. Transverse section of Echinus-tooth, magnified 50 diameters. *a*, extremity of keel; *b*, *b*, skirtings; *c*, *c*, enamel.

Fig. 5. The commencing growth of Echinus-tooth, originating in two systems of plates. This view is on the enteric or ventral surface. The figure is magnified 100 diameters.

Fig. 6. The same view as the preceding, but more advanced in development: the angles of the plates are modified, and the sprouting of the "flabelliform processes" has commenced. Magnified 100 diameters.

Fig. 7. These details represent the elementary fibres of the keel, and the flabelliform processes, whose dilated ends form the skirtings of the tooth: from *a* to *h* are shown different degrees of development of the keel-rods, or fibres: on the surface of *h* the soldering particles are seen in their earliest condition: *i* represents the broad flattened end of a large fibre; *k*, *l*, different forms of keel-fibres and flabelliform processes; *m*, keel-fibres detached from their plate, but showing mutual linear adhesion at their proximal extremities; *n*, flabelliform processes exhibiting the same; *o*, keel-fibres still attached to plate; *p*, flabelliform processes displaying the same condition. These details are variously magnified 150 and 75 diameters, as indicated in the Plate.

PLATE VII.

Fig. 1. Plates viewed on the external or dorsal surface. Magnified 100 diameters. The part of the tooth here represented corresponds with the lower portion of fig. 6, Plate VI., but is from a much larger tooth.

Fig. 2. Plates of a *large* tooth, seen on the enteric or ventral surface, and showing the progressive growth of the "secondary plates." The plates are artificially arranged at intervals of *fives*, so as to exhibit more conspicuously the change that occurs. The figure indicates the progressive development in seventy plates. Magnified 100 diameters.

Fig. 3. Ventral or enteric surface of Echinus-tooth in an advanced state of development. The secondary plates are recognized on either side by their mamilliform processes: the central straight and lateral converging acicular fibres of the keel are also displayed. The *lines of intersection* of the inferior edges of the primary plates at their dorsal aspect are seen looming indistinctly out of focus. Magnified 50 diameters.

Fig. 4. Primary plates viewed on dorsal surface in an advanced state of development, displaying their modified form, the *incremental lines* at their outer angles, and the commencement of the enamel rods on their lower margins, uncovered by the imbrication of the contiguous plates. Magnified 100 diameters.

Fig. 5. Transverse section of keel, showing its shell-like appearance when thus displayed. Magnified 200 diameters. *a* exhibits the different forms of cut surface as the vertical depth of the fibres increases, while the lateral breadth remains the same; *b*, the same, where a fracture has isolated the ends of several fibres.

PLATE VIII. exhibits the ultimate histology of Echinus-tooth.

Fig. 1. Portions of four plates studded with "soldering particles;" also reticulations of the flabelliform processes protruding between the plates, and likewise dotted with soldering particles. Magnified 200 diameters.

Fig. 2. A few soldering particles adherent to a portion of plate. Magnified 200 diameters.

Fig. 3. Soldering particles adherent to a portion of plate, but extending beyond its limits, and retained in relative position by their connecting film. Magnified 200 diameters.

Fig. 4. A sheet of soldering particles, entirely detached from their plate, but held together by their connecting film. Magnified 200 diameters.

Fig. 5. Other soldering particles: *a*, one seen edgewise; *b*, three connected particles—one produced by fusion of three smaller ones; *c*, large particle, the result of the fusion of four; *d*, small particles with very distinct single centres; *e*, large, nearly homogeneous particles. Magnified 200 diameters.

Fig. 6. Enamel on the extreme dorsal surface, as seen in transverse section, exhibiting its tubes, some containing bubbles of air. Magnified 400 diameters.

Fig. 7. Vertical section of keel, showing the general linear arrangement of the lacunæ, coincident with the oblique direction of the fibres of the keel. Magnified 100 diameters.

Fig. 8. Vertical section of keel, where just sufficiently condensed to admit of being ground into section, displaying the same general linear arrangement of lacunæ as in fig. 7,—the canaliculi mainly passing at right angles to that serial arrangement. At *a* the soldering particles are indicated, with the interstitial lacunæ; at *b* the ends of the fibres in their extreme vertical breadth and with their crystalline fracture. Magnified 400 diameters.

The whole of the figures in these Plates have been taken from specimens prepared by the author; and with one exception (fig. 8, Plate VIII.) they have been drawn, to ensure exactness, with the assistance of the camera lucida.

XXI. *On* Fermat's *Theorem of the Polygonal Numbers.*

By Sir Frederick Pollock, *F.R.S., Lord Chief Baron, &c. &c.*

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Fermat's theorem of the polygonal numbers has engaged the attention of some of the most eminent mathematicians. It was first announced (about the year 1670) in his edition of Diophantus, published after his death (it occurs in a note on the 31st question, p. 180). It is to be found stated at length in Legendre's 'Théorie des Nombres' (in p. 187 of the 2nd edition, &c.). For above a century after it appeared, no proof was discovered of any part of it; but in 1770 Lagrange (in the Transactions of the Royal Academy of Sciences at Berlin) gave a proof of the second branch of the theorem (the case of the square numbers), from the paper containing which it may be collected that Euler had endeavoured in vain to establish a proof, but had suggested the clue by which Lagrange succeeded in discovering one.

In the second volume of Euler's 'Opuscula Analytica,' there is an article on this subject, of some length, lamenting the loss of Fermat's investigations, and pointing out that Lagrange's proof as to the square numbers affords (from its nature) no assistance to the discovery of a proof of the other cases; he adds, "sine dubio plerique Geometræ in his demonstrationibus investigandis frustra desudaverint."

About twenty-five years after the death of Euler (who died in 1783), Legendre, in his 'Théorie des Nombres,' published a proof of the first branch of the theorem (the case of the triangular numbers), which proof is in part inductive, and not founded on pure demonstration; and subsequently M. Cauchy discovered a proof of all the cases (assuming the first and second cases to be proved); this was published about the year 1816, in a Supplement to Legendre's 'Théorie des Nombres.'

Fermat, after stating the proposition, alludes to the proof of it as arising out of "*many various and abstruse mysteries of numbers*;" and he states his intention to "*write an entire book on the subject, and very much to advance the bounds of arithmetic.*" No such work has appeared; and it is understood that among his papers no trace has been found of any materials for such a publication. It becomes a matter of more than mere curiosity to consider what could have been the properties of numbers alluded to; obviously they must have been connected, more or less, with the division of numbers into squares or other polygonal numbers.

The general object I have in view is to investigate the properties of numbers on which Fermat's theorem depend. In this paper I wish to call attention to some pro-

perties connected with the division of numbers into 4 squares, which probably (in some form) were part of the system to which FERMAT alluded.

I have already stated two properties of the odd numbers (not, I believe, noticed before), upon one of which the whole of FERMAT'S theorem depends (as will hereafter appear). The first is to be found in the Transactions of the Royal Society for the year 1854, p. 313; it is there called Theorem C.

"Every odd number may be divided into square numbers (not exceeding 4), the algebraic sum of whose roots (positive or negative) will (in some form of the roots) be equal to every odd number from 1 to the greatest possible sum of the roots.

"Or in a purely algebraic form. If

$$a^2+b^2+c^2+d^2=2n+1,$$

and

$$a+b+c+d=2r+1,$$

a, b, c, d being integral or nil, n and r being positive, and r a maximum, then if r' be any positive integer (not greater than r), it will always be possible to satisfy the pair of equations

$$w^2+x^2+y^2+z^2=2n+1$$

$$w+x+y+z=2r'+1,$$

by integral values (positive, negative, or nil) of w, x, y, z."

The other is to be found in the Royal Society's Transactions for 1859, p. 49, and relates not to the sum of the roots, but to the difference between two of them. The first of these connects together the first and second branch of FERMAT'S theorem.

For if every odd number can be divided into 4 square numbers, so that the sum of the roots of two of them being deducted from the sum of the roots of the other two, there shall be a remainder of 1,—

Then *every number is divisible into* 3 *triangular numbers*; for the 2 sums of the roots must be of the form $2a+1$, and $2a$, and the four roots will be of the form

$$a+p+1,\ a-p,\ a+q,\ a-q;$$

and if $2n+1$ equals the sum of these roots squared,

$$2n+1=4a^2+2p^2+2q^2+2a+2p+1, \text{ and } n=2a^2+a+p^2+p+q^2;$$

but $2a^2+a$ is a triangular number, and p^2+p+q^2 is the general form for the sum of any 2 triangular numbers*; therefore n any number is equal to 3 triangular numbers (nil being considered as a triangular number, as some of the terms may become equal to nothing).

There are some theorems worthy of remark arising out of a comparison of the differences of the roots of the four square numbers into which every odd number may be divided.

It will appear from the Table that accompanies this paper, that when a number of the form $4n+1$ is divisible into 2 square numbers (of which one must be even and the other odd, $4n+1$ being an odd number), the roots of these 2 squares furnish the exte-

* The proof of this is given presently.

rior differences of the roots of the four squares into which $2n+1$ may be divided. Before explaining the Table, it is proper to state that if an odd number be divisible into 4 square numbers, three of them must be odd, and one of them even, or one of them must be odd, and 3 of them even, otherwise their sum cannot be an odd number; it follows from this that if the difference between any two of them be an odd number, the difference between the other two must be an even number, and *vice versâ*; for let $a^2+b^2+c^2+d^2=2n+1$, then if $a^2-b^2=2p$, c^2-d^2 must equal $2q+1$; if possible let $c^2-d^2=2r$, then $a^2-b^2+c^2-d^2=2p+2r$; add $2b^2+2d^2$ (an even number) to each, and $a^2+b^2+c^2+d^2$ will be an even number, which by the hypothesis it is not; if, therefore, a^2-b^2 be an even number, c^2-d^2 cannot also be an even number, and therefore must be an odd one. If, therefore, the four roots of the squares into which any odd number may be divided are arranged in any order there will be three differences; the two exterior differences will be one odd, the other even; the middle difference may be either odd or even.

The Table is arranged thus:—the lowest row of figures is the series 1, 5, 9, 13, 17, &c. $(4n-1)$; the next row above is the series of natural numbers, 0, 1, 2, 3, 4, &c. (n), &c.; the next row is 1, 3, 5, 7, 9, &c. $(2n+1)$ the odd numbers; each of the odd numbers is the first term in a series increasing upwards by the numbers, 2, 4, 6, 8, 10, &c., forming an arithmetic series of the second order (the first and second differences being respectively 2 each); when the number in the lowest row cannot be divided into 2 squares, the arithmetic series is not formed, and the square spaces are marked with an asterisk, but when the number $4n+1$ is divisible into two square numbers, the roots of these squares constitute the two exterior differences of the roots into which the odd numbers may be divided, and also of the roots into which each term of the series increasing upward may be divided; the middle difference of the roots will be the smaller half of the sum of the 2 roots of the square numbers into which $4n+1$ may be divided, with a negative sign, and will increase by 1 in each successive term of the upward series.

For example, in the Table take the number 29 in the lowest row, $7\times4+1=29$, 7 is the number above it, and $7\times2+1=15$ the odd number, which is the first term of the series 15, 17, 21, 27, 35, &c. Now 29 is composed of 2 square numbers, 4 and 25, whose roots are 2 and 5, $2+5=7$; the smaller half is 3, and 2, -3, 5 will be the differences of the roots of the squares into which 15 may be divided, and whose sum will equal 1; thus

$$\overset{2,\ -3,\ 5}{-1,\ 1,\ -2,\ 3};$$

the roots when squared and added together equal 15, and the other terms of the series follow in like manner, obeying the law indicated; thus

$\overset{5,\ -2,\ 2}{-3,\ 2,\ 0,\ 2}$ when squared and added . . $=17$

$\overset{2,\ -1,\ 5}{-2,\ 0,\ -1,\ 4}$ when squared and added . $=21$

$$\overset{5,\ 0,\ 2}{-4,\ 1,\ 1,\ 3} \text{ when squared and added } \quad . \quad = 27$$

$$\overset{2,\ \ 1,\ 5}{-3,\ -1,\ 0,\ 5} \text{ whose squares } \quad . \quad . \quad . \quad . \quad = 35$$

The proof of all this depends on a property of numbers mentioned in the Philosophical Transactions for 1854, vol. cxliv. p 317.

If any number be composed of two triangular numbers, it will also equal a square and a double triangular number. If

$$n = \frac{a^2+a}{2} + \frac{b^2+b}{2},$$

it will be of the form a^2+a+b^2, and may be assumed equal to a^2+a+b^2. For if 2 numbers be both odd or both even, they may always be represented by $a+b$ and $a-b$; if one be odd and the other even, they may always be represented by $a+b\pm1$ and $a-b$, or by $a+b$ and $a-b\pm1$; and if the two numbers be made the bases of trigonal numbers, the sum of the two trigonal numbers will always be of the form a^2+a+b^2, or a^2+b+b^2: now when any number in the natural series of numbers is composed of two triangular numbers, it may be represented by a^2+a+b^2, and $4n+1$ will then equal $4a^2+4a+1+4b^2$,—obviously the sum of an odd and an even square, whose roots are $2b$ and $2a+1$; and $2n+1$, the corresponding odd number, will equal $2a^2+2a+1+2b^2$,—obviously composed of 4 square numbers, whose roots are b, b, a, $a+1$; and if they be arranged thus,

$$\begin{matrix} 2b, & -(a+b) & 2a+1 \\ -b, & b, & -a, \qquad a+1, \end{matrix}$$

so that the sum of their roots may equal 1, the exterior differences of the roots will be $2b$ and $2a+1$, the roots of the two squares into which $4n+1$ is divisible; and the middle difference will be $-(a+b)$, the smaller half of the sum of the roots $(2b+2a+1)$ with a negative sign; if the exterior differences be reversed and the middle difference be increased by 1, the differences will be $2a+1$, $-(a+b-1)$, $2b$, and the roots whose sum will equal 1 will be, with their differences above them,

$$\begin{matrix} & 2a+1, & -(a+b-1), & 2b \\ -(a+1), & a- & (b-1), & b+1, \end{matrix}$$

and the sum of the squares of the roots will be 2 more; from these two sets of roots all the rest may be obtained, by adding one to each of two roots and subtracting 1 from each of the other two roots; the exterior differences of the roots will therefore always be the same, and the middle difference will increase by 1 at each step; the sum of the squares of the roots will increase by

$$2,\ 4,\ 6,\ 8,\ \&c.$$

As the sum of any two square numbers of which one is odd and the other even $(4a^2+4a+1+4b^2)$ must be of the form $4n+1$, every possible case of an odd square

combined with an even square must occur somewhere in the series

1, 5, 9, 13, &c.,

and the Table (if extended) must contain every possible case of odd and even numbers as exterior differences, combined with every possible and available middle difference; for negative differences may be rejected, inasmuch as, if the roots be put according to their algebraic value, all the differences must be positive; thus the roots and differences of 15 above were

$$\overset{2,}{-1},\ \overset{-3,}{1},\ \overset{5}{-2},\ 3;$$

if the roots be placed according to their algebraic value, they would be -2, -1, 1, 3, and with the differences above

$$\overset{1,}{-2},\ \overset{2,}{-1},\ \overset{3}{1},\ 3;$$

15 will therefore be found in the column above 5, and in the fourth place. The Table (extended indefinitely) would therefore contain every possible odd number the sum of whose roots may equal 1.

In connexion with the Table just mentioned, it may be well to state a theorem respecting the differences of the roots, by which, having obtained one division of an odd number into 4, or 3 squares (equal to, or greater than 1, and not more than 2 of them equal to each other), other modes of dividing the odd number into 4 squares may generally be obtained.

Theorem.

If any number be composed of 3 squares, and the roots be arranged in the order of their algebraic value, if the two differences between the adjoining roots differ by 3, or a multiple of 3, then by reversing the differences and obtaining roots whose algebraic sum shall equal the sum of the former roots, but whose differences shall be reversed, another form of division into squares will be obtained; that is, the sum of the squares of the roots thus obtained will be equal to the sum of the squares of the first roots.

Example.

[*Note.*—I use the symbol ^{2}——— to indicate that the numbers below it are to be considered as *roots* which are to be squared and added together; thus, $100=6^2+8^2$; therefore $101=\overset{2}{0,\ 1,\ 6,\ 8.}$]

The differences of $\overset{5\ \ 2}{1,\ 6,\ 8}$ are 5, 2, which differ by 3. If, now, roots be obtained with differences 2, 5, and whose sum will equal $1+6+8=15$, the sum of the squares of these roots will equal 101. $\overset{2'\ \ 5}{2,\ 4,\ 9}$ are roots having the differences reversed, and their sum $=15$; therefore $2^2+4^2+9^2=1^2+6^2+8^2=101$. Again, leaving out 6 as a root, $65=\overset{2}{\underset{1\ \ 7}{0,\ 1,\ 8}}$; the differences are 1, 7; the sum of the roots $=9$: -2, 5, 6 are

roots having the same sum but the differences reversed, and the sum of their squares $4+25+36=65$; therefore $65=\overset{2}{2, 5, 6}$. Again, $-\overset{7}{5}, \overset{4}{2}, 6$ have the differences 7, 4; their sum $=3$; but $-\overset{4}{4}, \overset{7}{0}, 7$ have the differences reversed and the same sum; therefore

$$-\overset{2}{5}, \overset{7}{2}, \overset{4}{6}=-\overset{2}{4}, \overset{4}{0}, \overset{7}{7}=65, \text{ and } 101=\overset{2}{0}, 4, 6, 7.$$

The proof of this theorem will appear from putting the general case algebraically, which also will show the method of obtaining the new roots required. Let the differences of the roots be represented by a, $a+3n$ (which include every case); then (p), $^{\text{diff. }a}(p+a)$, $^{a+3n}(p+2a+3n)$ will represent any 3 roots having the required differences; the sum of these roots is $3p+3a+3n$ [*a multiple of* 3]: reverse the differences and take p as the first root, and they will be $-p$, $^{a+3n}(p+a+3n)$, $^{a}p+2a+3n$; the sum will be $3p+3a+6n$ [also a multiple of 3]; therefore the difference will be a multiple of 3, and the sums may be made equal (one to the other) by adding or subtracting from each root the difference divided by 3: here the difference is $3n$, and the new roots will be $p-n$, $^{a+3n}p+a+2n$, $^{a}p+2a+2n$; and if each of these sets of roots be squared and added together, the sum of each will be $3p^2+5a^2+9n^2+6ap+6np+12an$.

A similar theorem belongs to 4 roots whose differences differ by 4: thus 1, 2, 7, 16, as roots, have the differences 1, 5, 9; their sum is 26: -3, 6, 11, 12 have the differences reversed, 9, 5, 1; and their sum also equals 26; and

$$\overset{2}{1, 5, 9}\quad \overset{2}{9, 1, 5}\quad \overset{2}{1, 5, 13}\quad \overset{2}{13, 5, 1}$$

$$1, 2, 7, 16=-3, 6, 11, 12=310: \text{ so } -6, -5, 0, 13=-12, 1, 6, 7=230,$$

the sum of the roots in each case being equal, and the differences reversed.

A similar theorem also belongs to 5 roots whose differences differ by 5, and no doubt to n roots whose differences differ by n.

There are many arithmetic series of the 2nd order which, beginning with 1 as a first term, will have all their terms divisible into not exceeding four squares; there are 3 such series to which I wish to call attention. If 1 be increased by 2, 4, 6, 8, &c., the $(n+1)$th term of the series is always n^2+n+1, or $4n^2\pm 2n+1$, that is, $\overset{2}{n, n, n, n\pm 1}$. If 1 be increased by 2, 6, 10, 14, &c., the $(n+1)$th term will always be $\overset{2}{0, 1, n, n}$. If 1 be increased by 4, 8, 12, 16, &c., the $(n+1)$th term will be $\overset{2}{0, 0, n, n+1}$. The first of these series contains the numbers which, being divided into 4 squares, give the sum of the roots a maximum; the others give the differences between 2 roots a maximum, the one the even differences, the other the odd differences.

But if any odd number (instead of 1) be made the first term of the series, some remarkable consequences ensue. If any odd number $4n\mp 1$ be increased by 2, 4, 6, 8, &c., the term whose index of place is the lesser moiety of the odd number will be composed of 4 squares, whose roots will be the result of again dividing the moieties of the odd number; thus $4n\mp 1=2n\mp 1+2n=n\mp 1+n+n+n$; if the number be $4n-1$, the $(2n-1)$th term will be $\overset{2}{(2n-1), n, n, n}$; if it be $4n+1$, the $2n$th term will be

$\overset{9}{n, n, n, n+1}$; but every term will be composed of one or more square numbers + an arithmetic number, and the squares and the arithmetic numbers will each form a regular series. An example in figures will best explain this: $19=9+10=4+5+5+5$. If 19 be increased by 2, 4, 6, 8, &c., the 9th term is $91=\overset{9}{4, 5, 5, 5}$; so if 19 be increased by 2, 4, 6, 8, &c., the successive terms will be composed of squares and arithmetic numbers as below: to distinguish the arithmetic numbers from roots, I enclose them in a ◯.

	Numbers.	Roots.		Numbers.	Roots.
19=	(18)	0, 1	or =	(17)	1, 1
21=	(19)	1, 1	=	(16)	1, 2
25=	(20)	1, 2	=	(17)	2, 2
31=	(23)	2, 2	=	(18)	2, 3
39=	(26)	2, 3	=	(21)	3, 3
49=	(31)	3, 3	=	(24)	3, 4
61=	(36)	3, 4	=	(29)	4, 4
75=	(43)	4, 4	=	(34)	4, 5
91=	(50)	4, 5	=	(41)	5, 5
	(or 5, 5)				
109=	(59)	5, 5	=	(48)	5, 6
	&c.	&c.		&c.	&c.

It may also be composed of one square and one arithmetic number in two different ways, thus:—

	Arith. numbers.	Roots.		Numbers.	Roots.
19=	(19)	0	or =	(18)	1
21=	(20)	1	=	(17)	2
25=	(21)	2	=	(16)	3
31=	(22)	3	=	(15)	4
39=	(23)	4	=	(14)	5
49=	(24)	5	=	(13)	6
61=	(25)	6	=	(12)	7
75=	(26)	7	=	(11)	8
91=	(27)	8	=	(10)	9
109=	(28)	9	=	(9)	10
129=	(29)	10	=	(8)	11
151=	(30)	11	=	(7)	12
175=	(31)	12	=	(6)	13
201=	(32)	13	=	(5)	14
229=	(33)	14	=	(4)	15
259=	(34)	15	=	(3)	16
291=	(35)	16	=	(2)	17
325=	(36)	17	=	(1)	18
361=	(37)	18	=	(0)	19
399=	(38)	19	=	(-1)	20

Again, if a number of the form $4n+1$ be increased by 2, 6, 10, 14, &c., the series formed will have its $(2n+1)$th term $=\overset{2}{0, 0, 2n, 2n+1}$; its $(2n)$th term will be $=\overset{2}{2n-1, 2n,} + (2)$; the $(2n-1)$th term will be equal to $\overset{2}{(2n-2), 2n-1} + (4)$; the $(2n-p)$th term

$\overset{2}{(2n-(p+1)}, (2n-p)+2(p+1)$; so that, whenever $2(p+1)$ is composed of not exceeding 2 squares, the term is composed of not exceeding 4 squares; but the $(2n+1)$th term will also equal $\overset{3}{(2n-1)}, 2n+(8n)$*; the $2n$th term will equal $\overset{4}{(2n-2)}, (2n-1)$ $+\boxed{8n-2}$, and so on,—the series of arithmetic numbers decreasing by 2, instead of increasing. An example in actual figures will better illustrate this.

Series.	Roots.	Numbers.		Roots.	Numbers.
19=	0, 1	(18)	also=	1, 0	(18)
21=	1, 2	(16)	=	0, 1	(20)
27=	2, 3	(14)	=	1, 2	(22)
37=	3, 4	(12)	=	2, 3	(24)
51=	4, 5	(10)	=	3, 4	(26)
69=	5, 6	(8)	=	4, 5	(28)
91=	6, 7	(6)	=	5, 6	(30)
117=	7, 8	(4)	=	6, 7	(32)
147=	8, 9	(2)	=	7, 8	(34)
181=	9, 10	(0)	=	8, 9	(36)

The first of these cannot be continued usefully, because the number becomes negative after the 10th term, the other series continues.

219=	10, 11 &c.	−2	=	9, 10 &c.	(38) &c.

If the odd number be increased by 4, 8, 12, 16, &c., the series obtained will have similar properties; its $2n$th term will be $\overset{2}{0}, 1, n, n$, the roots n, n will diminish by 1 in each preceding term, and 1 will be an arithmetic number increasing by 2, as appears below in the case of the odd number 19.

* If the form of the odd number be $4n+3$, the arithmetic number is $8n+4$.

Series.	Roots.	Numbers.		Roots.	Numbers.
19=	1, 1	(17)	also =	0, 0	(19)
23=	2, 2	(15)	=	1, 1	(21)
31=	3, 3	(13)	=	2, 2	(23)
43=	4, 4	(11)	=	3, 3	(25)
59=	5, 5	(9)	=	4, 4	(27)
79=	6, 6	(7)	=	5, 5	(29)
103=	7, 7	(5)	=	6, 6	(31)
131=	8, 8	(3)	=	7, 7	(33)
163=	9, 9	(1)	=	8, 8	(35)

The numbers are alternately of the form $4n+1$ and $4n-1$; the terms of the series are therefore equal to 2 squares + a number of the form $4n+1$, and to 2 other squares + a number of the form $4n-1$. A number of the form $4n-1$ cannot be composed of less than 3 squares; for if a^2 and b^2 be odd squares, their sum is of the form $(8n+2)$; if even squares, of the form $(4n)$; if one be odd and the other even, $(4n+1)$; and $4n-1$ cannot equal $8n'+2$, or $4n''$, or $4n'''+1$; but as the 2 squares are always equal, the arithmetic number may always be turned into a number of the form $4n+1$, by substituting for the 2 equal squares 2 others, whose roots shall be, the one one more, the other one less; thus $79 = \overline{6, 6} + (7) = \overline{5, 7} + 5 = \overline{5, 7, 1, 2}$; also $= \overline{5, 5} + (29) = \overline{5, 5, 5, 2}$.

And every term of the series is divisible into 4 squares whenever $4n+1$ is divisible into 2 squares, or when $4n'-1-2$, another form of $4n+1$, is so divisible. It would follow, that if there be any 2 series in arithmetical progression with a common difference of 1, and the odd terms of the one be placed over the even terms of the other, then if either series be considered as composed of roots and the other of numbers, and the squares of the roots be added to the numbers, a series will be formed of the first sort; thus

9, 8, 7, 6, &c.

6, 7, 8, 9, &c.

If the lower be considered as roots, the series becomes

$45_{,13}\ 57_{,14}\ 71_{,15}\ 87_{,16}$ &c.;

if the upper be considered as roots, the series is

$87_{,16}\ 71_{,14}\ 57_{,13}\ 45$, &c.,

the same series, but decreasing instead of increasing; and it is worthy of remark that the first term of the series is the sum of the root and the arithmetic number, viz. 15. If both the series decrease, as

9, 8, 7, 6, &c.,
6, 5, 4, 3, &c.,

and the lower be considered as roots, the series is

$45_{,12}\ 33_{,10}\ 23_{,8}\ 15,$

whose first term is 3, the difference between the arithmetic number and the root; if the upper be considered as roots, the first term is 3, but negative, and the series would be

$-3_{,2}\ -1_{,4}\ 3_{,6}\ 9_{,6}\ 17_{,10}\ 27_{,12}$ &c. $87_{,18}\ 69_{,16}\ 53_{,14}\ 39$, &c.

If the series be composed of 2 equal roots, increasing or decreasing each by 1, or of 2 roots differing by 1, and increasing or decreasing in like manner, then if the series of numbers differ by 2, so that all the terms shall be odd, a series will be formed of the 2nd or 3rd kind, whose second difference will be 4; thus if the numbers be 9, 11, 13, 15, &c., and the roots 3, 3, 4, 4, 5, 5, 6, 6, the series will be $27_{,16}\ 43_{,26}\ 63_{,34}\ 87$, a series of the 3rd kind having a second difference of 4, and the first term will be the difference between the number and the sum of the roots, viz. $9-(3+3)$; for $3_{,4}\ 7_{,8}\ 15_{,12}\ 27$ produces the series; but if the numbers decrease by 2,

9, 7 5, 3, &c.
(3, 3), (4, 4), (5, 5), (6, 6),

the series will be

$27_{,12}\ 39_{,16}\ 55_{,20}\ 75,$

and the first term of that series is the sum of the roots and the odd number, viz. $9+3+3=15$, for 15, 19, 27, 39, &c. is the series.

So if the roots, instead of being equal, differ by 1, thus,

6, 8, 10, 12, &c.
3, 4, 4, 5, 5, 6, 6, 7, &c.,

the series will be

$31_{,18}\ 49_{,22}\ 71_{,26}\ 97$, &c.,

a series of the 2nd kind, whose first term is the difference between 6 and $3+4$, viz. 1, and negative, and the series is

$-1_{,2}\ 1_{,6}\ 7_{,10}\ 17_{,14}\ 31_{,18}\ 49_{,22}\ 71$, &c.;

but if the numbers decrease, as

12, 10, 8, 6, &c.
3, 4, 4, 5, 5, 6, 6, 7, &c.,

the series is

$37_{,14}\ 51_{,18}\ 69_{,22}\ 91$, &c.,

and the first term is

$19=12+3+4,$

$19_{,2}\ 21_{,6}\ 27_{,10}\ 37_{,14}\ 51$, &c.

Some remarkable properties arise from connecting these series together, which I must reserve for a future communication.

91 1, 9, 0 −5, −4, 5, 5	93 1, 8, 2 −5, −4, 4, 6	95 3, 8, 0 −6, −3, 5, 5	97 3, 7, 2 −6, −3, 4, 6	99 1, 7, 4 −5, −4, 3, 7	✠	103 3, 6, 4 −6, −3, 3, 7	105 5, 6, 2 −7, −2, 4, 6	✠	109 1, 6, 6 −5, −4, 2, 8	111 5, 5, 4 −7, −2, 3, 7
73 0, 8, 1 −4, −4, 4, 5	75 2, 7, 1 −5, −3, 4, 5	77 0, 7, 3 −4, −4, 3, 6	79 2, 6, 3 −5, −3, 3, 6	81 4, 6, 1 −6, −2, 4, 5	✠	85 4, 5, 3 −6, −2, 3, 6	87 2, 5, 5 −5, −3, 2, 7	✠	91 6, 5, 1 −7, −1, 4, 5	93 4, 4, 5 −6, −2, 2, 7
57 1, 7, 0 −4, −3, 4, 4	59 1, 6, 2 −4, −3, 3, 5	61 3, 6, 0 −5, −2, 4, 4	63 3, 5, 2 −5, −2, 3, 5	65 1, 5, 4 −4, −3, 2, 6	✠	69 3, 4, 4 −5, −2, 2, 6	71 5, 4, 2 −6, −1, 3, 5	✠	75 1, 4, 6 −4, −3, 1, 7	77 5, 3, 4 −6, −1, 2, 6
43 0, 6, 1 −3, −3, 3, 4	45 2, 5, 1 −4, −2, 3, 4	47 0, 5, 3 −3, −3, 2, 5	49 2, 4, 3 −4, −2, 2, 5	51 4, 4, 1 −5, −1, 3, 4	✠	55 4, 3, 3 −5, −1, 2, 5	57 2, 3, 5 −4, −2, 1, 6	✠	61 6, 3, 1 −6, 0, 3, 4	63 4, 2, 5 −5, −1, 1, 6
31 1, 5, 0 −3, −2, 3, 3	33 1, 4, 2 −3, −2, 2, 4	35 3, 4, 0 −4, −1, 3, 3	37 3, 3, 2 −4, −1, 2, 4	39 1, 3, 4 −3, −2, 1, 5	✠	43 3, 2, 4 −4, −1, 1, 5	45 5, 2, 2 −5, 0, 2, 4	✠	49 1, 2, 6 −3, −2, 0, 6	51 5, 1, 4 −5, 0, 1, 5
21 0, 4, 1 −2, −2, 2, 3	23 2, 3, 1 −3, −1, 2, 3	25 0, 3, 3 −2, −2, 1, 4	27 2, 2, 3 −3, −1, 1, 4	29 4, 2, 1 −4, 0, 2, 3	✠	33 4, 1, 3 −4, 0, 1, 4	35 2, 1, 5 −3, −1, 0, 5	✠	39 6, 1, 1 −5, 1, 2, 3	41 4, 0, 5 −4, 0, 0, 5
13 1, 3, 0 −2, −1, 2, 2	15 1, 2, 2 −2, −1, 1, 3	17 3, 2, 0 −3, 0, 2, 2	19 3, 1, 2 −3, 0, 1, 3	21 1, 1, 4 −2, −1, 0, 4	✠	25 3, 0, 4 −3, 0, 0, 4	27 5, 0, 2 −4, 1, 1, 3	✠	31 1, 0, 6 −2, −1, −1, 5	33 5, −1, 4 −4, 1, 0, 4
7 0, 2, 1 −1, −1, 1, 2	9 2, 1, 1 −2, 0, 1, 2	11 0, 1, 3 −1, −1, 0, 3	13 2, 0, 3 −2, 0, 0, 3	15 4, 0, 1 −3, 1, 1, 2	✠	19 4, −1, 3 −3, 1, 0, 3	21 2, −1, 5 −2, 0, −1, 4	✠	25 6, −1, 1 −4, 2, 1, 2	27 4, −2, 5 −3, 1, −1, 4
3 1, 1, 0 −1, 0, 1, 1	5 1, 0, 2 −1, 0, 0, 2	7 3, 0, 0 −2, 1, 1, 1	9 3, −1, 2 −2, 1, 0, 2	11 1, −1, 4 −1, 0, −1, 3	✠	15 3, −2, 4 −2, 1, −1, 3	17 5, −2, 2 −3, 2, 0, 2	✠	21 1, −2, 6 −1, 0, −2, 4	23 5, −3, 4 −3, 2, −1, 3
1 0, 0, 1 0, 0, 0, 1	3 2, −1, 1 −1, 1, 0, 1	5 0, −1, 3 0, 0, −1, 2	7 2, −2, 3 −1, 1, −1, 2	9 4, −2, 1 −2, 2, 0, 1	11	13 4, −3, 3 −2, 2, −1, 2	15 2, −3, 5 −1, 1, −2, 3	17	19 6, −3, 1 −3, 3, 0, 1	21 4, −4, 5 −2, 2, −2, 3
0	1	2	3	4	5	6	7	8	9	10
1 0, 1	5 2, 1	9 0, 3	13 2, 3	17 4, 1	21 ✠	25 0, 5 4, 3	29 2, 5	33 ✠	37 6, 1	41 4, 5

113 3, 5, 6 −6, −3, 2, 8	115 7, 6, 0 −8, −1, 5, 5	117 7, 5, 2 −8, −1, 4, 6	✠	121 5, 4, 6 −7, −2, 2, 8	123 7, 4, 4 −8, −1, 3, 7	✠	127 3, 4, 8 −6, −3, 1, 9	✠	131 9, 5, 0 −9, 0, 5, 5
95 6, 4, 3 −7, −1, 3, 6	97 0, 5, 7 −4, −4, 1, 8	99 2, 4, 7 −5, −3, 1, 8	✠	103 6, 3, 5 −7, −1, 2, 7	105 4, 3, 7 −6, −2, 1, 8	✠	109 8, 3, 3 −8, 0, 3, 6	✠	113 0, 4, 9 −4, −4, 0, 9
79 3, 3, 6 −5, −2, 1, 7	81 7, 4, 0 −7, 0, 4, 4	83 7, 3, 2 −7, 0, 3, 5	✠	87 5, 3, 6 −6, −1, 1, 7	89 7, 2, 4 −7, 0, 2, 6	✠	93 3, 2, 8 −5, −2, 0, 8	✠	97 9, 3, 0 −8, 1, 4, 4
65 6, 2, 3 −6, 0, 2, 5	67 0, 3, 7 −3, −3, 0, 7	69 2, 2, 7 −4, −2, 0, 7	✠	73 6, 1, 5 −6, 0, 1, 6	75 4, 1, 7 −5, −1, 0, 7	✠	79 8, 1, 3 −7, 1, 2, 5	✠	83 0, 2, 9 −3, −3, −1, 8
53 3, 1, 6 −4, −1, 0, 6	55 7, 2, 0 −6, 1, 3, 3	57 7, 1, 2 −6, 1, 2, 4	✠	61 5, 0, 6 −5, 0, 0, 6	63 7, 0, 4 −6, 1, 1, 5	✠	67 3, 0, 8 −4, −1, −1, 7	✠	71 9, 1, 0 −7, 2, 3, 3
43 6, 0, 3 −5, 1, 1, 4	45 0, 1, 7 −2, −2, −1, 6	47 2, 0, 7 −3, −1, −1, 6	✠	51 6, −1, 5 −5, 1, 0, 5	53 4, −1, 7 −4, 0, −1, 6	✠	57 8, −1, 3 −6, 2, 1, 4	✠	61 0, 0, 9 −2, −2, −2, 7
35 3, −1, 6 −3, 0, −1, 5	37 7, 0, 0 −5, 2, 2, 2	39 7, −1, 2 −5, 2, 1, 3	✠	43 5, −2, 6 −4, 1, −1, 5	45 7, −2, 4 −5, 2, 0, 4	✠	49 3, −2, 8 −3, 0, −2, 6	✠	53 9, −1, 0 −6, 3, 2, 2
29 6, −2, 3 −4, 2, 0, 3	31 0, −1, 7 −1, −1, −2, 5	33 2, −2, 7 −2, 0, −2, 5	✠	37 6, −3, 5 −4, 2, −1, 4	39 4, −3, 7 −3, 1, −2, 5	✠	43 8, −3, 3 −5, 3, 0, 3	✠	47 0, −2, 9 −1, −1, −3, 6
25 3, −3, 6 −2, 1, −2, 4	27 7, −2, 0 −4, 3, 1, 1	29 7, −3, 2 −4, 3, 0, 2	✠	33 5, −4, 6 −3, 2, −2, 4	35 7, −4, 4 −4, 3, −1, 3	✠	39 3, −4, 8 −2, 1, −3, 5	✠	43 9, −3, 0 −5, 4, 1, 1
23 6, −4, 3 −3, 3, −1, 2	25 0, −3, 7 0, 0, −3, 4	27 2, −4, 7 −1, 1, −3, 4	29	31 6, −5, 5 −3, 3, −2, 3	33 4, −5, 7 −2, 2, −3, 4	35	37 8, −5, 3 −4, 4, −1, 2	39	41 0, −4, 9 0, 0, −4, 5
11	12	13	14	15	16	17	18	19	20
45 6, 3	49 0, 7	53 2, 7	57 ✠	61 6, 5	65 4, 7	69 ✠	73 8, 3	77 ✠	81 0, 9

XXII. *On the Great Magnetic Disturbance which extended from August 28 to September 7, 1859, as recorded by Photography at the Kew Observatory.* By Balfour Stewart, *A.M.*

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During the latter part of August, and the beginning of September, 1859, auroral displays of almost unprecedented magnificence were observed very widely throughout our globe, accompanied (as is invariably the case) with excessive disturbances of the magnetic needle.

The interest attached to these appearances is, if possible, enhanced by the fact, that at the time of their occurrence a very large spot might have been observed on the disc of our luminary—a celestial phenomenon which we have grounds for supposing to be intimately connected with auroral exhibitions and magnetic storms.

The auroral displays above mentioned were very attentively observed throughout Europe, America, and Australia. In many places these were of the most gorgeous character, while other places were visited by this meteor where its appearance was an event of very rare occurrence. Even from as low a latitude as Cuba we have a description of it by the Director of the Havannah Observatory, accompanied with the remark that only four previous displays had been recorded in the traditions of the island. In not a few instances telegraphic communication was interrupted, owing to the current produced in the wires; and in some cases this proved so powerful that it was used instead of the ordinary current, the batteries being cut off and the wires simply connected with the earth.

It is unnecessary to enter into further particulars regarding this meteor, as the descriptions of it given by observers at places widely apart have been collected together by Professor E. Loomis, and published in a series of papers communicated to the American Journal of Science and Arts. I shall only add that, both from the European, the American, and the Australian accounts, there appear to have been two great displays, each commencing at nearly the same absolute time, throughout the globe,—the first on the evening of the 28th of August, and the second on the early morning of the 2nd of September, Greenwich time.

Magnetic disturbances of unusual violence and very wide extent were observed simultaneously with these displays. These were recorded more or less frequently at the various observatories; but at Kew there is the advantage of a set of self-recording magnetographs (the property of the Royal Society), which are in constant operation.

As a description of these instruments has already been published in the volume of Reports of the British Association for 1859, it is only necessary here to mention that

they afford the means of obtaining a continuous photographic register of the state of the three elements of the earth's magnetic force—namely, the declination, and the horizontal and vertical intensity. Reduced representations of the traces furnished by these instruments during the great disturbance under discussion accompany this paper; and it will now be necessary to give a short description of these.

In the originals we have for each element, for each day, a straight line and a curved one. The straight or zero-line serves as a line of abscissæ, along which the time is reckoned; and if from any point of this line denoting a certain time an ordinate be drawn to the corresponding point of the curved line, the length of this ordinate will represent the state of the magnetic element at this time.

The register is taken from 10 A.M. of one day to the same hour of the next, and the curve proceeds (in point of time) from the left to the right of the paper. In full size, the length of the zero-line is about 18 inches for twenty-four hours, so that three-quarters of an inch denote one hour; but on the reduced scale appended to this paper, three-tenths of an inch denote one hour. The exact Kew mean time corresponding to the commencement and end of each curve is stated at the conclusion of this paper. Increasing ordinates denote decreasing westerly declination, decreasing horizontal, and decreasing vertical force. In the reduced scale which accompanies this paper, a change of one inch in the ordinate represents a change of 55′ in the declination, while for the horizontal force it denotes a change equal to ·0237 of the whole, and for the vertical force a change equal to ·006 of the whole.

Referring to the curves, it will be seen that the first disturbance commenced about half-past ten on the evening of the 28th of August, affecting all the elements simultaneously. It will also be observed that for the early part of this day, before the disturbance commenced, the curves present a peculiar serrated appearance. This is a phenomenon which often precedes and follows large disturbances.

At about 7½ P.M., August 29, the violence of the disturbance had somewhat abated, and things remained nearly in this state until 5 A.M., September 2, about which time another very abrupt disturbance simultaneously affected all the elements, continuing with great violence until about 4 P.M. of the same day, when it became somewhat less. The elements, notwithstanding, remained in a state of considerable disturbance until September 5, and scarcely attained their normal state even on September 7 or 8.

A graphical representation of the amount of disturbance is furnished by the dotted line, or line of normals, which accompanies each curve.

These normals have been furnished through the kindness of General SABINE, by whom they were calculated. They denote the probable position which the curve corresponding to each element would have occupied, had not disturbance supervened.

The normal for the declination for any hour is a mean of the daily observations at that hour during August and September, after the omission of all disturbed observations which differ from the final normal by an amount equal to or exceeding 3′·3.

The normals for the horizontal and vertical force have been obtained in a somewhat

similar manner, the separating values (corresponding to 3'·3 for the declination) being for the horizontal force ·0014 of its whole amount, and for the vertical force ·000478 of the whole.

I now proceed to notice some of the peculiarities of this magnetic storm.

It appears that we have two distinct well-marked disturbances, each commencing abruptly and ending gradually, the first of which began on the evening of August 28, and the second on the early morning of September 2. These two great disturbances correspond therefore in point of time to the two great auroral displays already alluded to.

The average effect of the disturbance of August 28 was to increase the declination, and to diminish the horizontal and vertical components of the earth's magnetic force. The appearance of the curves indicates very well the *modus operandi* of the disturbing force on this occasion. From their serrated appearance, it will be seen that a force tending to increase one of the elements was generally followed after a short interval by one of the opposite description, and *vice versâ*. The exertion of the disturbing force was thus of a throbbing or pulsatory character. The interval of time between two of these minute pulsations may be said to have varied from half a minute, or the smallest observable portion of time, up to four or five minutes.

This pulsatory character of the disturbing force agrees well with the nature of its action on telegraphic wires, in which observers have noticed that the polarity of the current changes very frequently.

Apart from these comparatively rapid and minute changes, the curves referring to this great disturbance indicate, for all the elements, pulsations in the disturbing force which have a period of from forty to fifty minutes. These pulsations are of a very violent character in the case of the declination, where the ebb and flow of the force alternately carries the needle above and below its normal position.

We have thus, as it were, two sets of waves, the first or smaller of which is superimposed upon the second or larger, just as in the ocean we sometimes see ripples caused by the wind traversing the surface of a great wave. But in addition to these there is a still more remarkable period which this great disturbance seems to have accomplished for all the elements in about six hours from its commencement, after which it started anew in the same direction as at first, to accomplish another period or grand wave, which lasted about the same time. The violence of the disturbance seems to have exhausted itself in the accomplishment of these two grand waves; for after this, although the needle was far from tranquil, yet its evagations were of a more moderate description.

Three periods are thus observable,—

1. That extending from half a minute up to four or five minutes.
2. A period of from forty to fifty minutes.
3. A period of about six hours.

It is impossible to state with accuracy what were the greatest departures from the mean values caused by this disturbance, as the curves for all the elements went beyond the sensitive paper; very approximately, however, we may estimate these as follows:—

For the declination a departure of about . . .	+2° 20′
For the horizontal force	−·04 of the whole.
For the vertical force	−·01 of the whole.

The second great disturbance commenced very abruptly on September 2, at 5 A.M. In character it was similar to that of August 28, its mean tendency being to increase the declination, and to diminish the horizontal and vertical components of the earth's magnetic force.

It is impossible to state the greatest departures from the mean caused by this disturbance; but in all probability they equalled or even exceeded those of the first. In appearance, also, the second disturbance was similar to the first, and it lost its excessive character for all the elements simultaneously about $3^h\ 40^m$ P.M. of the 2nd of September; but, as has been already mentioned, and as may be seen from the lines of normals, it did not finally subside until September 7.

After it had somewhat abated, its nature was exhibited by that peculiar serrated appearance of the curves which has already been alluded to, and which is very prominent in those of September 3rd and 4th. This appearance, however, had ceased some time before the elements finally resumed their normal values.

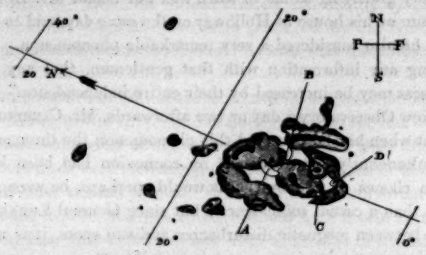
Such is a brief and very imperfect description of the leading features of this great magnetic storm, which for excessive violence of character and length of duration, I have been assured by General SABINE, has never been surpassed by any similar phenomenon which has occurred in his long and varied experience.

But, besides these two remarkable disturbances into which it divides itself, this great storm comprehends a minor disturbance, not approaching these two in extent, but yet possessing an interest peculiar to itself, which entitles it to be mentioned.

On September 1, a little before noon, Mr. R. C. CARRINGTON happened to be observing, by means of a telescope, a large spot which might then be seen on the surface of our luminary, when a remarkable appearance presented itself, which he thus describes in a communication to the Royal Astronomical Society.

"While engaged in the forenoon of Thursday, September 1, 1859, in taking my customary observation of the forms and positions of the solar spots, an appearance was witnessed which I believe to be exceedingly rare. The image of the sun's disc was, as usual with me, projected on to a plate of glass coated with distemper of a pale straw colour, and at a distance and under a power which presented a picture of about 11 inches diameter. I had secured diagrams of all the groups and detached spots, and was engaged at the time in counting from a chronometer and recording the contacts of the spots with the cross-wires used in the observation, when within the area of the great north group (the size of which had previously excited general remark), two patches of intensely bright and white light broke out, in the positions indicated in the appended diagram by the letters A and B, and of the forms of the spaces left white. My first impression was, that by some chance a ray of light had penetrated a hole in the screen

attached to the object-glass, by which the general image is thrown into shade, for the brilliancy was fully equal to that of direct sunlight; but, by at once interrupting the



current observation, and causing the image to move by turning the R.A. handle, I saw I was an unprepared witness of a very different affair. I thereupon noted down the time by the chronometer, and, seeing the outburst to be very rapidly on the increase, and being somewhat flurried by the surprise, I hastily ran to call some one to witness the exhibition with me, and on returning within 60 seconds, was mortified to find that it was already much changed and enfeebled. Very shortly afterwards the last trace was gone; and although I maintained a strict watch for nearly an hour, no recurrence took place. The last traces were at C and D, the patches having travelled considerably from their first position, and vanishing as two rapidly fading dots of white light. The instant of the first outburst was not 15 seconds different from $11^h\ 18^m$ Greenwich mean time, and $11^h\ 23^m$ was taken for the time of disappearance. In this lapse of five minutes, the two patches of light traversed a space of about 35,000 miles, as may be seen by the diagram, which is given exactly on a scale of 12 inches to the sun's diameter. On this scale the section of the earth will be very nearly equal in area to that of the detached spot situated most to the north in the diagram, and the section of Jupiter would about cover the area of the larger group, without including the outlying portions. It was impossible, on first witnessing an appearance so similar to a sudden conflagration, not to expect a considerable result in the way of alteration of the details of the group in which it occurred; and I was certainly surprised, on referring to the sketch which I had carefully and satisfactorily (and I may add fortunately) finished before the occurrence, at finding myself unable to recognize any change whatever as having taken place. The impression left upon me is, that the phenomenon took place at an elevation considerably above the general surface of the sun, and, accordingly, altogether above and over the great group in which it was seen projected. Both in figure and position the patches of light seemed entirely independent of the configuration of the great spot, and of its parts, whether nucleus or umbra. The customary observation was shortly resumed; and the diagram engraved,

as well as the larger drawing exhibited at the Meeting on November 11, was deduced from an exact reduction of the recorded times.

"It has been very gratifying to me to learn that our friend Mr. HODGSON chanced to be observing the sun at his house at Holloway on the same day, and to hear that he was a witness of what he also considered a very remarkable phenomenon. I have carefully avoided exchanging any information with that gentleman, that any value which the accounts may possess may be increased by their entire independence."

On calling at Kew Observatory a day or two afterwards, Mr. CARRINGTON learned that at the very moment when he had observed this phenomenon the three magnetic elements at Kew were simultaneously disturbed. If no connexion had been known to subsist between these two classes of phenomena, it would, perhaps, be wrong to consider this in any other light than a casual coincidence; but since General SABINE has proved that a relation subsists between magnetic disturbances and sun spots, it is not impossible to suppose that in this case our luminary was taken *in the act*.

This disturbance occurred as nearly as possible at $11^h\,15^m$ A.M. Greenwich mean time, on September 1, 1859, affecting all the elements simultaneously, and commencing quite abruptly.

The first or most abrupt portion of the disturbance lasted only about three minutes for all the elements; but after that there was a more gradual change in the same direction before the curve turned. This more gradual continuation of the first sudden movement lasted about seven minutes for all the elements.

The westerly declination was increased—

By the first, or three-minute movement, about . .	6·6
By the last, or seven-minute movement, about . .	6·6
In all, the disturbance increased the westerly declination	13·2

The horizontal force was diminished—

By the first movement	·005 of the whole.
By the last movement	·0025 of the whole.
In all, it was diminished by	·0075 of the whole force.

The vertical force was also diminished—

By the first movement	·0008 of the whole.
By the last movement	·0005 of the whole.
In all, it was diminished by	·0013 of the whole force.

It thus appears that the direction of this disturbance was the same for all the elements as that of the two great disturbances, the latter of which took place not many hours afterwards.

The leading features of this great storm appear to suggest something regarding the

nature of that relationship which manifestly exists between auroral displays, earth-currents, and magnetic disturbances.

I cannot think that the latter are caused directly and mainly by the two former, but rather that the three are simultaneous effects produced by the same cause. I believe that this is the opinion entertained by General SABINE, who has investigated the subject from another point of view.

A very remarkable feature of this disturbance was its period of about six hours, which is most distinctly shown in the curves of the horizontal and vertical forces. For about three hours the two components of the earth's force at Kew were diminishing, and for the next three hours these were increasing, until, after the lapse of about six hours, they had again attained their normal values. Were this due to the direct action of an electric current, it would require that this current should have flowed in the same direction for six hours; or at least that it should have been so limited in direction as to influence the earth's magnetism at Kew in the same manner for about six hours.

Referring now to the accounts collected by Professor LOOMIS of the influence of this great storm upon telegraphic wires, and also to a paper on magnetic storms and earth-currents, communicated by Mr. C. V. WALKER to the Royal Society on January 31, 1861, we find that the duration of the currents produced in the telegraphic wires is for the most part exceedingly small. The 1-minute currents (says this last author) are most in number; then, in order, the 2-minute, 3-minute, 4-minute, ½-minute, and 5-minute. Now it seems impossible that any combination of such currents of short period and rapid reversal can account for the six hours' march of the earth's force at Kew, and equally impossible not to associate these currents of small period with the rapid and minute changes which give a pulsatory character to the disturbing force, and a serrated appearance to the curves.

It is not difficult to conceive a mode of action of the primary force which would produce these effects.

We have grounds for supposing this primary disturbing force to reside in our luminary.

The earth may be viewed as the iron core of a RUHMKORFF'S machine, separated by an insulating medium (that is to say, the lower strata of the atmosphere) from a conducting medium (that is to say, the upper and rarer strata of the atmosphere).

Suppose the primary current in the sun suddenly to become increased or diminished a little. This will not produce a reversal in the magnetic state which this current has communicated to the earth, but merely a small change in its amount; or, in other words, the magnetic disturbance produced by the current will merely be somewhat increased or diminished.

The change in the primary current, heightened by the change of the iron core, will, on the other hand, produce a secondary current:

1st. Along the surface of the earth, which is sufficiently conducting for the purpose;

2nd. Along the upper strata of the atmosphere;

and this discharge will be in one direction for an increase, and in the opposite direction for a diminution of the primary current.

Disturbances would thus seem to be due to the absolute amount of the primary current, and auroras and earth-currents to the rapidity with which this current changes. Let me remark, in conclusion, that if it be true that the spots on the surface of our luminary (or action connected with these spots) are the primary cause of magnetic disturbances, it is to be hoped, since the study of the sun's disc is at present a favourite subject with observers, that ere long something more definite may be known with regard to the exact relation that subsists between these two great phenomena.

Explanation of the Plates.

Plate IX. contains reduced tracings of the declination curves.

Plates X. and XI. those of the horizontal and vertical force curves.

The curves run in order of time from the top to the bottom of the Plate, also from left to right.

The precise Kew mean time of the commencement and end of each curve is as follows:—

	d	h	m		d	h	m	
No. I. Aug.	27	22	21½	to Aug.	28	22	9½	declination Aug. 29 0 27 to Aug. 29 22 9½
II.	28	23	27	„	29	22	9½	
III.	29	22	13	„	30	22	10	
IV.	30	22	20½	„	31	22	9½	
V. Sept.	0	22	12½	„	1	22	9½	
VI.	1	22	19½	„	2	22	9½	
VII.	2	22	18	„	3	22	9½	
VIII.	3	22	12	„	4	22	11	
IX.	5	0	17	„	5	22	1	
X.	5	22	21	„	6	22	14	
XI.	6	22	25	„	7	22	9	
XII.	7	22	12	„	8	22	9	

XXIII. *On the Sources of the Nitrogen of Vegetation; with special reference to the Question whether Plants assimilate Free or uncombined Nitrogen.*

By John Bennet Lawes, *F.R.S., F.C.S.*, Joseph Henry Gilbert, *Ph.D., F.R.S., F.C.S.*, and Evan Pugh, *Ph.D., F.C.S.*

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PART SECOND.

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PART FIRST.

GENERAL HISTORY, AND STATEMENT OF THE QUESTION.

Section I.—INTRODUCTION, AND EARLY HISTORY.

The facts at the present time generally accepted regarding the ultimate composition, and the sources of the constituents, of plants, have, for the most part, received their preponderating weight of proof within the limits of the present century. But it is to the century preceding it that we must look for the establishment of much that was essential as the foundation of those advances which have since been made.

Whatever may be the value at present attached to the particular views of Hales regarding the composition and the sources of vegetable matter, we must accord to his labours, in the early part of the eighteenth century, the merit of having been guided by a proper spirit of experimental inquiry. Nor did he fail in applying to good account, and even in extending, the then existing knowledge of the material things around him which were apparently involved in the mysterious processes of vegetable growth.

With our present knowledge, however, of the general composition of plants, and of the sources of their constituents, it is easy to see how essential was a proper understanding of the chemistry of the air, and of water, to any true conceptions of the material changes involved in the vegetative process. It can, indeed, hardly excite surprise, that what may be called the germs of our present knowledge of the chemistry of plant-growth came forth almost simultaneously with the now adopted views of the composition of those universal, though not exclusive, media of vegetation—*air*, and *water*.

Accordingly, it is between the dates of 1770 and 1800 that we find Black, Scheele, Priestley, Lavoisier, Cavendish, and Watt establishing for us the facts that common air consists chiefly of nitrogen and oxygen, with a little carbonic acid, that carbonic acid itself is composed of carbon and oxygen; and that water is composed of hydrogen and oxygen; and it is within the same period that Priestley and Ingenhousz, Sennebier and Woodhouse, laboured to show the mutual relations of these bodies and vegetable growth.

But the observers last mentioned seem to have had more prominently in view the question of the influence of plants upon the media with which they were surrounded, than that of the influence of these media in contributing materially to the increased substance of plants themselves. Following closely on their footsteps, both in point of time and in general plan of research, came De Saussure. His labours were conducted towards the end of the last century and in the beginning of the present one; and their results, and the arguments he founded upon them, published by him in 1804, may be said to have indicated, if not indeed established, many of the most important facts with which we are yet acquainted regarding the sources of the constituents stored up by the growing plant. To De Saussure we owe the experimental, and even quantitative, illustration of the fact, that plants in sunlight increase in their amounts of carbon, hydrogen, and oxygen, at the expense of carbonic acid and of water. It is remarkable, too, that, in the case of the main experiment he cites on the point, he, with his very imper-

fect methods, found the increase in carbon and in the elements of water to be almost identically in the proportion in which these are known to exist in the so-denominated carbo-hydrates. He further maintained the essentialness of the so-distinguished "mineral" constituents of plants; and he pointed out, in opposition to previous views, that they were derived from the soil, and were not the result of a creative power exercised by the living plant. He also called attention to the probability that the incombustible or mineral constituents derived by plants from the soil, were the source of those found in the animals which are fed upon them.

Besides carbon, hydrogen, oxygen, and the more peculiarly mineral constituents, plants had already been shown to contain *nitrogen*. PRIESTLEY and INGENHOUSZ thought they had observed that plants absorbed the free nitrogen of the confined atmospheres in which they were placed in their experiments. SENNEBIER and WOODHOUSE arrived at an opposite conclusion. DE SAUSSURE, again, did not find that plants took up appreciable quantities of the nitrogen supplied to them in the free and gaseous form. On the other hand, he thought that his experiments indicated rather an evolution of that element at the expense of the substance of the plant, than any assimilation of it from gaseous media. On this point he further concluded that the source of the nitrogen of plants was, more probably, the nitrogenous compounds in the soil, and the small amount of ammonia which he demonstrated to exist in the atmosphere.

From his results, as a whole, DE SAUSSURE concluded that air and water contributed a much larger proportion of the dry substance of plants, than did the soils in which they grew. In his view, the fertile soil was the one which yielded liberally to the plant nitrogenous compounds and the incombustible or mineral constituents; whilst he attributed to air and water, at least the main part of the carbon, hydrogen, and oxygen of which the greater portion of the dry substance of the plant was made up.

Up to the present time, carbonic acid and water are admitted to be the chief sources of the carbon, hydrogen, and oxygen which constitute the great proportion of vegetable produce. Nor is it questioned that ammonia, and especially ammonia provided within the soil, is at least an important source of the nitrogen of such produce. But the experiments of DE SAUSSURE—however sagacious his conclusions—were less satisfactory as to the source of the nitrogen, than as to that of the carbon, hydrogen, and oxygen, of vegetable matter.

It will not be supposed, from what has just been said, that there remain no questions, of vast scientific as well as of practical interest, to be yet solved, regarding the conditions under which our different crops take up their carbon, hydrogen, and oxygen. At the same time, those who devote themselves to the subject of Agricultural Chemistry soon find that the explanation of the chemical phenomena of agricultural production awaits much more for a further elucidation of the sources, and of the modes of assimilation, of the *nitrogen* than of the other, so-called, organic elements of our crops—carbon, hydrogen, and oxygen.

In 1837, BOUSSINGAULT took up the subject of the sources of the Nitrogen of plants, where DE SAUSSURE had left it more than thirty years before. To the investigations

and conclusions of BOUSSINGAULT, and others, in connexion with this question, from the date above mentioned up to the present time, we shall have to refer pretty fully further on. It may here be mentioned, however, that already at that early period BOUSSINGAULT had so far advanced in his inquiries into the chemical statistics of certain agricultural practices on the large scale, as to be apparently led by them to see the importance of investigating much more closely the sources of the Nitrogen periodically yielded by a given area of land, over and above that which was artificially supplied to it.

We fully admit the pertinence of the considerations, and the sagacity of the observations adduced on this head, more than twenty years ago, by BOUSSINGAULT. It will, nevertheless, be well to preface the discussion of our own experimental evidence regarding the sources of the nitrogen of plants, by the statement of a few prominent and striking facts, established by investigations conducted here, at Rothamsted, illustrative of the amounts of nitrogen yielded by different crops over a given area of land, and of the relation of these amounts to certain measured, or known, sources of it. Of these points, however, we profess to speak only in a very brief and summary manner on the present occasion. The discussion in detail, of the evidence relating to them, would indeed itself exhaust the limits of our Paper. Moreover, we have already treated of this subject in a separate form, elsewhere*; and it is our intention to consider it much more fully at some future opportunity.

SECTION II.—ANNUAL YIELD OF NITROGEN PER ACRE, IN DIFFERENT CROPS†.

A.—*Yield of Nitrogen per acre when the same Crop is grown year after year on the same Land.*

The following Summary Table shows the average annual amounts of nitrogen yielded per acre, in the crops enumerated, when each was grown for a number of years consecutively on the same land, without manure.

TABLE I.

Description of Crop.	Dates of the Experiments.	Number of Years.	Average Annual yield of Nitrogen per acre, without Manure.
			lbs.
Wheat	1844—1859 inclusive	16	24·4
Barley	1852—1859 inclusive	8	24·7
Meadow Hay.....	1856—1859 inclusive	4	39·4
Beans.....	1847—1858 inclusive	12	47·8

There were obtained, then, in each of the Cereal crops (wheat and barley) about 24½ lbs. of Nitrogen per acre, per annum, without manure. In the case of each of the crops the land was, in an agricultural sense, exhausted at the commencement of the

* British Association for the Advancement of Science, Leeds Meeting (1858), Section B.

† The results given in this Section have been revised, and in some cases the periods over which the estimates are taken extended, since the reading of the Paper.

experiment; that is to say, it had been brought to such a condition by previous cropping, that, in the ordinary course of practice, it would be deemed necessary to supply manure to it before growing another corn-crop. It may be further remarked that in the case of the wheat there is as yet little, but in that of the barley more obvious indication of progressive decline in the annual yield.

The meadow-land yielded nearly 40 lbs. of Nitrogen per acre, per annum, or above one-half more than the exclusively Graminaceous crops, wheat and barley. The heterogeneous produce, meadow-hay, contained, however, a good deal of Trifolium, and other Leguminous plants, intermixed with the Grasses. To this fact is to be attributed, at least in great part, its comparatively high amount of Nitrogen. It should be observed, too, that the average is as yet taken over only four years.

The Leguminous crop (beans) has given, over a period of twelve years, an average of nearly 48 lbs. of Nitrogen per acre, per annum. The yield of Nitrogen in this *Leguminous* crop was, therefore, nearly twice as great as in the *Graminaceous* corn-crops. The bean and allied crops are, however, very subject to disease, especially when grown too frequently on the same land. It is, at least in part, owing to this circumstance, that the average annual yield over the twelve years was so much less than would be the yield of the crop when grown in suitable alternation with others in a season of average adaptation for its healthy development. In fact, so great was the deterioration in the character and amount of produce in the experiments in question, due to the continuous cropping, that whilst the average annual yield of Nitrogen over the first six of the twelve years was 70 lbs., that over the concluding six years was only 26 lbs. Nor did the addition of nitrogenous manure in the form of ammonia-salts, together with liberal mineral manuring, obviate this deterioration in any material degree more than did mineral manures alone.

In further illustration of the larger amount of Nitrogen obtained over a given area of land in Leguminous crops than in Graminaceous ones, some remarkable results with *clover* may be cited. Red clover was grown in three out of four consecutive years, the intermediate crop being wheat—all without manure. The following amounts of Nitrogen were obtained per acre:—

TABLE II.

Season.	Crop.	Nitrogen per acre.
		lbs.
1st Year, 1849	Clover.	206·8
(2nd Year, 1850 ...	Wheat.	45·2)
3rd Year, 1851	Clover.	29·3
4th Year, 1852	Clover.	111·9
Average of the three years Clover		116·0

All further attempts to grow clover year after year, on this land, have, however, failed. Neither ammonia-salts, nor organic matter rich in carbon as well as other constituents. nor mineral manures, nor a mixture of all has availed to restore the

clover-yielding capabilities of the land. On the other hand, it should be particularly observed that, after taking 206·8 lbs. of Nitrogen from an acre of land in the clover-crop of the first year, the wheat-crop of the second or succeeding year, compared with that of the same season in the adjoining experimental wheat-field where the crop is grown year after year on the same land, was about double that obtained from the plot which had there been unmanured for a series of consecutive years, and fully equal to that from a plot which had for the same period received annually a dressing of farm-yard manure. It should be added that, after failing to get any crop of clover at all in 1853 and in 1854, and getting a very poor one in 1855, the land was allowed to lie fallow for two years; that after this, in 1858, there was obtained an over-luxuriant and laid crop of barley, more than twice as great as the average annual produce of eight years of the successive growth of the crop without manure in the same field; yet, after resowing with clover in the spring of 1859, and getting a small cutting in the autumn of the same year, the plant has again died off during the winter of 1859–60. This was the case notwithstanding that it was a perennial variety that was last sown.

Again, eight consecutive crops of *turnips* (four "White Globe" and four "Swedish") gave an average annual yield, per acre, of about 40 lbs. of Nitrogen, without the supply of any in the manure. In the case of these turnips, however, the land received annually certain "mineral" manures. In fact, turnips grown year after year without manure of any kind, yielded, after a few years, only a few hundred-weights of produce per acre; but the *percentage* of Nitrogen in these diminutive unmanured turnips was very unusually high. It will be observed that the average annual yield of Nitrogen per acre, in the turnips grown by mineral manures (containing no Nitrogen), was considerably more than that in the unmanured Cereal grain-crops. And, in connexion with this point, it is worthy of remark, that, on barley, without manure, succeeding on the land from which these eight mineral-manured turnip-crops had been taken, the produce was only about three-fourths as much as that obtained, in the same season, where barley was grown for the second year in succession without manure, in another field; and it was only about three-fifths as much as that obtained, also in the same season, where barley was grown as the second crop of the second course, in a series of entirely unmanured four-course Rotation-crops.

It may be mentioned that, in the case of the purely Graminaceous crops, there has been but very little gain in the annual yield of Nitrogen per acre by the use of mineral or non-nitrogenous manures. But in the case of the Leguminous crops, as in that of the root-crops just referred to, there has been much more Nitrogen harvested over a given area, within a given time, when mineral manures were employed, than when no manure at all was used.

It has thus far been seen, then, that the Leguminous crops yield much more Nitrogen over a given area than the Graminaceous ones, and, further, that the amount of Nitrogen harvested in the former is increased considerably by the use of "mineral" manures, whilst that in the latter is so in a very limited degree. It is, nevertheless, a well-known agricultural fact, that the growth of the *Leguminous* crops, which *carry off* such a com-

paratively large amount of Nitrogen, is one of the best preparations for the after-growth of wheat. On the other hand, it is equally true that *fallow*—one important effect of which is to *accumulate* within the soil the available Nitrogen of two or more years for the growth of one—and *adding nitrogenous manures*, have each much the same effect in increasing the produce of the Cereal crops.

B.—*Yield of Nitrogen per acre when Wheat is grown in alternation with Beans, or with Fallow.*

The striking and interesting fact, that the growth (and removal from the land) of a highly nitrogenized Leguminous crop, and fallow, have each the effect of increasing the amount of produce, and with it the yield of Nitrogen per acre, of a succeeding Cereal crop, is briefly illustrated by the summary of direct experimental results given in the following Table:—

TABLE III.

Showing the Amount of Nitrogen obtained per acre, in Wheat grown consecutively, in Wheat alternated with Beans, and in Wheat alternated with Fallow.

Period of Experiment ten years, 1850—1859 inclusive.

		Nitrogen per acre, lbs.	
		Total.	Average annual.
Beans—10 crops consecutively	without Manure	346·9	34·7
	with Mineral Manure	510·6	51·1
Wheat—without Manure	10 Crops consecutively	234·0	23·4
	5 Crops alternated with Fallow	219·3	43·9 or 21·9
Wheat / Beans — without Manure	5 Crops alternated with Beans	225·8	45·2 or 22·6
	5 Crops alternated with Wheat	244·5	48·9 or 24·5
Wheat / Beans — with Mineral Manure	5 Crops alternated with Beans	207·0	41·4 or 20·7
	5 Crops alternated with Wheat	227·2	45·4 or 22·7

It is seen, then, that ten consecutive crops of beans, without manure of any kind, gave an average annual yield of Nitrogen, per acre, of 34·7 lbs.; and ten consecutive crops with "mineral" but without nitrogenous manure gave an average annual yield, per acre, of 51·1 lbs.

During the same period, ten consecutive crops of wheat without manure of any kind gave annually 23·4 lbs. of Nitrogen, or less than half as much as the beans with mineral but without nitrogenous manure. Again, extending over the same series of years, five crops of wheat *alternated with fallow* gave, taking the average of the five years under crop, 43·9 lbs., and on the average of the ten years, 21·9 lbs. per acre, per annum, of Nitrogen. That is to say, the wheat alternated with fallow gave, taking the average of the five years of its growth, nearly twice as much Nitrogen annually as the wheat grown after wheat in the same seasons. The total Nitrogen obtained, per acre, over the ten years, was, however, pretty much the same in the two

cases,—namely, 234 lbs. in the ten crops of wheat grown consecutively, and 219·3 lbs. in the five crops of wheat alternated with fallow.

Again, five crops of wheat *alternated with beans* gave 45·2 lbs. of Nitrogen per acre, per annum, over the five years—equal half that amount, or 22·6 lbs., averaged over the ten years. The total amount of Nitrogen obtained during the ten years was, in the ten crops of wheat grown consecutively, 234 lbs., in the five crops of wheat alternated with fallow, 219·3 lbs., and in the five crops of wheat alternated with beans, 225·8 lbs. —or not very materially different in the three cases. But, notwithstanding that the land has thus yielded in wheat, over ten years, almost as much total Nitrogen in five crops alternated with beans, as in ten crops grown consecutively, and rather more than in five crops alternated with fallow, the five intermediate crops of beans have, in addition to this, themselves carried off more than the same amount of Nitrogen as the wheat—namely, 244·5 lbs.

The general result is, then, that pretty nearly the same amount of Nitrogen was taken from a given area of land in wheat, in ten years, whether ten crops were grown consecutively, five crops in alternation with fallow, or five crops in alternation with beans. In fact, the crop of wheat was increased fully as much when it succeeded *beans*, which carried off a large amount of Nitrogen, and of mineral matters also, as when it succeeded *fallow*, which conserved the stores both of Nitrogen and of mineral matter.

It will be seen, by the illustrations given in the next sub-section (C.), that the experimental results thus far adduced are perfectly consistent in character with those obtained under circumstances more nearly allied to those of ordinary farm practice.

C.—*Yield of Nitrogen per acre when crops are grown in an actual course of rotation.*

In BOUSSINGAULT'S experiments, he obtained, taking the results of six separate courses of rotation, an average of between one-third and one-half more Nitrogen in the produce than had been supplied in the manure. He found, moreover, that the largest yields of Nitrogen were in the Leguminous crops, and, further, that the Cereal crops were the larger when they next succeeded upon the removal of the highly nitrogenized Leguminous crops.

For our own experiments at Rothamsted upon an actual course of rotation, a piece of land was selected which was, in an agricultural sense, exhausted; that is to say, it had grown a course of crops since the application of manure, and would, under ordinary practice, have received a new supply before growing another crop. On this land the four-course rotation of Turnips, Barley, Leguminous crop (or Fallow), and Wheat, in the order of succession here enumerated, and *without manure*, has now been followed for twelve years—that is, through three separate courses. The yield of Nitrogen during these twelve years, or three courses, has been determined; and the result shows an average annual amount, per acre, of 42·6 lbs. This, it will be remembered, is nearly twice as much as was obtained in either wheat or barley when these crops were, respectively, grown year after year on the same land. The greatest yield of Nitrogen obtained

in the Rotation experiment was in the case of a clover-crop, grown once during the twelve years, and which constituted the Leguminous crop of the first course. After both this clover-crop (in which was removed such a large amount of Nitrogen) and beans which replaced it in the second and third courses (but which gave a very small yield of Nitrogen), the wheat-crop was about double as much as the average where wheat has been grown succeeding wheat, and it was about equal to the average per crop when wheat was grown after fallow, or after beans, in the experiments already referred to.

It has been seen, then:—that even Cereal crops grown, year after year, on the same land, gave an average of about 24½ lbs. of Nitrogen per acre, per annum; that, under similar circumstances, Leguminous crops gave much more; that, nevertheless, the produce of the Cereal crop was nearly doubled when it was preceded by the more highly nitrogenized Leguminous crop; that the produce of the Cereal crop was also nearly doubled when it was preceded by fallow; and lastly, that in an actual rotation of crops, though entirely without manure, there was also an average annual yield of Nitrogen nearly twice as great as that obtained in the continuously grown Cereal.

It has been incidentally mentioned, too, that the highly nitrogenous Leguminous crops are comparatively little benefited by the direct application of nitrogenous manures (ammonia-salts). It has also further been stated, on the other hand, that, notwithstanding the comparatively small amount of Nitrogen harvested in a Cereal crop, and that both the crop and its Nitrogen are very much increased when succeeding upon the growth and removal of a highly nitrogenous Leguminous crop, yet the application of nitrogenous manures is also one of the surest means of increasing the produce, and the yield of Nitrogen, of a Cereal crop.

D.—*Relation of the increased yield of Nitrogen in the produce, to the amount supplied, when nitrogenous manures are employed.*

Not only do we harvest in our crops (particularly the Leguminous ones) a large amount of Nitrogen, the source of which, it will afterwards be seen, is by no means fully explained, but, when we increase their growth (particularly that of the Cereals) by the direct application of nitrogenous manures, it is found that, over a series of years, a considerable proportion of the so-supplied Nitrogen is not recovered in the increase of crop.

Thus, when a certain amount of ammonia-salts (in addition to a complex mineral manure) was applied, year after year, for the growth of wheat, the result, taken over a period of six years, was, that the increased yield of Nitrogen in the crop was only equal to about 43 per cent. of the Nitrogen which had been supplied in the manure. When double the amount of ammonia-salts was employed, by which the crop was still further increased, the proportion of the supplied Nitrogen which was recovered as increase was almost identically the same; but with more still, the proportion was less.

Again, when the smaller amount of ammonia-salts was applied annually, for six years, to barley, the increased yield of Nitrogen corresponded to only about 42 per cent. of the

Nitrogen supplied in the manure; and when the double amount of the manure was employed for barley, over the same series of years, only about 43 per cent. of the supplied Nitrogen were recovered as increased yield.

To the statement of these facts it should be added that the Nitrogen (equal in amount to, say 60 per cent. of that supplied in the manure) which is not obtained as increased yield in the immediate crop does not appear to exist in the soil availably for an immediately succeeding crop. Thus, when by the use of nitrogenous manures an increased yield of Nitrogen has been obtained in the first succeeding wheat-crop, equal in amount to about 40 per cent. of the Nitrogen supplied in the manure, the increased yield obtained in the second crop, without any further supply, is equal to little more than one-tenth of the remainder.

In connexion with this subject it may be mentioned that, so far as our experiments with *meadow-grasses* at present show, it does not appear that the increased yield of Nitrogen in the crop on the use of nitrogenous manures bears a much higher proportion to the amount supplied in their case than in that of either wheat or barley. In the case of the Leguminous corn-crops, the proportion of the increased yield to the amount supplied appears to be even less than in that of the Cereal grains. Root-crops, on the other hand, would seem to gather up an increase of Nitrogen bearing a larger proportion to the quantity directly supplied in the manure.

On the assumption that the relation of the immediately increased yield of Nitrogen to the amount supplied in manure represents really or approximately the proportion of the directly supplied Nitrogen which is actually recovered in the immediate crop, the following questions seem to suggest themselves:—

Is the unrecovered amount of supplied Nitrogen, or at any rate a considerable proportion of it, drained away and lost?

Are the nitrogenous compounds transformed within the soil, and their Nitrogen, in some form, evaporated?

Does the missing amount for the most part remain in some fixed combination in the soil, only to be yielded up, if ever, in the course of a long series of years?

Is ammonia itself, or Nitrogen in the free state, or in some other form of combination than ammonia, given off from the surface of the growing plant? Or, lastly,

When Nitrogen is supplied within the soil for the increased growth of the Graminaceous crop, is there simply an unfavourable distribution of it, considered in relation to the distribution of the underground feeders of the crop?—the Leguminous crop, which alternates with it, gathering from a more extended range of soil, and leaving a residue of assimilable Nitrogen within the range of collection of a next succeeding Cereal one?

But other and wider questions than those just enumerated present themselves on a careful review, as a whole, of the Nitrogen-statistics of field-produce to which attention has briefly been directed. For the moment, all may be asked in one—namely, What

are the sources of all the Nitrogen of our crops beyond that which is directly supplied to the soil by artificial means? This brings us to a consideration of the next Section of our subject.

Section III.—General view of the various actual or possible sources of the Nitrogen of our crops.

The following actual or possible sources of the Nitrogen obtained in our crops, beyond that supplied in manure, may be enumerated:—

1. The Nitrogen in certain constituent minerals of the soil, especially the ferruginous and aluminous; and certain nitrides.

2. The combined Nitrogen annually coming down in the aqueous depositions from the atmosphere:—
   - (*a*) As ammonia.
   - (*b*) As nitric acid.
   - (*c*) As organic corpuscles, &c.

3. The accumulation by the soil of combined Nitrogen from the atmosphere:—
   - (*a*) By surface absorption aided by moisture.
   - (*b*) By the chemical action of certain mineral constituents of the soil.
   - (*c*) By the chemical action of certain organic compounds in the soil.

4. The formation of ammonia in the soil, from free Nitrogen, and nascent Hydrogen (the so-formed ammonia either remaining as such, or being oxidated into nitric acid).

5. The formation of nitric acid from free Nitrogen:—
   - (*a*) By electric action.
   - (*b*) With common Oxygen, in contact with porous and alkaline substances.
   - (*c*) Under the influence of Ozone, or nascent Oxygen.

6. The direct absorption of combined Nitrogen from the atmosphere, by plants themselves.

7. The assimilation of free Nitrogen by plants.

A careful consideration of the above actual or possible sources of the Nitrogen of the vegetation which covers the earth's surface will show, in regard to some of them, that they at least are quantitatively inadequate to supply the amounts of Nitrogen which direct experiment has shown to be removable in various crops from a given area of land.

(1) The combined Nitrogen that may be due to certain of the constituent minerals of the débris of which our soils are made up cannot be supposed to be an adequate source of the nitrogen annually carried off in the vegetable produce of the land.

(2) The combined Nitrogen which comes down from the atmosphere in the various aqueous deposits of rain, hail, snow, mists, fog, and dew—whether it be merely the return from previously existing generations of plants or animals elsewhere, or whether in part the product of a new formation—undoubtedly does contribute materially to the

annual yield of Nitrogen in our crops. The amount of Nitrogen derivable from these sources is, moreover, perhaps more readily quantitatively estimated than that from any of the other sources enumerated. Accordingly, much labour has, of late years, been bestowed in determining the amounts of ammonia and nitric acid in these several aqueous deposits. Extensive series of observations have been made on these points by BOUSSINGAULT, BARRAL, WAY, and two of ourselves; and others have experimented on a more limited scale. It may be stated, generally, that the rain of the open country has indicated an average of very nearly the same amounts of ammonia in the hands of BOUSSINGAULT in Alsace, and of WAY and ourselves in England. The most numerous and reliable determinations of the amount of nitric acid in rain-water are probably those of Mr. WAY.

By the aid of numerous determinations of the ammonia by ourselves, and of both the ammonia and nitric acid by Mr. WAY, we are enabled to form an estimate of the total amount of Nitrogen coming down as ammonia and nitric acid in the total rain, hail, and snow, and in some of the minor aqueous deposits, during the years 1853, 1855, and 1856, here at Rothamsted, where the experiments relating to the acreage yield of Nitrogen in the different crops were made. The result was, that in neither of the three years did the Nitrogen so coming down as ammonia and nitric acid amount to 10 lbs. per acre.

Supposing the combined Nitrogen coming down in the direct aqueous deposits were to be estimated, in round numbers, at 10 lbs. per acre, per annum, this amount would supply less than half as much Nitrogen as was annually removed in the continuously grown wheat and barley crops. It would amount to only about one-fourth of that which was obtained in the hay, and in the turnips; to a less proportion of that obtained in beans; and to a still less proportion of that obtained in the clover. Lastly, it would amount to only about one-fourth as much as was obtained per annum, over twelve years of ordinary Rotation, but without manure of any kind either during that period or for some years previously.

We are driven, then, to seek for other sources of the Nitrogen of our crops, than that which comes down as ammonia and nitric acid in the more direct and more easily measurable aqueous deposits from the atmosphere. Nor does it appear, so far as can be judged from the results of BOUSSINGAULT on this point, that the amounts of combined Nitrogen deposited by dew are such as to lead to the supposition that our approximate estimate would require any material modification, were as large a proportion of dew included in our collected and analysed aqueous deposits as is probably received by the soil itself or the vegetation which may cover it.

(3) With regard to the amounts of combined Nitrogen accumulated by the soil from the atmosphere by virtue of surface absorption, or chemical action, it is probable that they constitute no inconsiderable proportion of that which is annually available for vegetation over a given area of land. Numerous investigations have indeed been undertaken during the last few years, both by ourselves and others, to determine the actual or relative capacities for absorption of different soils, or constituents of soils. Unfortu-

nately, however, even quantitative results established by laboratory methods do not admit of very direct and certain application in accounting, quantitatively, for the amount of combined nitrogen that may be so fixed, to a given depth, over a given area of land, within a given time. We hope, however, to treat of this subject in some detail on some future occasion.

(4 & 5) The circumstances of the formation of ammonia, or nitric acid, from gaseous, dissolved, or nascent Nitrogen, are at present involved in too much obscurity, and are the subject of too much conflicting statement for their consideration to serve us much in our present inquiry. The various assumed actions are, as yet, by no means all clearly established in a merely qualitative way; and still less, quantitatively. Moreover, as in the case of absorption, so in that of the formation of ammonia, or of nitric acid, there would be considerable difficulty and uncertainty in applying the results of laboratory experiments to the estimation of the probable amount of the Nitrogen of vegetation due to such sources. To some of the questions involved, we shall, however, have to refer more or less in detail in discussing the conditions of the experiments which will form the subject of the second part of the present Paper.

(6) With regard to the direct absorption of ammonia or nitric acid from the air by plants themselves, we have little of either qualitative or quantitative evidence of any kind to guide us. Still, a few observations may be usefully hazarded, in passing, which may bear more or less directly upon the point.

In our ripened Cereal crops, we find 1 part of Nitrogen to somewhere about 30 parts of carbon; and in our Leguminous crops, 1 part of Nitrogen to about 15 or fewer parts of carbon. It is supposed that the atmosphere, on the average, contains 1 part (or rather more) of carbon in the form of carbonic acid to 10,000 parts of air. We may perhaps assume, as an extreme amount, that the atmosphere contains only 1 part of Nitrogen in the form of ammonia to about 12,000,000 parts of air. Adopting these assumptions, there would obviously be, instead of only 30 or 15 times less Nitrogen than carbon (as in the respective crops), 1200 times less Nitrogen in the ambient air in the form of ammonia, than of carbon in the form of carbonic acid in the same medium.

If, however, we were to adopt as more nearly the amount of ammonia in the air that found by M. G. Ville (namely, only about one-fifth as much as we have assumed above), it would then appear that there were 6000 times less of Nitrogen in the air in the form of ammonia, than of carbon in that of carbonic acid.

Taking the former or more favourable assumption of the two, the result would be, that the ambient atmosphere contained Nitrogen as ammonia, to carbon as carbonic acid, in a proportion 40 times less than that of Nitrogen to carbon in the Cereal produce, and 80 times (or more) less than that of Nitrogen to carbon in the Leguminous produce. Adopting M. G. Ville's estimates, on the other hand, the proportion of the so-combined Nitrogen to the so-combined carbon, in the air, would be 200 times less than that of the Nitrogen to the carbon in the Cereal crops, and about 400 times less than that of the Nitrogen to the carbon in the Leguminous crops.

Looking, therefore, at the subject from the point of view of *actual quantity merely*, the ammonia in the atmosphere would appear very inadequate to yield Nitrogen in a degree at all corresponding to the yield of carbon by carbonic acid. It would appear too, from the observations hitherto recorded bearing upon the point, that the amount of Nitrogen existing in the atmosphere as nitric acid is very much less than that existing as ammonia. Hence, the inclusion, in the estimate of the combined Nitrogen in the atmosphere, of the amount existing as nitric acid would, in point of quantity, by no means materially affect the question.

But it is worthy of remark, in reference to the question of the proportion of Nitrogen as ammonia to carbon as carbonic acid, that may be available to vegetation from atmospheric sources, that, although the actual amount of Nitrogen as ammonia in the atmosphere is very small compared with that of the carbon as carbonic acid, yet, a given amount of water would absorb very much more Nitrogen as ammonia, or dissolve very much more Nitrogen as carbonate of ammonia, than it would absorb of carbon in the form of carbonic acid under equal circumstances. In illustration, it may be mentioned that water at 60° F. (about 15°·5 C.) would at the normal pressure absorb about 850 times as much Nitrogen in the form of ammonia as it would of carbon in the form of carbonic acid; and, under equal circumstances, very many times more Nitrogen as carbonate or even as bicarbonate of ammonia would be dissolved, than there would be of carbon as carbonic acid absorbed. There would appear to be, then, a compensating quality for the small actual amount of Nitrogen as ammonia in proportion to carbon as carbonic acid in the atmosphere, in the greater absorbability or solubility of the compounds in which Nitrogen exists than of the carbonic acid in which the carbon is presented. How far, however, the compensating quality here suggested may really influence the proportion of the Nitrogen to the carbon available from the atmosphere, in the combined form, under the actual conditions involved in vegetation, is a question the numerous and intricate bearings of which we do not profess here to enter upon.

Before passing from this question of the direct absorption of Nitrogen in the combined form from the atmosphere by plants themselves, one or two further observations may yet be made which are suggested by the actual facts of agricultural production. It is undoubtedly the case that the Graminaceous crops depend very materially upon combined Nitrogen *within the soil*, to determine the amount of their produce. They seem, however, to be comparatively independent of carbonic acid yielded by manure within the soil. The Leguminous crops, on the other hand, appear to be much less benefited by direct supplies of characteristically nitrogenous manures. It would hence seem that they are more able to avail themselves of Nitrogen supplied in some way by the atmosphere, possibly by the aid of their green parts. But it can hardly be to a greater *mere extent of surface above ground* that the property which the Leguminous plants possess of acquiring a greater amount of Nitrogen than the Graminaceous ones, over a given area of land, and under otherwise equal circumstances, is to be attributed.

A bean and a wheat crop may yield equal amounts of dry matter per acre, whilst the bean produce would contain from two to three times as much Nitrogen as the wheat. Nevertheless some attempts at approximate measurement have indicated that the wheat-plant offers a greater external superficies in relation to a given weight of dry substance, than does the bean. The wheat-plant would, of course, show a still higher relation of superficies to a given amount of Nitrogen fixed. If, therefore, the larger amount of Nitrogen yielded per acre by a bean than by a wheat crop be due to a larger assumption of it directly from atmospheric sources in some form, it is obvious that the result must be due to *character*, and *function*, and not to mere extent of surface above ground. In connexion with this point it may be observed, more particularly with reference to the crops that are grown for their ripened seed, that the Leguminous ones generally maintain their green and succulent surface in relation to a more extended period of the season of active growth and accumulation than do the Graminaceous ones.

(7) *Assimilation of free or uncombined Nitrogen by Plants.*—It has been seen, in the course of the foregoing brief review of the various sources of combined Nitrogen to plants, that those of them which have as yet been quantitatively estimated are inadequate to account for the amounts of Nitrogen obtained in the annual produce of a given area of land beyond that which may be attributable to the supplies by previous manuring. It must be admitted, too, that the sources of combined Nitrogen which have been alluded to as not yet even approximately estimated in a quantitative sense (if indeed they are all fully established qualitatively) offer many practical difficulties in the way of any such investigation of them as would afford results directly applicable to our present purpose. It appeared, therefore, that it would be desirable to settle the question, whether or not that vast storehouse of Nitrogen, the atmosphere, in which the vegetation which covers the Earth's surface is seen to live and flourish, be of any measurable avail to the growing plant, so far as its *free* or *uncombined Nitrogen* is concerned.

The settlement of this question (whether affirmatively or negatively) would at any rate indicate the degree of importance to be attached to the remaining open points of inquiry. Indeed, were it found that plants generally, or some of those we cultivate more than others, were able to fix Nitrogen from that presented to them in the free or uncombined form, we should, in this fact, have a clue to the explanation of much that is yet clouded in obscurity in connexion with the chemical phenomena of Agricultural production. We should establish for vegetation, the attribute of effecting chemical combinations with an element at once the most reluctant to associate itself with other bodies in obedience to laboratory processes and at the same time apt to rid itself of connexions once formed in the most violent manner—as the explosive character of many Nitrogen compounds forcibly illustrates. We should further be able, much more satisfactorily than we are at present, to account—by processes established to be going on under our own observation—for the actually large total amount of combined Nitrogen which we know to exist and to circulate, in land and water, in animal and vegetable life, and in the atmosphere.

But another and potent reason for investigating the relation of plants to the free or uncombined Nitrogen of the atmosphere is to be found in the fact, that the question has, of late years, been submitted to an immense amount of research by numerous experimenters, and from the results obtained very opposite conclusions have been arrived at. Thus, M. Boussingault concludes that plants do not assimilate the free or uncombined Nitrogen of the atmosphere. M. G. Ville maintains, on the contrary, that the assimilation of free Nitrogen does take place, and further, that, under favourable circumstances, a considerable proportion of the Nitrogen of a plant may be derived from this source. Others have experimented in connexion with the subject on a more limited scale; and various explanations have been offered of the discrepant results and conclusions of M. Boussingault and M. G. Ville.

Before entering upon the discussion of our own experimental evidence in regard to the question of the assimilation of free or uncombined Nitrogen by plants, it will be desirable to pass in review the methods, results, and conclusions of M. Boussingault and M. G. Ville, and also of some other experimenters, who seem to have been led to take up the subject by a consideration of the contrary opinions arrived at by Boussingault and Ville.

Section IV.—Review of the researches of others, on the question of the assimilation of free nitrogen by plants, and on some allied points.

It has already been mentioned that, in 1837, Boussingault took up the question of the sources of the Nitrogen of Plants where De Saussure had left it more than thirty years before. De Saussure and his predecessors had sought to solve the question, among others, whether plants assimilated the free or uncombined Nitrogen of the atmosphere, by determining the changes undergone in the composition of limited volumes of air by the vegetation of plants within them. Boussingault pointed out that the methods which had been adopted were not sufficiently accurate for the determination of the point in question. The general plan instituted by himself, and adopted with more or less modification in most subsequent researches, was:—

To set seeds or plants, the amount of Nitrogen in which was estimated by the analysis of carefully chosen similar specimens.

To employ soils and water containing either no combined Nitrogen, or only known quantities of it.

To allow the access, either of free air (protecting the plants from rain and dust), of a current of air freed by washing from all combined Nitrogen, or of a fixed and limited quantity of air, too small to be of any avail so far as its compounds of Nitrogen were concerned. And finally—

To determine the amount of combined Nitrogen in the plants produced, and in the soil, pot, &c., and, so, to provide the means of estimating the gain or loss of Nitrogen during the course of the experiment.

A.—M. Boussingault's Experiments.

1. M. Boussingault's *experiments in* 1837 *and* 1838, *in which the plants were allowed free access of air, but were protected from rain and dust.*

In 1837* Boussingault grew, in burnt soil, watered with distilled water, and with the access of free air, a pot of Trifolium for two months, and another for three months; also a pot of Wheat for two months, and another for three months.

The total Nitrogen in the seeds sown in the two experiments with *Trifolium*, amounted to 0·224 gramme. The Nitrogen in the produce, soil, pot, &c., amounted to 0·276 gramme. There was a gain, therefore, of 0·052 gramme of Nitrogen = nearly 20 per cent. of the total Nitrogen of the products. The development of vegetable matter, implying, of course, the assimilation of carbon, hydrogen, and oxygen, was, however, in a much greater proportion; the dry matter of the produce in the two experiments amounting to nearly three times that of the seed sown.

In the two experiments with *Wheat*, the total Nitrogen in the seed was estimated at 0·100 gramme. The Nitrogen in the products was exactly the same amount. In the case of the Wheat, there was, therefore, no gain of Nitrogen indicated. Nevertheless the *dry matter* of the produce amounted to nearly double that of the seed.

In 1838†, Boussingault, in a similar manner, sowed *Peas* containing 0·046 gramme Nitrogen. The plants obtained; yielded flowers and ripe seed, and their dry matter was more than four times as much as that of the seed sown. The Nitrogen of the total products amounted to 0·101 gramme. Here again, therefore, the *Leguminous* plants seemed to gain Nitrogen from some undetermined source.

Boussingault made experiments in the same year (1838), with Trifolium, and with Oats. In these cases, he commenced with carefully selected plants instead of with seeds. The *Trifolium* nearly trebled its total vegetable matter during growth; and it gained 0·023 gramme of Nitrogen out of 0·056 gramme in the total products. The *Oat*, on the other hand, indicated only 0·053 gramme Nitrogen in the total products, whilst it was estimated that there was 0·059 gramme contained in the plants taken for the experiment. The total vegetable matter was, however, doubled.

The substance of M. Boussingault's conclusions from the above experimental results, may be stated as follows:—That under several conditions, certain plants seem adapted to take up the Nitrogen in the atmosphere; but that it was still a question, under what circumstances, and in what state, the Nitrogen was fixed in the plants. He submitted —that the Nitrogen might enter directly into the organism of the plant, provided its green parts were adapted to fix it; that it might be conveyed into the plant in the aërated water taken up by its roots; that, as some physicists suppose, there may exist in the atmosphere an infinitely small amount of ammoniacal vapour. He further suggested that the gain of Nitrogen beyond that supplied in manure, which he had observed in agricultural production on the large scale, and which he thought evidently

* Ann. de Chim. et de Phys. sér. 2. tome lxvii. 1838. † Ibid. tome lxix.

came from the atmosphere, might be partly due to Nitrate of Ammonia produced by electrical action and brought down by rain.

2. M. BOUSSINGAULT's *experiments in* 1851, 1852, *and* 1853, *in which the plants were confined in limited volumes of air**.

BOUSSINGAULT resumed the subject of the sources of the Nitrogen of vegetation in 1851. His object was, apparently, to settle more definitely, whether plants assimilated Nitrogen from any other source than the combined forms of it.

In his experiments in 1851 and 1852, BOUSSINGAULT confined his experimental plants under a glass shade of about 35 litres capacity, which shut off the free access of external air by resting in a lute of sulphuric acid. Tubes passed under the shade for the supply of carbonic acid, and water, as they might be needed. Pumice-stone, coarsely powdered, washed, ignited, and cooled over sulphuric acid, served as soil. To this, at the commencement, some of the ash from farm-yard manure, and also from seed of the kind to be sown, was added.

In 1851, a Haricot was grown under these conditions, the seed of which, when sown, was estimated to contain 0·0349 gramme of Nitrogen. After two months of growth, flowers being formed, the dry substance of the plant was more than double that of the seed sown; and the total products yielded only 0·0340 gramme of Nitrogen. There was, therefore, apparently a slight loss of Nitrogen, which amounted, however, to less than a milligramme. In 1852, two Haricots, sown respectively in separate pots, contained, together, 0·0455 gramme Nitrogen. They were each allowed to grow for three months, during which time the dry substance was nearly doubled; and in one instance open flowers were formed. The products of both experiments taken together yielded to analysis only 0·0415 gramme of Nitrogen. There was an apparent loss, therefore, in the two experiments, of 4 milligrammes of Nitrogen. It is seen, then, that in these new experiments with *Leguminous* plants, in which the free circulation of atmospheric air was not permitted, there was not the apparent gain of Nitrogen that had been met with in BOUSSINGAULT's early experiments (in 1837 and 1838), in which free access of air into the enclosing apparatus was allowed.

In 1851, ten seeds of Oats, and in 1852 four, were experimented upon in a similar manner. In both cases there was an apparent very slight loss of Nitrogen. In the first case the Oats vegetated for two months, and in the second for 2½ months; and in the latter, the plant arrived at the point of shooting forth the ear.

In 1853, the apparatus adopted by BOUSSINGAULT consisted of a large globe, or carboy, of white glass, having a capacity of 70 or 80 litres. At the bottom of this vessel, a matrix of pumice-stone (or burnt brick) and ashes, prepared as in the last series, was placed to serve as soil. This was watered with distilled water, and then the seeds were sown. The neck of the vessel was then closed with a cork, through a perforation in which, a flask of carbonic acid was inverted, whose aperture, opening into the globe, was

* Ann. de Chim. et de Phys. sér. 3. tome xli. 1854.

somewhat contracted. Finally, access of air from without was excluded by bandages of caoutchouc, which were so secured as to render the whole apparatus air-tight.

In such an apparatus, BOUSSINGAULT made five separate experiments with White Lupins. In all he sowed thirteen seeds, which were estimated to contain, together, 0·2710 gramme of Nitrogen. The experiments extended over periods varying from six to eight weeks. In one instance, burnt brick instead of pumice-stone was used as the soil; and in this case, as well as in one where pumice was used, bone-phosphate as well as ashes was added as manure. The dry matter of the produce was about three times as much as was contained in the thirteen seeds sown. The Nitrogen in the total products of the five experiments amounted to 0·2669 gramme. There was, therefore, a loss, in the five experiments taken together, of about 4 milligrammes of Nitrogen. In two of the cases there was a slight gain of Nitrogen, but in neither instance did it amount to 1 milligramme.

In a similar apparatus, two experiments were made with Dwarf Haricots, a single seed only being sown in each case. One of the experiments extended over two months, and the other over two and a half months. In both instances flowers were formed, and in one of them seed. The dry matter of the produce was three to four times as much as that of the seed sown. Taking the two experiments together, the Nitrogen contained in the seed was estimated at 0·0652 gramme; and that found in the products amounted to 0·0637 gramme. There was a loss, therefore, of $1\frac{1}{3}$ milligramme of Nitrogen.

There was, then, in this third series of experiments with *Leguminous* plants, again rather a loss than a gain of Nitrogen,—the supplies of it in this case being confined to the combined Nitrogen contained in the seeds sown, and to the free or uncombined Nitrogen in the fixed and limited volume of air within the apparatus.

Still in the same apparatus, BOUSSINGAULT sowed Garden Cress. Thirteen seeds were sown, all of which germinated, but three plants only survived. The growth of these extended over three and a half months; and flowers and seed were produced. The Nitrogen in the products amounted to precisely as much as was estimated to be contained in the thirteen seeds sown.

The last experiment in this closed globular apparatus was as follows: Two White Lupins were sown to grow; and eight others were applied as manure, after treatment with boiling water to destroy their powers of germination. The experiment continued for a period of between four and five months. The dry matter of the produce was nearly twice as much as would be contained in the ten seeds involved in the experiment. The whole ten seeds were estimated to contain 0·1827 gramme of Nitrogen; whilst the total products yielded only 0·1697 gramme. The loss of Nitrogen was here, therefore, 13 milligrammes; or about one-fourteenth of the whole amount involved in the experiment. BOUSSINGAULT considered that the loss was probably due to free Nitrogen being given off in the process of decomposition of the organic matter employed as manure.

In order to ascertain whether the limitation of growth in the foregoing experiments was due to the limitation in the amount of air, or to a deficiency of available Nitrogen in the matters used as soil, BOUSSINGAULT sowed Cress in a good soil, placed the vessel

in a limited atmosphere, and supplied carbonic acid. The result was, that the plants thus grown, in a limited atmosphere, but in a good soil, were even more luxuriant than a parallel set, grown in a similar soil, in the open air. In both cases a large quantity of seed was produced.

3. M. Boussingault's *experiments in* 1854, *with a current of washed air**.

In this series of experiments, Boussingault supplied his plants with a *current* of air, previously washed by passing first through vessels containing pumice-stone saturated with sulphuric acid, and then through water. He also supplied carbonic acid from bicarbonate of soda acted upon by sulphuric acid,—the gas evolved being passed first over chalk, then through a solution of carbonate of soda, and lastly over pumice-stone saturated with a solution of carbonate of soda. The enclosing apparatus consisted of a metal-framed glass case of 124 litres capacity, which was cemented down upon a polished iron plate, upon which the experimental pots were placed. Across one side of the case was a metallic joint-bar, in which were apertures for the insertion of tubes for the admission of the washed air, and for the supply of water and carbonic acid. On the opposite side was a similar joint-bar, to an aperture in which, a tube was attached connecting the case with an aspirator of 500 litres capacity. By this apparatus, therefore, the plants could be supplied with a current of air freed from ammonia, with water, and with carbonic acid, at pleasure. During the experiment, the atmosphere in the Case generally contained from 2 to 3 per cent. of carbonic acid. Lastly, by means of one of the apertures any withered leaves were removed as they fell from the plants; and they were then dried and preserved for analysis with the remainder of the products.

One of the experiments made in this apparatus was with a single Lupin, which was allowed to grow for two and a half months. The dry matter of the produce was more than six times that of the seed. The Lupin sown was estimated to contain 0·0196 gramme of Nitrogen. The Nitrogen found in the products amounted to 0·0187 gramme. There was a loss, therefore, of nine-tenths of a milligramme of nitrogen.

Four experiments were made with Dwarf Haricots, in three of which single seeds, and in the fourth two seeds, were sown. One experiment lasted over two and a half months, and the plant flowered; one over three months, and the plant seeded; one over three and a half months, in which case also the plant seeded; and another over three and a quarter months. The dry substance of the produced plants was from three to four times as much as that of the seed sown. The total Nitrogen in the five seeds employed in the four experiments was estimated at 0·1672; the Nitrogen found in the total products amounted to 0·1661 gramme. There was therefore, upon the whole, a loss of 0·0011 gramme of Nitrogen. In two of the experiments there was a loss of 1 milligramme each of Nitrogen; and in the other two a gain, amounting to less than 1 milligramme in each case.

In the next experiment, one Lupin seed was sown to grow, and another was steeped in hot water and applied as manure. The dry matter of the produce from the one seed amounted to nearly three times that of the two seeds employed in the experiment. The

* Ann. de Chim. et de Phys. sér. 3. tome xliii. 1855.

total Nitrogen in the two seeds was estimated at 0·0355 gramme. That found in the products was 0·0334 gramme. There was a loss, therefore, of 0·0021 gramme Nitrogen.

Lastly, forty-two seeds of Cress were sown, twelve of which served as manure. Many of the plants seeded. The dry matter of the produce was more than five times that of the seed. The Nitrogen in the forty-two seeds was estimated at 0·0046 gramme. That found in the products amounted to 0·0052 gramme. There was a gain, therefore, of 0·0006 gramme, or little more than half a milligramme of Nitrogen.

The whole of these experiments in 1854, in which a current of air was supplied to the plants, taken together, indicated a slight loss of Nitrogen. This was the case, notwithstanding that all the plants, excepting the Cress, were of the *Leguminous* family.

4. M. Boussingault's *experiments in* 1851, 1852, 1853, *and* 1854, *in which the Plants were allowed free access of air, but were protected from rain and dust*[*].

Contemporaneously with the several series of experiments above described, Boussingault grew plants simply covered with a case, in such a manner as to exclude any material amount of dust, but so as to allow of the free access of the external air.

Single Haricots were grown in the manner here described, in the seasons of 1851, 1852, 1853, and 1854, respectively. All four plants flowered; one podded; and one seeded. The Nitrogen in the seed of the four experiments amounted to 0·1173 gramme. That found in the vegetable produce, soil, &c., was 0·1238 gramme. There was a total gain of Nitrogen, therefore, under these circumstances, of 0·0065 gramme. In one case there was an apparent loss of Nitrogen of a little more than 2 milligrammes; in the three others the gain was about equal. The dry matter in the produce amounted to from three to four times as much as that in the seeds sown.

In the seasons of 1853 and 1854, three experiments of the same kind were made with White Lupins. The dry matter of the produce was from three to four or more times as much as that in the seed. The Nitrogen in the seed of the three experiments taken together amounted to 0·0780 gramme. That in the total products was 0·0873 gramme. Here again, therefore, there was a gain of Nitrogen—amounting in this case, in all, to between 9 and 10 milligrammes.

Under similar conditions, Oats were grown in 1852 which yielded seed. The Nitrogen sown was 0·0031 gramme. That in the products was 0·0041 gramme. There was a gain, therefore, of 1 milligramme of Nitrogen.

In like manner, five seeds of Wheat were sown in 1853. The dry matter of the produce was more than three times that of the seed. The Nitrogen in the seed was estimated at 0·0064 gramme. That in the products was 0·0075 gramme. The gain was, therefore, 0·0011 gramme.

Lastly, 210 seeds of Cress were sown in 1854. Many of the plants seeded; and there was, of course, a considerable gain of dry matter. The Nitrogen in the seed was 0·0259 gramme. That in the products amounted to 0·0272 gramme. There was a gain, therefore, of 0·0013 gramme.

[*] Ann. de Chim. et de Phys. sér. 3. tome xliii. 1855.

Taking all these experiments together, in which the plants were shaded from rain and dust, but still allowed free access of air, the total gain of Nitrogen was 0·0192 gramme upon 0·2307 gramme supplied in the seed sown. There was a gain of Nitrogen, therefore, equal to about one-twelfth of that sown in the seed. BOUSSINGAULT considered that part of the gain was due to organic corpuscles, and part to the ammonia in the atmosphere. He also considered that, bearing in mind the circumstances of the experiment, the gain was not sufficiently great to justify the conclusion that there had been any assimilation of the free or uncombined Nitrogen of the air.

5. M. BOUSSINGAULT'S *collateral experiments to control and explain his results*[*].

In order to ascertain the amount of Nitrogen that might be introduced into the materials under experiment when the matter used as soil, &c. was not excluded from the air whilst cooling after ignition, or when free access of air was allowed during the whole period of vegetation, BOUSSINGAULT instituted the following experiments.

Sand, powdered brick, powdered bone-ash, and wood-charcoal were each exposed to the air for two or three days after being ignited, and then the Nitrogen determined in them. The result was that, after this exposure, a kilogramme of sand gave 0·5 milligramme, a kilogramme of powdered brick 0·5 milligramme, a kilogramme of powdered bone-ash 0·84 milligramme, and a kilogramme of wood-charcoal 2·9 milligrammes of ammonia.

In order to test the influence of the organic corpuscles of the atmosphere, a pot of burnt sand, with ashes, the whole moistened with water, was so arranged under a shade as nevertheless to allow free access of air, and it was so exposed for two and a half months. At the end of this period small spots of cryptogamic vegetation were visible on the surface of the sand; but the whole yielded only 0·74 milligramme of Nitrogen.

Again, BOUSSINGAULT found that unless the ashes used as manure were burnt until nearly all apparent traces of carbon were destroyed, they were liable to retain more or less and sometimes material amounts of Nitrogen. In some imperfectly burnt ashes cyanides, and in some, ferrocyanides were found; in others the Nitrogen seemed to exist in neither of these conditions.

With regard to the much larger gain of Nitrogen indicated in his early experiments in free air (1837 and 1838) than in those made more recently, BOUSSINGAULT remarks that the result may be partly due to the comparatively defective methods of analysis at the early date, and partly also to the distilled water used for watering the plants containing some ammonia. For, at the time of his first experiments, he was not aware of the fact, since learned in his analyses of rain and other waters, that water distilled from that which contained minute quantities of ammonia did not come over free from it until about two-fifths of the whole had been drawn off.

It will be observed that, in most of the experiments of BOUSSINGAULT thus far passed in review, he limited the supply of Nitrogen to the plants to that contained in the seed sown, and to that which they could obtain from the atmosphere, either washed or un-

[*] Ann. de Chim. et de Phys. sér. 3. tome xliii. 1855.

washed, in which they grew. In no case among those experiments in which the modern refinements of analysis were had recourse to did he find, either with Leguminous or with other plants, such a gain of Nitrogen beyond that supplied in the seed, as could lead to the conclusion that the free or uncombined Nitrogen of the atmosphere had been assimilated. In many of the instances the plants yielded not only flowers but seed; and hence it might be concluded that the conditions provided were adequate for the performance, by the plant, of the complete course of its natural functions of growth. Still it might be objected that the vigour of growth was somewhat limited, and that, under these circumstances, the plant might well refuse to perform the, perhaps, difficult office of assimilating a very refractory elementary body. In a few instances, seeds whose germinating power had been destroyed were supplied as manure. In these cases the amount of Nitrogen assimilated by the plants was much greater than that contained in the living seed sown; and the luxuriance of growth was consequently comparatively great. Nevertheless, instead of a gain, there was generally a loss in the total amount of combined Nitrogen, which was considered to be due to the evolution of free Nitrogen by the decomposing manurial matter. To get increased vigour of growth—to avoid, if possible, a loss of Nitrogen such as is above supposed—and, at the same time, to determine whether or not the Nitrogen of Nitrates were really assimilable by plants—BOUSSINGAULT has latterly made some experiments in which Nitrates were employed as manure, a brief notice of the results of which should be here given.

6. M. BOUSSINGAULT'S *experiments in which he supplied combined Nitrogen in the form of Nitrate of Potash, or Soda**.

In 1855 BOUSSINGAULT made one experiment with Helianthus in which he supplied no nitrate to the soil, and another in which a small known quantity of Nitrate of Potash was employed. In a third experiment Cress was grown in a manured soil, in a fourth in a soil destitute of combined Nitrogen, and in a fifth in a soil to which a known quantity of Nitrate of Soda was added. In the case of the manured soil, and in the two cases where Nitrate was employed, there was a very considerable increase in the assimilation of carbon; and there was also much more Nitrogen assimilated than was supplied in the seeds sown. The increased assimilation of Nitrogen where Nitrate was used, did not, however, exceed that supplied in the manure. BOUSSINGAULT concluded that the gain of Nitrogen was to be attributed to the Nitrogen of the Nitrate.

Lastly in regard to BOUSSINGAULT'S experiments: In 1858† he resumed the question of the action of Nitrates upon vegetation. He grew two separate pots of Helianthus, two seeds being sown in each pot. The soils were composed of sand and quartz well washed from saline matter and ignited. To one pot Nitrate of Potash containing 0·0111 gramme of Nitrogen, and to the other Nitrate containing 0·0222 gramme Nitrogen, was added. In the first case, he did not get back, in the plant, soil, and pot, the Nitrogen of the seed and Nitrate by 0·0014 gramme. In the second experiment the loss of Nitrogen amounted to just 1 milligramme. BOUSSINGAULT found, however, that there remained

* Ann. de Chim. et de Phys. sér. 3. tome xlvi. 1856. † Compt. Rend. tome xlvii. 1858.

in the soil an amount of carbonate of potash very nearly corresponding in potash to the amount of nitrate of potash which would represent the observed loss of Nitrogen. He concluded that nitrate had been decomposed in the soil, by the organic matter of the débris of the seeds and of the roots, and that Nitrogen had been evolved. If we clearly understand this explanation of the loss of Nitrogen of the nitrate, we would suggest that it would seem to require for its validity that the plant should have assimilated potash from the nitrate exactly corresponding in amount to the Nitrogen it fixed from the same source.

From the results of these experiments with nitrate, BOUSSINGAULT drew the following conclusions:—

1. That there was no assimilation of free Nitrogen.
2. That there was a loss of supplied Nitrogen, either from the soil, or by the plant.
3. That, in the two cases, the amount of carbon assimilated bore a close relation to that of the Nitrogen taken up by the plant.

It is seen, then, that the results of the laborious investigations of BOUSSINGAULT, extending at intervals over a period of more than twenty years, have led him to conclude that, neither *Leguminous* plants, nor the others experimented upon, were able, either when their supplies of combined Nitrogen were limited to that contained in the seed sown, or when their vigour of growth was stimulated by artificial supplies of combined Nitrogen, to assimilate the free or uncombined Nitrogen of the atmosphere.

B.—M. G. VILLE'S EXPERIMENTS*.

1. M. G. VILLE'S *determinations of the Ammonia in the atmosphere.*

M. G. VILLE, of Paris, commenced his investigations, on the subject of the assimilation of Nitrogen by plants, in 1849. He first sought to determine the proportion of Ammonia in the atmosphere. To this end, he aspired known quantities of air through acid, and determined the amount of ammonia absorbed. He operated upon very much larger volumes than previous experimenters had done. His results show, moreover, a much smaller proportion of ammonia in the air than those of others.

The air of Paris, during part of 1849 and part of 1850, gave a mean of only 0·0237 part by weight of ammonia, to 1,000,000 parts by weight, of air; and that of the suburbs of Paris, during some period of 1852, gave 0·0211 parts of ammonia, to 1,000,000 parts of air.

2. M. G. VILLE'S *general plan of experimenting on the question of the assimilation of Nitrogen by plants.*

M. G. VILLE employed specially-made porous flower-pots, and used, as soil, washed

* Recherches Expérimentales sur la Végétation, par M. GEORGES VILLE. Paris, 1853.

and ignited sand, sand and brick, or sand and charcoal, with the addition of the ash of the plant to be grown. He planted seeds or plants, the composition of which was estimated by the analysis of parallel specimens. Several pots were for the most part enclosed in an iron-framed glazed case of 150 litres (or more) capacity, through which a current of air equal in amount to several times the volume of the vessel was aspired daily. Carbonic acid and distilled water were supplied as needed. In some cases the air admitted into the apparatus was only previously freed from dust; and then the amount of atmospheric ammonia that would be brought in was calculated according to the determination of the proportion of ammonia in the air, above alluded to. In other cases the aspired air was previously freed from ammonia by washing. In some experiments, ammoniacal gas was passed, in known quantities, into the air of the apparatus. Lastly, others were made, in which combined Nitrogen was added to the soil in the form of nitrate, or of ammonia salts; and in these cases the plants were allowed to grow in free air, only shaded from rain and dust.

3. M. G. VILLE's *experiments in* 1849 *and* 1850, *in which the plants were supplied with a current of unwashed air.*

In 1849, sand was used as soil; three pots of plants were grown for two months; namely, one of Cress, one of large Lupins, and one of small Lupins. The air admitted into the apparatus was not previously deprived of its natural ammonia. The dry substance of the produced Cress plants amounted to more than sixteen times that of the seed sown. The Nitrogen in the Cress seeds amounted to 0·026 gramme; that in the products to 0·147 gramme. The Nitrogen in the products was, therefore, between five and six times as much as that in the seed; and the actual gain of it amounted to 0·121 gramme. In the case of the large Lupins, the dry matter of the produce was about $3\frac{1}{2}$ times as great as that of the seeds sown; but there was neither gain nor loss of Nitrogen. The small Lupins gave $2\frac{1}{2}$ times as much dry substance in the produce as was supplied in the seeds; and of the Nitrogen of the seeds sown, there was an apparent loss of rather more than one-fourth, during the experiment.

The total gain of combined Nitrogen in the apparatus, taking the three experiments together, was 0·103 gramme. The Nitrogen in the ammonia of the current of unwashed air, was, however, estimated at only 0·001 gramme. M. G. VILLE concluded, therefore, that the Cress had appropriated a considerable quantity of the free or uncombined Nitrogen of the atmosphere.

The plants experimented upon in 1850, were Colza, Wheat, Rye, and Maize. In the case of the Colza, the experiment commenced with young plants, but in the other cases with seed. The four pots were placed in an apparatus similar to that used before, and the conditions supplied were also the same as in 1849.

The dry matter of the produced Colza plants amounted to between forty and fifty times as much as that of the young plants when taken for experiment. The Nitrogen was also increased more than forty-fold. The dry matter of the Wheat plants was about

four times that of the seed sown; and the Nitrogen in the total products was nearly double that in the seed. The dry substance of the produced Rye plants was five times, and their Nitrogen nearly three times that of the seed. In the experiment with Maize, the dry matter increased only about three times, but the Nitrogen nearly 4½ times.

The actual gain of Nitrogen in the total products of the four experiments, was 1·1803 gramme. The whole admitted in the form of atmospheric ammonia was estimated at 0·0017 gramme, or less than 2 milligrammes. M. VILLE remarks, moreover, that an examination of the distilled water before being used to water the plants, and of the water afterwards removed from the apparatus, showed more ammonia in the latter than in the former. The conclusion from this second series of experiments also was, therefore, that a considerable quantity of free or uncombined Nitrogen had been assimilated.

4. M. G. VILLE'S *experiments in* 1851 *and* 1852, *in which the plants were supplied with a current of air washed free from ammonia.*

In 1851, one pot of Sun-flower, from seed, and two pots of Tobacco, starting from plants transplanted from good soil, were grown together, under circumstances similar to those of the preceding experiments, with the exception that now the air was deprived of its ammonia by passing over pumice-stone saturated with sulphuric acid, and also through a solution of bicarbonate of soda, previous to entering the apparatus.

The Sun-flowers gave 95 rudimentary grains; but the Tobaccos did not flower. However, taking the three experiments together, the dry matter increased nearly 200-fold, and the Nitrogen increased nearly 40-fold, during a period of growth of three months. The total gain of Nitrogen in the apparatus was 0·481 gramme.

In 1852, the conditions of the apparatus were the same as in 1851. The selection of plants was as follows:—One pot of Autumn Colzas, starting with young plants; one of Spring Wheat, from seed; one of Sun-flower, from seed; and two of Summer Colzas, from plants.

In every case the dry matter of the produce was many times that of the young plants or seed. In the case of the Sun-flower, it was more than 100 times that of the seed. In each experiment, there was of Nitrogen, also, much more at the conclusion, than at the commencement. In the experiment with Autumn Colzas there were 4·7 times, in that with Spring Wheat 2·2 times, in that with Sun-flower 25·5 times, in one with Summer Colza 3·4 times, and in the other with Summer Colza 6·7 times as much Nitrogen in the total products as in the original plants or seeds. The total amount of Nitrogen gained in the five experiments, was 1·624 gramme, which was 5·3 times as much as was contained in the total original plants and seeds.

To show the degree of luxuriance of growth of the different descriptions of plant, it may be mentioned that the Winter Colzas flowered, but gave no seed; the Sun-flower gave 412 rudimentary grains; and the Wheat seeded completely, giving 47 grains. The Summer Colzas had little tendency to go to seed, but developed a great deal of leaf; and hence it was, it was supposed, that they gained large actual amounts of Nitrogen.

5. M. G. VILLE'S *experiments in which known quantities of Ammonia were admitted into the atmosphere of the enclosing apparatus.*

In each of the three seasons 1850, 1851, and 1852, M. G. VILLE had a duplicate apparatus, enclosing, in each case, similar plants to those in the other, but with this difference in the conditions—that ammonia was supplied to the atmosphere of the second apparatus. As might be expected, the increase, both in dry substance, and in Nitrogen, was much the greater, in relation to the amounts of them contained in the seed or young plants, when ammonia was thus employed. In no case, however, did the plants take up Nitrogen equal in amount, much less exceeding, the whole of that supplied to the air in the combined form, as ammonia. The results have not, therefore, so direct a bearing on the question of the assimilation of free or *un*combined Nitrogen, as to require that we should quote them in any detail. Their chief interest was in showing the influence of ammoniacal supply, not only upon the vigour and luxuriance of growth generally, but upon the order, or course of development, of the plants, according to the stage of growth at which the application was made.

6. *Comparison of* M. G. VILLE'S *results with those of* M. BOUSSINGAULT *up to* 1853 *inclusive.*

It will be remembered that, up to 1853 inclusive, M. BOUSSINGAULT'S experimental plants had been grown either in *free air*—in which case they had fixed, from some source, slightly larger amounts of Nitrogen than were contained in the seed,—or in fixed and *limited volumes* of air (carbonic acid being added), in which cases no gain of Nitrogen was observed. The gain of Nitrogen in the free air was, moreover, considered to be too small to indicate, under all the circumstances, any assimilation of free or uncombined Nitrogen. On the other hand, M. G. VILLE'S experiments up to the same period had indicated an enormous gain of Nitrogen. The Nitrogen in the products, indeed, sometimes amounted to more than forty times that contained in the seed.

Results so strikingly contradictory could hardly fail to excite great attention and interest among Chemists and Vegetable Physiologists. But M. VILLE'S plants had been grown in a *constant current of renewed air*, and not in only a *fixed and limited volume* of it. This fact, and some other points, were alleged to account for the difference in result. At any rate, on the one hand, M. BOUSSINGAULT commenced in 1854, to experiment with a *current of air*; whilst, on the other, a Commission, composed of Members of the Academy of Sciences of France, was appointed to superintend the conduct of a new set of experiments by M. G. VILLE. It has already been shown, that M. BOUSSINGAULT'S new experiments in which a *current of air* was employed, did not indicate any assimilation of free or uncombined Nitrogen, any more than did those in which the plants had grown in limited volumes of air only.

7. M. G. VILLE'S *experiments conducted under the superintendence of a Commission comprising* MM. DUMAS, REGNAULT, PAYEN, DECAISNE, PELIGOT, *and* CHEVREUL.

These experiments were conducted at the Muséum d'Histoire Naturelle, Jardin des Plantes, Paris, in the autumn of 1854. M. CLOEZ was appointed to assist M. VILLE; and M. CHEVREUL reported on behalf of the Commission, in 1855*.

In an apparatus similar to that employed in the experiments of M. VILLE which have been already described, three pots of Cress were placed. The soil consisted of ignited brick and sand, to which was added some of the ash of the plant. Carbonic acid was supplied artificially; and the plants were watered with distilled water. The Cress in one of the pots did not thrive well; and, in this case, analysis showed a loss of 2 milligrammes of Nitrogen. In one of the other cases, there was a gain of 0·0492 gramme of Nitrogen, upon 0·0038 gramme supplied in the seed; and in the other, there was a gain of 0·0071 gramme of Nitrogen, upon 0·0039 gramme contained in the seed.

At the suggestion of one of the members of the Commission, a smaller vessel was also attached to the aspirator, in which one pot sown with Cress was placed. The soil being duly watered with distilled water, the apparatus was then closed, and not opened (as the other frequently was) until the conclusion of the experiment. In this case also, there was a considerable gain of Nitrogen indicated, namely, 0·0287 gramme gain, upon 0·0063 gramme in the seed.

Unfortunately, an element of uncertainty attached to the evidence afforded by these experiments made under the superintendence of the Commission, which is very much to be regretted. A quantity of distilled water taken from the same bulk as that used for watering the experimental plants was saved for analysis. The examination of this water devolved on M. CLOEZ; who, unfortunately, was called away for some days, during the evaporation of the water with oxalic acid, with a view to the after-determination of any ammonia it might contain. M. PELIGOT determined the ammonia in the acid residue of the evaporation of this water, as well as in that of the water removed from the cases, after it had served in the experiments. The result was, that there was indicated such an excess of ammonia in the water before being used, over that in the residual water after removal from the larger case, as more than covered the increase in the Nitrogen of the plants over that in the seeds sown. M. CLOEZ found, however, that, in his absence, the evaporation of the water had been conducted by the side of ammoniacal emanations from other processes. But when new portions of the original water were evaporated with proper precautions, less ammonia was indicated in it than in the water at the close of the experiment; and then, also, a gain of Nitrogen by the plants in the larger apparatus was indicated.

At any rate, however, the result with the single pot, in the small apparatus, showed a considerable gain of Nitrogen, even supposing the first analysis of the supplied water to be correct.

* Compt. Rend. 1855.

From the result of the whole inquiry, the Commission announced the following conclusion:—

That the experiment made at the Muséum d'Histoire Naturelle by M. VILLE, *is consistent with the conclusions which he has drawn from his previous labours.*

8. M. G. VILLE'S *experiments in which the plants were exposed to free air, and Nitrates or Ammonia salts were employed as manure**.

In 1855 and 1856, M. G. VILLE conducted a series of experiments with the double object, of investigating the action of nitrates upon vegetation, and of still further examining into the capability of plants to assimilate the free or uncombined Nitrogen of the atmosphere. The whole of the experiments now in question were made in free air, the plants being only shaded from rain; that is to say, without any enclosing apparatus, or artificial current of air and supply of carbonic acid. The soils consisted of calcined sand; ashes of plants such as those to be grown were added; and distilled water was used for watering. Colza and Wheat were the plants experimented upon. Lastly, the special conditions of the experiments were, that nitrate of potash in smaller or in larger quantity, or nitrate of potash and different ammonia salts, in equivalent quantities so far as their Nitrogen was concerned, were employed.

To the prosecution of this series of experiments, an exact method of estimating minute quantities of nitric acid was essential. M. VILLE succeeded in devising such a method, which was very favourably reported upon by M. PELOUZE, on behalf of a Commission composed of MM. BALARD, PELIGOT, and PELOUZE.

In 1855, two pots, and in 1856 one pot, of Colzas were grown, to each of which 0·5 gramme of nitrate of potash was supplied as manure. By examination of the soil, the point was ascertained when the whole of the nitrate had been drawn from it by the plants. The experiment was then stopped; and analysis showed that the total produce contained almost identically the amount of Nitrogen supplied in the seed and in the nitrate. The dry vegetable substance was, however, increased about 200-fold.

Again in 1855, two pots of Colzas were sown, to each of which 1 gramme instead of 0·5 gramme of nitrate was added; and in 1856 two more, with the same quantity. In each of these cases, the produce (which in dry matter was several hundred times that of the seed) contained considerably more nitrogen than had been supplied in the seed and in the nitrate. M. G. VILLE'S conclusions were, that the plants had taken up the nitrate and assimilated its Nitrogen, and that when by the larger supply of nitrate the growth had been extended, the free Nitrogen of the atmosphere was also assimilated.

In 1855 an experiment was made with Wheat manured with 1·72 gramme of nitrate of potash. The plants were allowed to mature, and they gave 84 grains. There was more Nitrogen in the vegetable produce alone, than in the seed and nitrate, and very much more in the total products, taking into account the residual Nitrogen in the soil. In 1856, two pots of Wheat were sown, to each of which 1·765 gramme of nitrate were added. The plants of one pot were taken up at the time of flowering, and they contained

* Recherches Expérimentales sur la Végétation, 1857.

almost identically the same amount of Nitrogen as was provided in the seed and nitrate. Those in the other pot were allowed to go to seed, and 119 grains were formed. In this case, again, the Nitrogen in the produce was much more than had been supplied, and very much more when the residual Nitrogen in the soil and pot was taken into the calculation. Lastly, on this head, two pots of Wheat (also in 1856) were sown without nitrate, and two with 0·792 gramme of nitrate to each. There was a considerable gain of Nitrogen in each of the four cases. The actual amount of gain was greater in the cases where the nitrate was employed; but the proportion gained, to that supplied, was greater where no nitrate was used.

To show the comparative efficacy of Nitrogen supplied in different conditions of combination, the following experiments were made during the season of 1856.

Two pots of Colzas received, each 0·5 gramme of nitrate of potash; and two other pots of Colzas received each an amount of sal-ammoniac equivalent in Nitrogen to the 0·5 gramme of nitrate. The two experiments with Nitrate gave equal amounts of Nitrogen in the produce; and the two with sal-ammoniac, also equal amounts. But the two with nitrate gave more than 1½ time as much Nitrogen in the produce, as the two with sal-ammoniac. In two other experiments, double the quantity of nitrate and sal-ammoniac, respectively, was employed, and the growth was allowed to extend over a longer period. The amount of Nitrogen in the produce was, in both these cases, very much greater in proportion to the amount supplied, than in the preceding experiments where the smaller amounts of nitrate and sal-ammoniac were used. It was, moreover, more than three times as much where the nitrate, as where the sal-ammoniac was employed. There was, too, where the nitrate was used, a considerable amount of Nitrogen assimilated beyond that provided, in the combined form, in the seed and manure.

Experiments similar to the above were made with Wheat. Two pots, to each of which nitrate of potash was added, containing 0·110 gramme of Nitrogen, yielded, respectively, in produce, 0·218 and 0·224 gramme of Nitrogen. Two pots of Wheat, each manured with sal-ammoniac, containing also 0·110 gramme of Nitrogen, gave, respectively, in the produce, 0·161 and 0·124 gramme of Nitrogen. In the same way, nitrate of ammonia containing 0·110 gramme of Nitrogen gave 0·118 and 0·149 gramme, and phosphate of ammonia 0·116 and 0·150 gramme of Nitrogen in the matured Wheat plants.

In regard to the experiments of M. VILLE referred to in this Division (8), he remarks, that the point at which the artificially supplied Nitrogen becomes exhausted is indicated by a lightening of the colour of the leaves, and that it is then that the plants begin to assimilate the uncombined Nitrogen of the atmosphere. To secure this assimilation, he considers that it is not only necessary that the supply of combined Nitrogen, and the vigour of growth, should reach beyond a certain limit, but that the artificial supply itself should, on the other hand, not exceed a certain limit. Further, the gain of Nitrogen in the experiments conducted on the plan now under consideration was so great, that, bearing in mind previously obtained results wherein the limit of the effect

of atmospheric ammonia had been ascertained, the influence of that source may, in the case of these new results, be entirely overlooked.

The fact that a given amount of Nitrogen in the form of combination of a nitrate was more efficacious than the same amount supplied in either of the ammoniacal salts experimented upon, was held to show that the nitrate was taken up by the plants as such, and was not previously transformed into ammonia.

M. Ville's experiments, as a whole, thus indicated that plants can take up Nitrogen in three forms—namely, as nitric acid, as ammonia, and as free Nitrogen. He enumerates the following conclusions:—

1. By means of nitre we may prove, without the aid of an enclosing apparatus, that plants absorb and assimilate the gaseous Nitrogen of the atmosphere.
2. Nitre acts by its Nitrogen. It is absorbed in the state of nitre.
3. In relation to the amount of Nitrogen, nitre is more active than ammonia-salts.

9. M. G. Ville's *collateral experiments to control or explain his results**.

M. Ville adduces evidence of yet another kind, in support of his view that plants assimilate the free Nitrogen of the air. Air was passed through an otherwise closed apparatus, in which was placed a vessel containing calcined sand, or calcined sand and decomposing organic matter. In no case was nitric acid formed. Nitrification, the result of the combination of the oxygen and nitrogen of the air within the porous soil, was not, therefore, the source of the Nitrogen gained by his experimental plants.

Experiments were made in which a given amount of organic matter (Lupins, Gelatine, &c.) was mixed with calcined sand, and exposed in an apparatus to a current of air, which carried the gaseous products into acid, to retain any ammonia that might be formed. The determination of the Nitrogen remaining in the matrix, and of the ammonia given off and absorbed by the acid, showed a loss of Nitrogen, which could only have passed away in the free gaseous form.

Other vessels of sand were prepared, to which similar known amounts of organic matter were added, and then seeds of Wheat were sown, the organic matter serving as manure. When the growth was stopped at a certain stage, almost exactly the same amount of Nitrogen was found in the Wheat plants and in the sand, &c., as was originally contained in the seeds sown and in the organic matter added. Assuming that the decomposition of the organic matter had taken the same course as in the other experiments—free Nitrogen being given off—it was obvious that a corresponding amount of free Nitrogen had been taken up by the plants. In other cases the growth of the Wheat was allowed to continue longer than in the experiments just alluded to; and then the total Nitrogen in the products not only equalled, but considerably exceeded, that in the seed sown and in the organic manure. In this instance, at least, it could not be said that the Nitrogen not received by the plant as ammonia had been taken up by it as nascent Nitrogen evolved in the decomposition.

* Recherches Expérimentales sur la Végétation, 1857.

The general conclusions from this part of the inquiry were as follow:—

1. Organic matters in decomposition lose a part of their Nitrogen as ammonia, and a part as Nitrogen gas.

2. Vegetation does not interfere with the progress of this decomposition.

3. Plants cultivated in a manured soil, give more Nitrogen in their produce than the manure yields as ammonia.

4. The excess of Nitrogen in the produce has been absorbed as free gaseous Nitrogen.

In regard to the explanation of the assimilation of free Nitrogen by plants, M. Ville calls attention to the fact, that nascent hydrogen is said to give ammonia, and nascent oxygen nitric acid, with free Nitrogen; and he asks—Why should not the Nitrogen in the juices of the plant combine with the nascent carbon and oxygen in the leaves? He further refers to the supposition of M. de Luca, that the Nitrogen of the air combines with the nascent oxygen given off from the leaves of plants, and forms nitric acid. Again, the juice of some plants (mushrooms) has been observed to ozonize the oxygen of the air; is it not probable, then, that the Nitrogen dissolved in the juices will submit to the action of the ozonized oxygen with which it is mixed, when we bear in mind that the juices contain alkalies, and penetrate tissues the porosity of which exceeds that of spongy platinum, a body so apt to favour combinations?

Summary Statement of the results and conclusions of M. Boussingault *and* M. G. Ville.

M. Boussingault, when, in his earlier investigations, he grew plants in free air, found only such indications of a gain of Nitrogen as, in his opinion, may be attributed to inaccuracies in the methods of experimenting and analysis at the early date, and to the combined influences of ammonia and organic corpuscles in the atmosphere; and when, more recently, he grew plants only shaded in such a manner as still to allow the free access of air, the gain of Nitrogen observed was not more than he considered might be due to the influences last mentioned. When he grew plants, either in confined and limited volumes of air, or in a current of air washed free from ammonia and organic corpuscles, the results did not show any appreciable gain of Nitrogen. Lastly, when he supplied either decomposing organic matter, or nitrate, to increase the activity of growth, he did not find such an amount of combined Nitrogen in his products, as to lead him to conclude that there had been any assimilation by the plants of free or uncombined Nitrogen. In these cases, indeed, he generally found a loss of combined Nitrogen during the experiment, supposed to be due to the evolution of free Nitrogen in the decomposition of the matters used as manure.

The results of M. G. Ville, on the other hand, showed a very considerable gain of Nitrogen during growth, whether the plants were subjected to a current of unwashed air, or of ammonia-free air,—and also when the plants were grown in free air, and their activity of development increased by the use of nitrates, or other nitrogenous matters, as manure. This gain of Nitrogen he considers to be due to the assimilation of free or uncombined Nitrogen. It is remarkable, too, that the proportion of Nitrogen gained, to

that supplied in the combined form, was observed to be the largest in some of the cases where no nitrogenous manure was employed, and where the total amount of combined Nitrogen within the reach of the plants was confined to a few milligrammes only, contained in the original young plants or seeds that were planted. In some such instances, the amount of combined Nitrogen found in the products was about forty times as much as was supplied. In other cases, the assimilation of free nitrogen only seemed to take place when the activity, and stage of growth, of the plants, had been forced beyond a certain point by the use of considerable amounts of nitrogenous manure.

Results and conclusions so astonishingly conflicting as those of M. Boussingault and M. G. Ville, have naturally incited others, either to investigate anew, or to seek, in the conditions provided in their experiments, for some explanation of the discordance. Before entering upon the consideration of our own experiments bearing upon the points in question, it will be desirable to add to the foregoing review a brief notice of the labours, or opinions, of these other experimenters or arbitrators.

C.—M. Mène's Experiments*.

In 1851, M. Mène made some experiments in reference to the assimilation of Nitrogen by plants. He seems to assume that Boussingault had concluded from his experiments that the free Nitrogen of the atmosphere was appropriated by plants; and he refers to the experiments of M. G. Ville as confirmatory of such a view. M. Mène made three sets of experiments in reference to this question.

1. He grew Wheat and Peas, respectively, in powdered glass as soil, allowing them contact with common air, and watering them with pure water. The Wheat increased in Nitrogen in amount equal to one-fourth of that contained in the seed sown; whilst its carbon, hydrogen, and oxygen were double those of the seed. The Pea-plants doubled the carbon, oxygen, and hydrogen of the seed sown, and their Nitrogen was threefold that of the seed.

2. Lentils, Peas, Haricots, Beans, Wheat, Rye, and Oats were grown in a sterile matrix under a bell-glass. They were respectively supplied with an atmosphere of known composition, and with acetate of ammonia in the soil. The plants increased in Nitrogen, and the ammonia in the soil diminished; but the free Nitrogen of the air was not perceptibly affected.

3. This series of experiments was in every way similar to the second, with the exception that the Nitrogen of the air was replaced by hydrogen. The plants flourished, and took up some of the acetate of ammonia.

M. Mène concludes that plants do not appropriate the free Nitrogen of the air.

D.—M. Roy's Views on the subject of the Assimilation of Nitrogen by Plants†.

M. Roy gave a paper on this subject in 1854. His supposition was that carbonate of ammonia constituted the chief source of Nitrogen to plants. Leguminous plants, he

* Compt. Rend. xxxii. † Ibid. xxxix.

considered, appropriated carbonate of ammonia from the atmosphere by their leaves. Graminaceous crops, on the other hand, he supposed, only took it up in solution by their spongioles. He further supposed that the free Nitrogen of the air was not appropriated by the leaves of plants, but that Nitrogen dissolved in water, and so taken up, by their roots, could be assimilated. He concluded that, in the case of M. Boussingault's plants grown in limited air, there would be but little passage of solution of Nitrogen by their roots, and evaporation of water from their leaves, and that, hence, the necessary conditions did not exist for the assimilation of free Nitrogen. M. Ville's rapid current of air would, on the other hand, cause a considerable amount of solution of Nitrogen to be drawn into the plants.

E.—The Experiments of MM. Cloez and Gratiolet.

In 1850, MM. Cloez and Gratiolet published the results of some experiments made with Water-plants. They found that, carbonic acid and air being both present, the plants gave off oxygen slowly, or very rapidly, according to the condition of the sunlight and the temperature. In water deprived of common air, but containing carbonic acid, the evolution of oxygen rapidly declined, Nitrogen was given off, and the plant contained less Nitrogen than a similar plant in water not deprived of its air. The evolution of Nitrogen diminished as the experiment proceeded. They considered that, in the vegetation of Water-plants, Nitrogen is given off from their nitrogenous constituents and that there must be restoration either from free or combined Nitrogen. And as their experiments showed that ammonia-salts were injurious to the plants, they concluded that they take up free Nitrogen dissolved in water.

In 1855* M. Cloez published the results of some experimental inquiries on nitrification, with a view to the question of the source of the Nitrogen of plants. He made twenty experiments, passing washed air through as many different combinations of porous, earthy, and alkaline matters. The experiments continued from September 1854 to April 1855, when, in some cases, efflorescence was observed. He found nitrates to be formed in notable quantity in calcined brick, or pumice, impregnated with alkaline or earthy carbonates; also, in uncalcined brick similarly impregnated. In chalk, marl, a mixture of kaolin and precipitated carbonate of lime, &c., only traces of nitrate were formed.

M. Cloez concluded that nitrates would be formed when a current of air was passed over porous bodies, alkalies being present. He considered, therefore, that the porosity of the pots and brick fragments, the alkalinity of the ashes, the moisture, and the current of air, in M. Ville's experiments with plants, provided the conditions for the formation of nitric acid. He asks, can such formation take place in limited air?

F.—The Experiments of M. de Luca†.

M. de Luca found that, on passing moist ozonous air over potash and potassium, nitrate of potash was formed. He further found that the oxygen given off by plants

* Compt. Rend. xli. † Ibid. 1856.

in sunlight was in many cases ozonous. He aspirated a large quantity of air, in the neighbourhood of vegetation, through carded cotton, and then through sulphuric acid, to wash it. The washed air then passed over potassium, and through a dilute solution of pure potash, when nitrate of potash was formed. When, on the other hand, air in the midst of habitations was operated upon in a similar way, the formation of nitric acid was not observed. M. DE LUCA supposes the air surrounding vegetation, in sunlight, to be ozonous; that by its means the Nitrogen of the air may be converted into nitric acid; and that thus the Nitrogen of the air may be rendered available for assimilation by plants, under the influence of vegetation itself.

G.—THE EXPERIMENTS OF M. HARTING*.

In 1855, M. HARTING published some criticisms, and the results of some experiments, on the question of the assimilation of Nitrogen by plants. He considered that the Nitrogen of the air might contribute indirectly to vegetation. He attributed a formation of ammonia from the decomposing débris of seeds, &c., and the free Nitrogen of the air, in the case of M. VILLE'S experiments; and also supposed that nitric acid might be formed by the oxidation of the atmospheric Nitrogen. The increase of Nitrogen in M. VILLE'S plants, and of ammonia in the water of the enclosing apparatus, was taken as proof of such formation of ammonia.

M. HARTING made two sets of experiments, in one of which the plants grew in a limited volume of air, and in the other in a current of air washed free from ammonia—both arranged with a view to avoid the formation of ammonia. He employed enclosing-apparatus somewhat on the plan of M. BOUSSINGAULT and M. VILLE; but he used glass vases, instead of porous pots, for his plants. He grew Beans, Buckwheat, and Oats. After the seeds had germinated, and the plants had protruded a little above the surface of the artificial soil, he covered the latter with a mixture of wax and oil, to shut off the access of air. He further enclosed the stems of the plants in caoutchouc tubes; and inserted other caoutchouc tubes through the waxy coating, for the supply of water. Some of the plants were very vivacious at first; and in the case of the Beans, two began to flower; but then the leaves turned yellow, and the experiment was stopped. His apparatus consisted of tinned-iron pans, varnished, and surmounted by glass shades of 18 litres capacity. There was an aperture for the admission of carbonic acid, another for that of water, and so on.

The result was that the produced plants yielded no more dry matter than was contained in the seeds. M. HARTING considered, therefore, that the determination of the Nitrogen was superfluous. The growth evidently stopped when the supplies of the seeds were exhausted. M. HARTING'S general conclusions on the subject were as follow:—

1. Plants absorb salts of ammonia, and nitrates, by their roots.

2. The Nitrogen of the air contributes to the formation of ammonia, and nitrates, in the soil.

3. It is not proved that Nitrogen serves directly for the nutrition of plants.

* Compt. Rend. xli. 1855.

H.—M. A. Petzholdt on the Source of the Nitrogen of Plants*.

In the years 1852 and 1853, M. H. M. Chlebodarow made some experiments on the subject of the assimilation of Nitrogen by plants, at Dorpat, under the direction of M. Petzholdt, who has reported the results of the inquiry.

M. Petzholdt assumes that if plants can appropriate the free Nitrogen of the air, they will not need ammonia; and that if they take Nitrogen from ammonia, the artificial supply of the latter will increase growth.

The experiments were made upon Barley. In 1852, an ignited yellow sand was taken as the soil. To one set of plants, no ammonia was supplied; to a second, carbonate of ammonia was provided in the soil; and to a third, carbonate of ammonia was supplied in the air. Both the crops with an artificial supply of ammonia gave three times as much produce as the crops without such supply. The Nitrogen in the produce was also very much greater, both in percentage, and in actual amount, where the ammonia was used.

In 1853, six sets of experiments were made, and as before, with Barley. The soils consisted of an artificial mixture of clay, sand, and felspar, decomposed by heating with lime. The first set of three pots was provided with this soil alone; the second had, in addition, 0·13 per cent. of bone-ash acted upon by sulphuric acid; and the third had 1·33 per cent., or ten times as much, of the same phosphatic manure. The three other sets were, respectively, so far like the three just described, but in addition ammonia was artificially supplied to the atmosphere in which the plants grew. The phosphatic manure, whether with or without the ammoniacal supply, much increased the produce of both corn and straw. The Nitrogen of the crops was also very much increased in actual amount (though diminished in percentage in the dry substance) by the aid of the phosphatic manure; and the actual amount of Nitrogen was still further increased by the addition of ammonia to the atmosphere of the plants; and the percentage of Nitrogen in the dry substance was also greater where the ammonia was supplied, than in the corresponding cases without it. The experiments without ammonia were made in free air. The Nitrogen in the produce was about seven times that of the seeds where no phosphates were employed; about twelve times that of the seed witn the smaller quantity of phosphate; and about twenty times that of the seed with the larger amount of phosphate.

M. Petzholdt considered it difficult to account for the fact of M. Boussingault getting little or no increase of Nitrogen when he grew plants in free air, which must have supplied some ammonia, even though rain and dew were excluded. He thinks the error must be on the side of M. Boussingault.

It is seen that the explanations or conclusions of these several arbitrators are nearly as conflicting as those of M. Boussingault and M. G. Ville themselves.

For ourselves we are free to confess that we are unable to discover, either in the

* Journ. für Prakt. Chem. Band lxv.

differences of plan adopted by M. Boussingault and M. G. Ville, so far as they have themselves described them, or in the results and explanations of other experimenters, any satisfactory solution of the difference of result arrived at. *À priori*, there are reasons for concluding, both from the chemical characters of Nitrogen itself, and from what we at present know of the chemistry of vegetation in other respects, that plants would not assimilate Nitrogen offered to them in the free state. On the other hand—to say nothing of the large total amount of combined Nitrogen actually existing—the statistics of Nitrogen-production show that there is an amount of Nitrogen periodically available for the vegetation of a given area of land, the source of a considerable proportion of which is as yet not satisfactorily explained. The question whether or not the assimilation of free Nitrogen by plants may account for all, or a part, of the otherwise unexplained fixation, is seen to be left in a dilemma almost inexplicable, by the conflicting character of the results that have been recorded relating to it. Yet, as has been already said, upon the decision finally come to in regard to this question, must materially depend the degree of importance to be attached to the investigation of the other actual or possible sources of Nitrogen to plants, which we have briefly noticed. Under these circumstances, it seemed desirable that any opinions we might offer or adopt on this subject should have the support of such evidence as might be afforded by renewed experiment. We proceed, then, to follow up our account of the Nitrogen-statistics of vegetable production, the consideration of the several possible sources of Nitrogen to plants, and the review of the results and opinions of others on some of the points at issue, by a statement of our own experimental evidence in regard to this important question.

PART SECOND.

EXPERIMENTAL RESULTS OBTAINED AT ROTHAMSTED DURING THE YEARS 1857, 1858, AND 1859.

Introductory observations.

In laying this part of the subject before the Fellows of the Royal Society, we shall follow the general order in which the questions involved were presented to ourselves in the investigation. In so doing it will be necessary:—

1. To consider all possible conditions to be fulfilled in order to effect the solution of the main question of the assimilation of free Nitrogen by plants, and to endeavour to eliminate all sources of error in our investigation.

2. To examine a number of collateral questions, which have a bearing upon the points at issue, and to endeavour so far to solve them as to reduce the general solution to that of a single question to be answered by a final set of experiments.

3. To give the results of the final experiments themselves, and to discuss their bearings upon the question which it is proposed to solve by them.

We shall dwell more fully upon the conditions involved in the experiments than upon the numerical results which they have afforded, since the value of these results is so wholly dependent on those conditions, that, if the latter are properly arranged and thoroughly considered, any conclusion with regard to the former will be sufficiently apparent from the numerical results themselves.

In studying the conditions, we shall be obliged to touch upon several collateral points, embracing some questions not necessarily involved in the investigation, and which, therefore, we have not attempted to treat with that fulness which, as distinct questions in vegetable Physiology, they merit. Yet, we think, it will appear that, in the degree in which we have followed them, their discussion is essential to complete the consideration of the main question of the investigation, and that, in relation to it, they possess an interest quite commensurate with the attention we have devoted to them.

These questions are embraced in the following:—

1. The preparation of the soil or matrix for the reception of the plant, and of the nutriment to be supplied to it.

2. The preparation of the nutriment to be supplied to the plant,—embracing that of mineral constituents (as in the ash), of certain solutions, and of water.

3. The conditions of the atmosphere to be supplied to the plant, together with the means of securing them,—involving a consideration of the circumstances affecting the composition of the atmosphere, and of the apparatus used to supply it.

4. The changes undergone by nitrogenous organic matter during its decomposition, affecting the quantity of combined Nitrogen present, in circumstances more or less analogous to those in which the plants were grown in our experiments upon the assimilation of Nitrogen.

5. The action of agents, as ozone, and the influence of other circumstances which may affect the quantity of combined Nitrogen present in connexion with the plant, and yet independent of the direct action of the vital (growing) process.

In considering these five questions, two important series of conditions must be fulfilled:—

1. Those that relate to the growth of the plant,—which must be so arranged as to include all that is necessary for healthy and vigorous growth, *excepting only*, in some instances, such conditions as may depend upon the presence of a supply of combined Nitrogen.

2. Those that relate to our means of measuring the quantity of combined Nitrogen present at different periods of growth,—it being essential that we should be able, with the means of investigation afforded in the present state of science, to ascertain the quantity of combined Nitrogen present with the plant at different periods of its growth, with sufficient exactness to detect any changes that may take place, so as to enable us to refer them to their proper source.

If we succeed in fulfilling all these conditions, we shall have at our command all the data requisite for the solution of the question whether plants assimilate free

Nitrogen. Our preliminary investigations will have enabled us to avoid, or to eliminate, all sources of error due to the incidental circumstances of the research; and the numerical results of a final series of experiments, showing the quantities of combined Nitrogen supplied, and those eventually found in connexion with the plant, will afford the necessary data for the solution desired.

In discussing the conditions involved in the experiments, and the researches undertaken to enable us to estimate the value of those conditions, we shall arrange the subject in such order as will most clearly bring out their bearings upon the main question, rather than according to the order as to time in which they were made. Several collateral experiments were made, to prove that our conditions of growth, provided in soil, atmosphere, and nutriment, were such as we had assumed them to be; for had they not been so, the object of the investigation could not be attained. The time required for the conduct of these collateral experiments, made it necessary that many of them should be performed simultaneously with the investigations the proper conditions of which they were designed to make known.

We shall first consider the arrangement of the main experiments, and the plan and results of the collateral inquiries with a view to show what the conditions of the former should be, and then show how far the conditions assumed for the first year's experiments, and those arranged in the second year, after the results of some of the collateral investigations were known, agree with the conditions indicated by the results of all the collateral investigations taken together.

Section I.—CONDITIONS REQUIRED, AND PLAN ADOPTED, IN EXPERIMENTS ON THE QUESTION OF THE ASSIMILATION OF FREE NITROGEN BY PLANTS.

A.—*Preparation of the Soil, or matrix, for the reception of the plant, and of the nutriment to be supplied to it.*

In considering the subject of the soil to be used, the remarks made above on the necessity of combining the conditions of healthy growth with the simplicity of constitution which would allow of a quantitative estimation of the results obtained, acquire a high degree of importance.

So complicated is the constitution of ordinary soils, and so intimately are the nitrogenous compounds existing within them associated with the other matters, that it is impossible either to estimate the Nitrogen with sufficient accuracy for our present purpose, or to extract it from the soil without entirely destroying the other conditions of vegetable growth. We are, moreover, so entirely ignorant of the character of the organic constituents of soils, of the state in which the principal part of the Nitrogen exists in them, of the changes to which it is subject during vegetable growth and decay, and, more especially, of its relations to vegetable growth, that an ordinary soil could not possibly be used for our purpose.

Our ignorance of the actual constitution of soils, as regards the state of the organic

matter in them, and its relations to the inorganic substances, entirely precludes the possibility of our imitating, by artificial means, a natural soil, so as to include all its conditions excepting a supply of combined Nitrogen.

It is evident, therefore, that if all the conditions embraced in an ordinary soil were essential to vegetable growth, the solution of the question of the assimilation of free Nitrogen by plants would involve difficulties which our means of investigation in the present state of science could not overcome. But the experiments to which attention has been directed in the history of this subject, as well as others, the details of which we shall give further on, show that such is not the case. They show that many of the complicated conditions of an ordinary soil may be entirely dispensed with, so as to bring the examination of it within our means of investigation, and yet to retain all the conditions of healthy growth.

In the experiments of the first year, 1857, two kinds of soil, or matrix, were used.

One was prepared from an ordinary soil, so as more nearly to imitate the usual conditions of vegetable growth. The other was prepared from volcanic pumice, with the view to eliminate certain supposed sources of error which the prepared soil might introduce. It was found, however, in the experiments of 1857, that there was no necessity for this difference of matrix, and hence, in the experiments of 1858, only prepared soil was used.

The soil selected for the preparation of the matrix was a somewhat heavy one (clayey), resting upon chalk, and interspersed with flints. The large stones were removed by picking and sifting; and the clayey lumps were powdered to prevent them from baking into hard nodules during ignition. An attempt to ignite in ordinary clay crucibles was not successful, owing to the reduction of the peroxide of iron to the state of black oxide, and to the formation of sulphides from the reduction of the sulphates present, as indicated by the vapours of sulphurous acid emitted during the ignition, and by the evolution of sulphide of hydrogen on the addition of an acid to the mass after cooling.

The combustion proceeded satisfactorily in a large cast-iron muffle, through which a constant current of air could pass. The ignition was continued until a portion of the soil assumed, on cooling, the red colour due to peroxide of iron, and exhibited no trace of coaly matter. The mass thus prepared was taken from the muffle, and thrown into a large vessel filled with distilled water. The water was rendered highly alkaline by the quantity of caustic lime present. The fluid was decanted, and fresh portions of water added several times during eight or ten days, until all the soluble matter was removed. The residue was then dried, and retained for final ignition before being used. The ferruginous and aluminous character of this soil-matrix pointed to the danger there might be of its acting as a porous body, to promote the formation of nitrogenous compounds independently of vegetable growth, on the one hand, or to absorb and retain the ammonia given to the plant, or that which might be formed from the nitrogenous matter of the seed, on the other.

To ascertain the value of any influence exerted by the soil independently of the plant,

in the manner just indicated, a pot of soil, prepared exactly as for an experiment with a plant, was submitted to the same conditions of air, temperature, moisture, &c., as the pots containing the experimental plants. The result was, that there was no accumulation of combined Nitrogen. The result with the matrix of pumice also showed, compared with that of the soil-matrix, that no error was to be feared from the influence of the latter in absorbing and retaining combined Nitrogen already in connexion with the plant.

For our purpose, pure volcanic pumice was used. It was powdered until the mass was quite fine and the largest pieces were about the size of peas. This powder was subjected to long washing in the same manner as the ignited soil. Lastly, it was dried ready for a final ignition before being used.

B.—*The Mineral Constituents added to the prepared Soil.*

In most cases the necessary Mineral Constituents were supplied in the form of the ash of the plant of the description to be grown. In a few instances, where this was not practicable, the ash of some other plant was selected. Weak solutions of sulphates and phosphates, as well as ash, were also sometimes used.

In some instances the ash was obtained by burning a quantity of the entire plant when in seed. In other cases, the seed and the rest of the plant being burnt separately, a mixture of the two ashes was made in such proportion as to represent the composition of the ash of the entire plant.

Thus, in the experiments of 1857, for Wheat a mixture of one part of the grain-ash and six parts of the straw-ash, for the Barley a mixture of one part of the grain-ash and three parts of the straw-ash, and for Beans a mixture of one part of the corn-ash and two parts of the straw-ash was used. In the experiments of 1858, the ash used for these crops was obtained by burning the entire plant. For Clover, the ash of Clover-hay was employed.

In some instances of Leguminous plants the ash was saturated with sulphuric acid, and then ignited, before being used.

Each ash was burnt in a large shallow platinum dish, heated in a current of air, in a cast-iron muffle. The burning was continued until all coaly matter had disappeared. The ash was then preserved, but was always submitted to a final ignition before being used. Examination failed to detect combined Nitrogen in any of the ashes so prepared.

In order that the roots of the plants should find an abundance of mineral matter at the most active period of growth, it was desirable that the matrix should contain as much of such matter as was consistent with healthy development. A consideration of the chemical constitution of soils suggested a proportion of 0·8 to 1·0 per cent. of ash; and this was the quantity added to the matrix for the experiments of 1857; but for those of 1858 only about half as much was employed.

C.—*The Distilled Water.*

The first two-fifths of the distillate from ordinary water was allowed to escape, and the next two-fifths were collected for further treatment. The water so obtained retained traces of ammonia. It was mixed with phosphoric acid, free from nitric acid and ammonia, in such quantity that the amount of acid present exceeded that of the ammonia several thousand times. It was then re-distilled from a copper vessel to which was attached a large Liebig's condenser.

Under these circumstances no ammonia could go over unless it were carried over mechanically, in which case it would be accompanied with several thousand times its own weight of phosphoric acid; and, as no distilled water was used that gave any evidence of the presence of this acid, the amount of ammonia in it, if any, must have been several thousand times less than that to which the term "traces" is applied.

The distilled water was so prepared only a few days prior to being required for use.

All parts of the apparatus, the presence of ammonia in which could possibly affect the result, were, after thorough washing both with ordinary and with common distilled water, finally well rinsed with this pure double-distilled water just before being used.

D.—*The Pots used to receive the Soil, Ash, Plant, &c.*

For the experiments of 1857 common flower-pots were used; their height, and diameter at the top, were each 6 inches, and their diameter at the bottom 3·2 inches; their weight was about 1 lb. Small common white glazed earthenware plates were used as the pans.

Subsequent observation suggested, for the experiments of 1858, the kind of pot, and pan beneath it, represented in Plate XII. figs. 1, 2 & 3.

The Pot, of which fig. 2, Plate XII., represents the elevation, was made of the same material as ordinary flower-pots. It was, however, made as light as possible, and was not baked so hard as the latter generally are. The height, and diameter at the top, were each 5 inches; and the diameter at the bottom was 4 inches. The bottom is perforated with about twenty holes of nearly one-fourth of an inch diameter, as is shown in figs. 1 & 2. There were also two rows of similar holes (A, B, fig. 2) round the sides at a distance of 0·5 to 1 inch from the bottom.

The Pan, represented with the pot placed in it in fig. 3, Plate XII., is made of hard-baked and well-glazed stone-ware. It is 1·5 inch deep and 5·2 inches in diameter at the bottom. At the top it is curved inwards (A, B, fig. 3), so as to adapt its upper rim to the sides of the pot.

These arrangements of pot and pan afford several advantages, for the purposes of the investigation, over those adopted in 1857. The surface for evaporation is less in proportion to the volume of soil. The facilities for the exit of roots, and for the access of air, are, on the other hand, greater. The pan affords room for an abundance of water,

in which the roots develope luxuriantly*. Yet this water does not evaporate so freely as it otherwise would do, owing to the inward curve of the top of the pan, which also serves to protect the roots distributed through the water from the direct action of sunlight. All the conditions of growth are thus attained with a minimum of evaporation from all sources excepting through the plant itself; and a drier atmosphere is maintained. Consequently evaporation through the plant is favoured, and hence the conditions are provided for a constant supply to the plant of all the mineral and gaseous substances in solution in the fluid of the soil and pan.

E.—*Final preparation of the Soil, Ash, and Pot, for the Plant.*

The soil and ash, each prepared as described in the foregoing subsections, and the pot, also as described above, were simultaneously heated to redness; and the soil and ash, whilst red-hot, were mixed together in the red-hot pot, which was placed upon a red-hot brick over sulphuric acid. The pot and contents were then covered with a large glass shade, and left to cool.

The soil, as in its former preparation, was heated in a cast-iron muffle, from which it was removed with a small iron shovel adapted to the purpose, and heated to redness before being used.

Four or five pots were heated together, one inside the other, the top and bottom ones of which almost invariably broke, either on the application of the heat, or on removal from the fire; so that only about half of those operated upon were finally available for use.

From 2½ lbs. to 3 lbs. of ignited soil were put into each pot; but in the experiments of the second year, 1858, the lower part of the pot was first filled, to the depth of about 1 inch, with very coarsely broken-up red-hot flint. In 1857, about 14 grammes of ash, and in 1858 about 7 grammes only, were used for each pot. The greater portion of the ash was mixed with the lower layers of the soil; but some was distributed through the whole of it.

After cooling down sufficiently, the shade was removed, and about 500 cub. centims. of distilled water, prepared as described in subsection C, were added to the soil of each pot, this being as much as it would absorb. Then, after a lapse of ten to twenty hours, the seeds or plants were put in.

F.—*The Seeds and Plants taken for experiment.*

In all the experiments recorded, the plants were grown directly from *seed* sown in the soils prepared as above described.

In every case, seed of the best quality was taken, which was kindly presented to us for our purpose by the Messrs. Thomas Gibbs and Co., of Half-Moon Street, Piccadilly,

* See Table, and general remarks at p. 524; also notes of root-development of Wheat No. 6 (1857), p. 553, Wheat No. 1 (1858), p. 560, and Wheat No. 9 (1858), p. 569.

Seedsmen to the Royal Agricultural Society of England, who bestowed much labour and attention upon the selection. From the quantity of each kind received, the largest and the smallest were picked out, as were also any that did not look quite healthy. Given numbers of the remainder were then weighed, and the average weight, per seed, was calculated. A few seeds, each weighing as nearly as possible the mean weight, were then selected for planting.

In order to estimate the quantity of Nitrogen in the seeds sown, in some cases a quantity of seeds equal in weight and number to those sown was submitted to analysis. But the difficulties of grinding, without loss, so small a quantity, and the consideration that one small quantity might differ more in composition from another such quantity, than either would from the average composition of a large number of well-selected seeds, led us generally to estimate the Nitrogen in the seeds sown from the percentage of it found in the mixture of a large number ground up together.

The seeds selected for growing were sown in the pots of soil prepared as already described, to the depth of about 1 inch below the surface. With large seeds, such as Beans, it was necessary that care should be taken so to deposit them that the radicle and plumule should each take its natural direction. If this precaution was neglected, the seed was liable to be raised out of the soil after sprouting, which involved the inconvenience of opening the apparatus in which the plant was enclosed, in order to re-bury the seed.

In some cases, as soon as the seeds were sown the pots were removed from over the sulphuric acid, and placed at once beneath the large glass shades which were to serve as the enclosing apparatus. In other instances, the pots were first placed under other shades, luted by mercury or sulphuric acid, and standing in the laboratory, and then, after a few days, they were removed to their final position.

G.—*The Atmosphere supplied to the Plants.*

As regards the essential conditions of growth, and the circumstances associated with it, which must be kept within the control of our means of investigation, the same remarks apply to the atmosphere, though with less force, as have already been made in reference to the soil (subsection A, p. 470).

It is true that the constitution of the atmosphere is less complicated, and that we are much better acquainted with it than we are with that of ordinary soils; yet the extreme mobility of the atmosphere renders the presence in it of exceedingly small quantities of substances calculated to influence vegetable growth much more dangerous in quantitative experiments on vegetation than would be their presence in the soil. Thus the presence of gaseous impurities, and of solids mechanically suspended, in the atmosphere cannot be overlooked. And hence, although it is not necessary to submit the natural atmosphere to such radical changes as those to which the natural soil must be subjected, some measures must be taken to exclude the sources of error to which allusion has just been made.

As in the case of the soil, so in that of the atmosphere, the only essential conditions to be attained are such as are required for healthy growth, and as will at the same time enable us to estimate the amount and the sources of the combined Nitrogen coming within the reach of the plant by its means.

In consequence of the mobility of the atmosphere above referred to, it was necessary to exclude the experimental plants from its free access. The quantity of ammonia in the air is, however, so very small, that, provided the atmosphere of the enclosing apparatus were allowed to remain unchanged throughout the period of an experiment, the amount of combined Nitrogen so coming within the reach of the plant might be altogether neglected. Nor, so far as regards the nitrogen and oxygen of the air, is there any necessity for change; but, owing to the peculiar circumstances of temperature and of moisture to which the air of the apparatus is subjected, conditions more closely allied to those of ordinary vegetation are attained by a frequent change of atmosphere. The large quantity of air which thus becomes involved in an experiment precluded the idea of neglecting the consideration of the combined Nitrogen which it contains. It devolved upon us, therefore, either to determine the total amount of combined Nitrogen in the air before and after it came in contact with the plant, or to free the air from combined Nitrogen before admission into the enclosing apparatus. The latter alternative was adopted as the most simple; and the manner in which the object was effected will appear from the following description of the apparatus employed.

H.—*The Apparatus used to enclose the Plants, and to supply them with Air, Water, Carbonic Acid, &c.*

Plate XIII. represents the entire apparatus as used for each separate experiment in 1857; and fig. 1, Plate XIV., that used, also for each separate experiment, in 1858, in which, as will be seen, several important modifications of the arrangement adopted in 1857 were made.

The same letters of reference apply to the two so far as the parts are alike; and where there has been any modification in the arrangement in 1858, as compared with that in 1857, the same letters represent the parts of the apparatus used for the same purpose in each, with the exception, that those which apply to the modification of the apparatus in 1858, are distinguished by a dash, thus $'$.

A, Plate XIII. (and fig. 1, Plate XIV.), represents a large stone-ware Woulfe's bottle, 18 inches in diameter and 24 inches high.

B, C, and E are glass Woulfe's bottles of 30 ounces capacity.

F is a large glass shade, the dimensions of which were, in most of the experiments, diameter 9 inches, and height 40 inches; in other cases the dimensions were, diameter 16 inches, and height 28 inches.

a represents the cross section of a leaden pipe $1\frac{1}{2}$ inch in diameter, which is in connexion with a reservoir of water, not shown. This pipe passes over all the vessels A (of

which there is one included in the apparatus for each separate experiment) in a direction at right angles to the plan of the figure. It is connected with each vessel A by means of a tube *a b*, in which is fixed a stopcock, to open and shut the connexion between the water-supply tube *a* and the vessel A.

c d e, Plate XIII., is a leaden exit-tube for air. *c' d' e'*, fig. 1, Plate XIV., is the corresponding tube in the apparatus of 1858, which is enlarged at the point *c'* and downwards until it opens into the vessel A, thus allowing another, *q' r' s'*, to pass through it and down to the bottom of the vessel A, as indicated by the dotted line. This tube *q' r' s'* is a half-inch safety-tube, opening externally at *q'*, and in the apparatus of 1858 replaces the tube *q r s* shown in Plate XIII.

The bottles B and C are filled to the depth of $2\frac{1}{2}$ inches with sulphuric acid of sp. gr. 1·85.

The tube D D is about 3 feet long and about 1 inch in diameter, and is filled with fragments of pumice saturated with sulphuric acid. At *f*, *f*, in this tube, are small indentations to prevent the sulphuric acid from draining against the corks.

The Woulfe's bottle E contains a saturated solution of ignited carbonate of soda.

g h is a bent and caoutchouc-jointed glass tube, connecting the interior of the Woulfe's bottle E with that of the large glass shade F.

i k, better indicated in fig. 2, Plate XIV., is the exit-tube for the air, connecting the interior of the shade F with an eight-bulbed apparatus M, containing sulphuric acid.

w w, Plate XIII., is a block of slate 12 inches square and $3\frac{1}{4}$ inches thick, in which is a circular groove, half an inch wide and 2 inches deep, adapted to the diameter (9 inches) of the glass shade F, the bottom of which rests in it. The groove is filled with quicksilver, which shuts off the communication of the external air with the interior of the shade. It is widened and deepened at four equidistant points, to admit of glass tubes passing underneath the shade. Two of these tubes, *g h* and *n o*, are shown in Plate XIII., and *g h* also in fig. 1, Plate XIV., *n o* being there replaced by *n' o'*. The other two are at right angles to these, and are best seen in the vertical section of the shade and lute, fig. 2, Plate XIV., lettered *u v* and *i k* respectively. The tube *u v* is for the supply of water to the plant; and the tube *i k* is for the exit of the air, from which it passes outwards through the sulphuric acid in the eight-bulb apparatus M. This vertical section of the shade and lute is at right angles to the view of them in fig. 1., Plate XIV.; and from it a judgment may be formed of that of the shade and lute of Plate XIII., as well as of the corresponding tubes to those last described, in the apparatus of 1857.

The tube *n o*, Plate XIII., passing from the outside, beneath the shade, and extending to the surface of the mercury in the groove within the shade, is for the purpose of withdrawing condensed water. In the apparatus for 1858, the arrangement for this object is rather different. Thus O, fig. 1 (and fig. 2), Plate XIV., is a bottle into which passes a tube *n' o'*, opening into the bottom of the lute *w' w'* by means of a hole at *n'*,

seen better in figs. 4 & 5, Plate XII., which represent, respectively, the plan and the vertical section of the glazed stone-ware lute-vessel used in 1858. Another glass tube (t') passes to the bottom of the vessel O (figs. 1 & 2, Plate XIV.), for the purpose of withdrawing the condensed water which collects in it.

The plan of the stone-ware lute-vessel used in 1858 (fig. 4, Plate XII.) shows the groove for the mercury, the four widened and deepened points of it for the passage of the tubes under the shade (of which however three only were used), and the hole n' at the bottom, for the reception of the tube for carrying off the condensed water. This lute-vessel is made of hard-baked and well-glazed stone-ware, and is, in fact, simply a shallow dish with double concentric sides, the space between which latter forms the groove for the reception of the shade and of the mercury luting, and for the passage of the tubes. Figure 5, Plate XII., is a vertical section of the stone-ware lute-vessel, from A to B, fig. 4, through two of the widened and deepened portions of the groove, and through the hole n'. Figure 6, Plate XII., is also a vertical section of the lute-vessel, but from C to D, fig. 4.

The Woulfe's bottle T, fig. 1, Plate XIII., and T', fig. 1, Plate XIV., is for the supply of carbonic acid, and will be referred to, more fully, in the following subsection I.

I.—*Use of the Apparatus.*

If the stopcock below a (fig. 1, Plate XIII., and fig. 1, Plate XIV.) is opened, water flows into the vessel A from a large reservoir with which the leaden tube a is in connexion. As the pressure increases, the water rises in the safety tube $q\,r\,s$, or $q'\,r'\,s'$, above the level in the vessel A, and at the same time the air begins to escape by the tube $c\,d\,e$, or $c'\,d'\,e'$, to force its way through the sulphuric acid in the bottles B, C, then to traverse the tube D D, containing the pumice saturated with sulphuric acid, to bubble through the solution of carbonate of soda in E, and finally to enter the shade F by the bent and jointed tubes g, h; and from the shade it passes out through the tube $i\,k$ and the bulb-apparatus M containing sulphuric acid, into the external air.

The minimum pressure required to produce this passage of air, expressed in the height of a column of mercury which it would sustain, is equal to the sum of the products obtained by multiplying the height of each fluid through which the air has to pass by the sp. gr. of the same, divided by the sp. gr. of mercury, or

$$[(2{\cdot}5+2{\cdot}5+1{\cdot}0)\times 1{\cdot}85+2{\cdot}5\times 1{\cdot}2]\tfrac{1}{13{\cdot}6}=1{\cdot}037 \text{ inch},$$

in which 1·2 is the sp. gr. of the carbonate-of-soda solution.

The difference between the height of the water in the vessel A and in the safety-tube $q\,r\,s$, or $q'\,r'\,s'$, must always be equal to the weight of the mercury column obtained in the manner just indicated, multiplied by the sp. gr. of mercury.

If the difference between the height of the highest points of the tubes $q\,r\,s$ and $c\,d\,e$, fig. 1, Plate XIII. (that of the former being the higher), be less than the minimum height

just referred to, then the water must flow out of A through the safety tube *q*, *r*, *s* before it can pass through the tube *c*, *d*, *e*, into the bottle B. In accordance with this consideration, the safety-tubes for the apparatus of 1857 were arranged as shown in Plate XIII. (*q*, *r*, *s*); but, owing to occasional leakage of the joints at the top of the vessel A, this principle could not be relied upon; and hence the arrangement shown in fig. 1, Plate XIV. was adopted in the experiments of 1858.

The height from the top of the vessel A to *t'* (fig. 1, Plate XIV.) was 12 inches, which is sufficient to allow the whole of the air to pass out of the vessel A, whilst the great height of *d'*, of the tube *c'*, *d'*, *e'*, entirely prevented the water from passing over into the bottle B,—an accident which unfortunately happened on a few occasions with the apparatus of 1857.

When the vessel A was full of water, it was drawn off by a cork-hole at the bottom, air being at the same time admitted by the tube *x* at the top of the vessel.

The minimum pressure upon the glass shade F would be

$$\frac{1{\cdot}0 \times 1{\cdot}85}{13{\cdot}6} = 0{\cdot}136 \text{ inch},$$

in which 1·0 is the difference between the height of the lowest and the highest level of the sulphuric acid in the bulb-apparatus M. Experiments showed, however, that owing to friction, &c., the maximum pressure on the inside of the glass shade would be raised to double the above estimated minimum.

The plants were supplied with water, as already said, through the tube *u v*, shown best in fig. 2, Plate XIV. At first fresh distilled water was supplied; but as soon as a sufficient quantity of condensed water had run through the tube *n' o'* and collected in the bottle O, this was drawn off by means of the tube *t'* to water the plant when required. In the experiments of 1857, the condensed water was drawn off from time to time, from the surface of the slate and mercury, by means of the tube *n o*.

All the Woulfe's bottles were made as air-tight as possible by means of very good corks. Those of the bottles E, Plate XIII., and fig. 1, Plate XIV., and also those of O, fig. 1, Plate XIV., were, however, covered with a cement, composed of eight parts gutta percha, twelve parts common rosin, and one part Venice turpentine, well melted together. The glass tube *n' o'* was also fixed into the lute-vessel *w w*, at *n'*, with this cement.

In the experiments of 1857, tubes of unvulcanized caoutchouc, made by ourselves from the sheet, were used for the various joints indicated in the figures; but as these soon became unsound under the influence of the atmospheric changes to which they were exposed, tubes of vulcanized caoutchouc were substituted in 1858. The ends of the glass tubes *o* and *u*, Plate XIII., and of the tubes *t'* and *u*, fig. 1, Plate XIV., were fitted with pieces of caoutchouc tubing into which pieces of solid glass rod were fixed as stoppers.

In 1857, twelve such sets of apparatus, and in 1858 a larger number, were employed. The whole were arranged side by side, on stands of brickwork erected for the purpose,

in the open air, and were protected from rain, or the too powerful rays of the sun, by a canvas awning which could be drawn down over them, or withdrawn, at pleasure.

In 1858, in addition to the sets of apparatus above described, two glazed cages, such as were used by M. G. VILLE in his experiments, and which he kindly sent over to us for the purpose, were employed.

1.—*The supply of Carbonic Acid to the Plants.*

Owing to the small proportion of carbonic acid in the atmosphere, and to the fact that a part of it would be absorbed in passing through the apparatus just described, it was necessary to give a supply of it to the plants artificially. It was obtained by the action of chlorhydric acid upon fragments of marble in the vessel T, Plate XIII., or T', fig. 1, Plate XIV.

In regulating the supply of carbonic acid, the points to be observed were, to keep the proportion in the enclosed atmosphere below that in which it would prove injurious to the plants, and at the same time to provide a sufficient quantity for the demands of vegetation at the most active periods of growth.

BOUSSINGAULT found* that the air surrounding a plant might, consistently with healthy growth, contain 8 per cent. of carbonic acid. This amount, then, on the one hand, and the very small quantity in the atmosphere which is sufficient for natural vegetation (about 0·04 per cent.), on the other hand, afford us limits between which a wide range is allowed for variation.

Calculation showed that a minimum quantity of 0·2 per cent. of carbonic acid in the air of the enclosing apparatus would supply 5 cubic inches of the gas within the shade at one time, corresponding to 0·0439 gramme of carbon—a quantity which, maintained daily throughout the sunlight, would be very much more than was required by the plants.

It is obvious, therefore, that a variation in the amount of carbonic acid in the atmosphere of the plants between 4·0 per cent. and 0·2 per cent. would be very safely within the limit suggested by the experiment of BOUSSINGAULT as the maximum, on the one hand, and that indicated by the above considerations as the minimum desirable in the experiment, on the other.

A question arises as to the influence which the *changes* in the proportion of carbonic acid in the air, between the assumed limits, may have upon the plant. In reference to this point, it may be mentioned that our own experiments upon the nature of the gas in plants (some of the results of which will be given further on) appear to show that the changes in the proportion of the carbonic acid in the air of the cells and intercellular passages, and in that in the fluids of the stem, are much greater, and more rapid than those which can take place in the atmosphere of our apparatus. In addition to this, may be stated the fact that plants derive much of their carbonic acid from aqueous solution absorbed by the roots; and most probably the remainder is absorbed by the fluids of the plant before influencing its growth. These absorptions can take place but

* Mémoires de Chimie Agricole et de Physiologie, 1854, p. 441.

slowly; so that somewhat rapid variations in the proportion of carbonic acid in the atmosphere surrounding the plant, will be accompanied by much less variation in the proportion of carbonic acid within the plant. The latter will, therefore, be a slightly varying mean between amounts corresponding to the foregoing extremes.

From the above considerations, it appeared probable that there would be no danger in so supplying carbonic acid to the atmosphere of the plants as that its proportion should reach its maximum in a short time, and then, by the passage of air, gradually fall again to the minimum. A few trials, adding different quantities of chlorhydric acid to the vessel T, Plate XIII. (or T', fig. 1, Plate XIV.), containing marble, enabled us to ascertain the proper quantity to add, to provide about 4 per cent. of carbonic acid in the shade F when air was not passing. Then passing air, it was found that the proportion of the gas was never reduced below that which we have above assumed as the proper minimum. In practice a little more chlorhydric acid than the amount so determined was used; and then the passage of the air was commenced simultaneously with the addition of the acid. Repeated analysis of the air in the enclosing apparatus showed that, operating in this way, our assumed limits for the maximum and minimum proportions, respectively, of carbonic acid were not passed.

The volume of the air passed through the apparatus daily, was that of the vessel A, Plate XIII. and fig. 1, Plate XIV., and was equal to about 2·5 times that of the enclosing shade F.

K.—*Advantages of the Apparatus above described.*

The advantages, for the purpose in question, of the plan of apparatus which has been described, over those of several of the forms that have been suggested or used by others, may be very briefly stated.

1. When once ready to receive the plant, the use of the apparatus is extremely simple and easy. It is only necessary to place the pot containing the soil, seed, &c., with its pan, in the stone-ware lute-vessel, to pour mercury into the groove, to arrange the several tubes, and to put on the shade. The plant is then entirely excluded from all external sources of combined Nitrogen; and, in case of its being necessary to open the vessel for any purpose, this can be done with great facility.

2. By means of the arrangement of the bottle O (fig. 1, Plate XIV.), the water which condenses within the shade is removed from the atmosphere of the plant as soon as it collects. The small pan in which the pot stands (fig. 3, Plate XII.), with its inward-turned sides, allows of a store of water being kept beneath the plant which is at the same time protected from free evaporation. The vessel O holds as much water as can be evaporated from the plant and soil during several days. The supply of water to the plant is exceedingly easy and simple, it being only necessary to remove that which has collected in the bottle O by means of the tube *t'*, and to pour it in at *u* (figs. 1 and 2, Plate XIV.). [In the arrangement for the experiments of 1857 the condensed water collected on the surface of the slate, until removed by means of the tube *n o*.]

3. A simple glass shade is liable to introduce fewer sources of error than a complicated metallic framework with panes of glass cemented into it. The shade is easier to clean before commencing the experiment, and it is less likely to retain, at the termination of it, any of the combined Nitrogen, either derived from the plant, or from that which has been supplied during growth. Lastly, the presence of oxidizable metallic surfaces, affording a possible quantity of nascent hydrogen which might form ammonia with the Nitrogen of the air, is avoided.

4. There is no organic matter present which can affect the result of the experiment. The only organic matter within the shade is that of a thin coating of the gutta-percha cement which has been described, by which the tube n' (fig. 1, Plate XIV.) is fixed into the hole n' (fig. 4, Plate XII.) at the bottom of the stone-ware lute. On analysis this cement was found to contain from 0·10 to 0·15 per cent. of Nitrogen. Hence, if the whole quantity of the cement in contact with the condensed water became decomposed, and yielded up its Nitrogen in such a manner as to become a product of the experiment, it would only so yield a few tenths of a milligramme of Nitrogen; but experiment proved that it did not suffer sensible decomposition when subjected, during a whole year, to exposure in the open air.

5. In the passage of the air through the apparatus, the excess of pressure was upon the inside, instead of, as in the experiments of others, upon the outside of the enclosing vessel. In experiments of the kind in question in which the apparatus is exposed to the open air, and so subjected to climatic vicissitudes during a considerable period of time, the ordinary means of securing tightness in the laboratory cannot be depended upon; and an apparatus proved to be tight at one time may, as the result of a variety of causes beyond our control, be subject to leakage at another. But a leakage from the inside of the apparatus outwards cannot affect the result of our experiment; whilst a leakage in the opposite direction might introduce combined Nitrogen from the external atmosphere. In the arrangement which has been described, the excess of pressure is always on the inside during the passage of the air; and when the air is not passing there cannot be any important amount in the opposite direction due to changes of temperature and barometric condition, for it can never exceed that required to drive the air inwards through the bulb-apparatus M (Plate XIII., and fig. 1, Plate XIV.), which is altogether insignificant.

6. That part of the apparatus which would be the most liable to leak, and which would be the most damaged by pressure, is subjected to the minimum amount of it. The entire pressure required to force the air through the apparatus, independently of that necessary to overcome friction, is

$$5 \times 1{\cdot}85 = 9{\cdot}25 \text{ inches of water}$$

to pass through the sulphuric acid in the bottles B and C (Plate XIII., and fig. 1, Plate XIV.), and

$$2{\cdot}5 \times 1{\cdot}2 = 3{\cdot}0 \text{ inches of water}$$

to pass through the solution of carbonate of soda in the bottle E, and

$$1 \times 1{\cdot}85 = 1{\cdot}85 \text{ inch of water}$$

to pass through the sulphuric acid in the bulb-apparatus M, equal a total of 14·10 inches water, or a minimum pressure of about 0·5 lb. per square inch. Direct experiment with a manometer showed, however, that the entire pressure, minus that due to the sulphuric acid in the bulb-apparatus M, might, owing to friction, &c., amount to 0·8 lb. per square inch. This would give a lateral pressure upon the sides of the glass shade of about 900 lbs., if the current of air were produced by aspiration instead of forcing—a condition which would be incompatible with the safety of the vessel. In the mode of experimenting adopted, however, the only pressure exerted upon the glass shade was the amount requisite to force the air through the bulb-apparatus M.

It remains to consider the influence upon the air of its contact (in the vessel A) with the water employed to force it through the apparatus. This can be of three kinds:—

1. The proportions of nitrogen and oxygen may be slightly affected by absorption, under the influence of the slightly increased pressure to which the air is subjected.

2. The air may lose its carbonic acid.

3. It may become more or less saturated with aqueous vapour.

The increase of pressure to which the air is subjected in the vessel A is so slight, and the time in which it is there in contact with the water is so short, that the total amount of oxygen and nitrogen absorbed by the water must be very small; and, since any change in the constitution of the total amount of air will be dependent on the ratio of the absorption coefficients of oxygen and nitrogen on the one hand, and on the ratio of the quantities of these gases in the air on the other, it will be very much less than in the actual amount of air absorbed; it will in fact be too small to be of any importance.

The whole of the carbonic acid of the air may be absorbed by the water; but as arrangements are made for the artifical supply of it, this is of no consequence.

The amount of water taken up by the air in the vessel A would at first sight appear to be of more importance. But the time during which the air is in contact with the water in the vessel A is very short, and probably too short for its saturation; it must lose most or all of its acquired water in passing through the sulphuric acid in the bottles B and C, and over the pumice saturated with sulphuric acid in the tube DD, whilst the redried air passes too rapidly through the carbonate of soda solution in the bottle E for re-saturation; and lastly, as the air in its previous course through the apparatus will be cooler than within the shade, it will not be so near its point of saturation in the latter as it may be before it reaches it.

L.—*Adaptation for healthy growth of the conditions of experiment adopted.*

We have thus far discussed the possible sources of error in an experiment on the question of the assmilation of Nitrogen by plants, so far as regards the soil, the inorganic nutriment, and the air, to be provided for the plant, and we have pointed out the means

adopted to avoid them. From known considerations with regard to the requirements in the soil and inorganic nutriment on the one hand, and in the atmosphere of the plants on the other, and in all combined, we have concluded what are the proper conditions of vegetable growth. It remains, however, to appeal to the results of direct experiment, to show that our adopted conditions possess the value which we have assumed them to have.

A pot of good garden soil, capable of supporting luxuriant vegetation in the open air, was sown with Wheat, Barley, and Beans, and then placed under one of the experimental shades, and submitted to exactly the same atmospheric conditions as those provided in the experiments on the assimilation of Nitrogen. The result was, exceedingly luxuriant growth (see Records of growth in Appendix, Experiment No. 12, of "Plants grown in 1857," fig. 13, Plate XV.; and also Experiment No. 15, of "Plants grown in 1858." It was thus proved that the *aërial* conditions supplied in our experiments were adapted for healthy growth.

When pots of soil, prepared precisely as has been described above, were sown with seed and combined Nitrogen artificially supplied, vigorous growth was the result. Hence it was shown that the conditions of *soil* were properly selected.

Section II.—OTHER CONDITIONS OF EXPERIMENT, REQUIRING COLLATERAL INVESTIGATION.

There remain to be considered several conditions which might affect the result of a quantitative experiment on the assimilation of Nitrogen by plants, dependent upon the reciprocal action of the air and the soil, with or without the connexion of the plant.

The following conditions possibly affecting the result of such an experiment, due to the mutual action of the soil, air, and organic matter of the plant, require to be considered:—

1. The influence of ozone, either within the cells of the plant, or in connexion with it, in promoting the formation of nitrogenous compounds from free Nitrogen. The influence of ozone in promoting such formation within the soil, either directly, or in connexion with the organic matter of the plant.

2. The decomposition of nitrogenous organic matter, in relation to the question whether there be an evolution of free Nitrogen in the process.

3. The formation of nitrogenous compounds, through the mutual action of nascent hydrogen evolved by decomposing organic matter, and free Nitrogen.

A.—*General considerations in regard to the possible influence of Ozone on the supply of combined Nitrogen to growing plants.*

The consideration of Ozone in connexion with the plant suggests the possibility of its presence in two distinct ways. It may occur within the cells and intercellular passages of the plant, either in the gaseous state or in solution, or it may be simply around the plant, without existing within its structures.

With regard to the origin of Ozone in connexion with the plant, it may be a product

of the action of the sun's rays, by virtue of which carbonic acid is decomposed, and oxygen evolved. Or, it may result from other causes, to which we shall refer presently.

In order to ascertain how far the presence of Ozone within the plant may have a bearing upon the point at issue, we have attempted to solve, by experiment, the following questions:—

1. Is there, during the growth of plants, Ozone within the cells or intercellular passages?

2. If Ozone be present within the structures of the plant, is it in circumstances in which it would be likely to oxidize free Nitrogen into any of its oxygen compounds?

3. Is Nitric acid present in the living cells of any plant of which it is not a natural product of growth?

In a number of experiments which we have made upon the gases obtained by exhausting plants placed in water freed from air by boiling, no Ozone was perceptible. Another series of experiments upon the oxygen evolved from plants immersed in water saturated with carbonic acid gave similar results.

In the latter series about 1 ounce of the green plant was placed in 500 cub. cents. of carbonated water, and the whole subjected to sunlight. The decomposition of carbonic acid commenced almost immediately, and the evolution of gas was rapid. In this way 100–200 cub. centims. of gas were obtained, which contained sufficient oxygen to inflame a glowing taper; yet no trace of Ozone was manifested on placing test-paper in the gas. That evolved from Wheat, Barley, Oats, Beans, and Clover behaved alike in this respect. Granting that these experiments may not be conclusive for all conditions of the decomposition of carbonic acid by plants, that under certain circumstances Ozone may exist within the vegetable cells and the passages between them, and that it is possible that some of the oxygen of the decomposed carbonic acid may at times appear as Ozone, still, it is difficult to see how it can exert any oxidizing influence upon the free Nitrogen within the plant, under the peculiar circumstances in which it must come in contact with it.

In order to study more fully the circumstances, and to examine, in some detail, the value of the oxidizing and reducing forces operating in the vegetable organism, in the different conditions to which it is subjected during growth, a number of experiments have been made upon plants, under a variety of conditions more or less analogous to those of ordinary growth. As the results of these investigations are too extended in their bearings for full consideration in the present Paper, and are, moreover, not yet sufficiently complete for publication, we shall give here only such of them as bear upon the point now in question.

It is obvious that the formation of Nitric acid, by the mutual action of Ozone and free Nitrogen within the plant, will be dependent upon the activity of the oxidizing power of the Ozone, and on the intensity of the reducing power of other substances in contact with the Nitrogen to be oxidized.

The investigations of SCHÖNBEIN and others appear to show that, under certain circumstances, nitric acid may be formed by the mutual action of Ozone and free Nitrogen. The question for our consideration here is, whether these circumstances are presented in the cells of plants, and in the passages between them, during growth? The subject of the relation of Ozone to organic matter is obviously too extensive for anything more than a passing consideration here; but we may refer to the well-known intense action of this peculiar body upon organic matter generally, by which carbonic acid is formed, and the Ozone destroyed. It is well known that Ozone is rapidly destroyed if kept in contact with phosphorus or any other reducing substance. If such conditions for the destruction of Ozone exist within the plant, the probability that it can there oxidate free Nitrogen, and so form nitrates, would appear to be exceedingly small. The actual conditions within the plant in regard to the points in question may be most efficiently studied by the examination of the gases they contain, under various circumstances. We proceed, therefore, to notice some of the results of such an examination.

B.—*Composition of the Gas in Plants.*

Experiments, Series 1.

Plants, or parts of plants, were put into a flask filled with water that had previously been well boiled to remove all air from it. A cork, through which a bent glass tube was passed, was then pressed into the flask, so that the tube was filled with the displaced water. The flask was then placed over a lamp, the water boiled, and the water and gas driven over collected over mercury, the boiling being continued until the water distilled over raised that first driven out with the gas to the boiling-point. The vapour thus produced expelled most of the water collected over the mercury. In this way the gas driven out from the plant at the boiling-point was obtained. The following Table (I.) shows the composition of the gas collected under these circumstances. It is seen that Nitrogen and Carbonic acid only were present.

TABLE I.—Showing the Percentage Composition of the Gas evolved from plants, in water, on continued boiling.

Date (1857).	Description of plant operated upon.			Per cent.		
	Plant.	Part of plant.	How manured, &c.	Nitrogen.	Oxygen.	Carbonic acid.
May 6.	Wheat.	Whole plant.	Mineral manure	45·47	0·0	54·53
May 2.	Wheat.	Whole plant.	Mineral manure	46·29	0·0	53·71
May 2.	Wheat.	Lower part.	Mineral manure	57·00	0·0	43·00
May 2.	Wheat.	Whole plant.	Mineral and Ammoniacal manure.....	39·14	0·0	60·86
May 2.	Wheat.	Upper part.	Mineral and Ammoniacal manure.....	37·53	0·0	62·47
May 6.	Bean.	Whole plant.	Mineral manure	62·79	0·0	37·21
May 6.	Bean.	Whole plant.	Mineral manure	63·80	0·0	36·20

Other experiments gave similar results, all tending to show that the reducing power

of the vegetable cells, dependent on the character and conditions of the carbon compounds they contain, was sufficient, under the circumstances specified, to consume all the oxygen (or ozone) that might be present. But the high temperature at which the experiment was conducted must have tended very much to increase this action. In subsequent experiments a different plan of operation was adopted, not open to the same objection.

Experiments, Series 2.

In these experiments, as in all those subsequently referred to, the plants were put into a tall glass vessel (fig. 7, Plate XII.) 1·75 inch in diameter, and 14 inches in height. The mouth of this vessel is fitted with a long cork, previously well boiled in bees'-wax. Through the cork, two glass tubes, *a* and *b*, are inserted. The vessel being filled with water well boiled and then cooled without access of air, the plant is put in and well shaken to remove adherent air-bubbles. The cork, with its two tubes, is then forced in, taking care that both the tubes become filled with water and that no air remains in the vessel. As a further security for tightness, a piece of wide and thick caoutchouc tubing may be drawn over the neck of the vessel, projecting upwards a little above the cork, and then the cup thus formed partly filled with melted wax, forming a layer over the cork and its joints. A funnel is then attached to the tube *b*, by means of a caoutchouc tube which can be closed by a strong pinch-cock. Water being admitted through the funnel into the tube *b*, the tube *a* becomes filled, and it is then brought into connexion, by means of a glass tube and caoutchouc joint fitted with a pinch-cock, with a vessel filled with quicksilver. The connexion being opened, the quicksilver is allowed to flow from the vessel by means of a long tube of more than barometric length fitted into the lower part of it, thus forming a Torricellian vacuum in the mercury vessel. The gas from the plant passes over into this vacuum, and by a simple arrangement is collected in a eudiometer tube for examination.

The following Table shows the amount and composition of the gas obtained from different plants, in the shade, in the manner above described.

TABLE II.—Showing the amount and the composition of the Gas given off by plants, in the shade, into a Torricellian vacuum.

Date.	Description.		Total Gas collected, cub. cents.	Per cent.			
	Part of Plant.	How manured, &c.		Nitrogen.	Oxygen.	Carbonic acid.	Oxygen and Carbonic acid.
		Wheat; 1858.					
June 16.	Whole plant	Mineral manure	57·0	77·72	2·28	20·00	22·28
June 17.	Whole plant	Mineral manure	55·3	77·94	5·06	17·00	22·06
June 16.	Whole plant	Mineral and Ammoniacal manure	57·0	78·60	1·75	19·65	21·40
June 16.	Whole plant	Mineral and Ammoniacal manure	55·7	77·38	3·23	19·39	22·62
June 16.	Whole plant	Mineral and Ammoniacal manure	65·7	82·50	0·30	17·20	17·50
		Barley; 1857.					
June 24.	Whole plant	Unmanured	8·6	85·12	3·93	10·95	14·88
June 24.	Whole plant	Unmanured	20·9	81·48	1·97	16·55	18·52
		Beans; 1858.					
June 17.	Whole plants coming into Flower	Unmanured	54·3	79·74	5·16	15·10	20·26
June 17.	Whole plants coming into Flower	Unmanured	41·5	86·74	3·10	9·16	13·26
June 17.	Whole plants coming into Flower	Ammoniacal manure	52·5	80·38	4·38	15·24	19·62
June 17.	Whole plants coming into Flower	Ammoniacal manure	50·4	84·33	4·36	11·31	15·67
		Clover; 1857.					
Aug. 10.	Heads	Unmanured	47·7	85·61	6·00	8·39	14·39
Aug. 10.	Stems and Leaves	Unmanured	59·8	83·23	2·33	14·44	16·77
Aug. 11.	Heads	Unmanured	91·0	87·15	1·89	10·96	12·85
Aug. 11.	Stems and Leaves	Unmanured	42·3	78·32	1·31	20·37	21·68

These experiments also tend to show that the reducing-power of certain of the carbon compounds of the plant was sufficient to convert nearly all the oxygen (or ozone) present into carbonic acid, when in the shade.

The next point is to consider how far the conditions are favourable to the oxidation of Nitrogen in the vegetable organism, when the plant is subjected to the action of the direct rays of the sun.

Experiments, Series 3.

In these experiments, in which over 100 exhaustions were made, the operation was conducted precisely as in the case of the last experiments, with the exception that the plants were exposed during the whole process to the direct rays of the sun. The following Table exhibits a few of the results obtained, which are sufficient for our present purpose.

TABLE III.—Showing the amount and composition of the Gas given off into a Torricellian vacuum, by plants exposed to sunlight.

Date.	How manured, &c.	Total Gas collected, cub. cents.	Per cent.			
			Nitrogen.	Oxygen.	Carbonic acid.	Oxygen and Carbonic acid.
	Wheat (whole plant), 1858.					
June 22.	Unmanured	44·4	73·65	21·17	5·18	26·35
June 23.	Unmanured	34·8	77·01	21·26	1·73	22·99
June 30.	Unmanured	44·1	72·79	20·86	6·35	27·21
June 22.	Mineral and Ammoniacal manure	54·5	73·76	21·29	4·95	26·24
June 23.	Mineral and Ammoniacal manure	42·1	78·15	15·44	6·41	21·85
June 25.	Mineral and Ammoniacal manure	37·2	78·76	19·09	2·15	21·24
	Grass (whole plants), 1857.					
August 15.	Mineral and Ammoniacal manure: second crop	39·0	82·10	16·19	1·71	17·90
August 15.	Mineral and Ammoniacal manure: second crop	47·8	77·08	15·35	7·57	22·92
August 15.	Mineral and Ammoniacal manure: second crop	41·6	76·56	21·46	1·98	23·44
August 17.	Mineral and Ammoniacal manure: second crop	39·9	75·07	23·39	1·54	24·93
August 18.	Mineral and Ammoniacal manure: second crop	36·8	79·88	15·19	4·93	20·12
August 18.	Mineral and Ammoniacal manure: second crop	42·3	80·23	15·97	3·80	19·77
	Beans, 1858.					
July 12.	Mineral manure; almost podding	44·3	71·11	18·28	10·61	28·89
July 12.	Farm-yard manure; almost podding	45·8	73·14	10·26	16·60	26·86
July 15.	Unmanured; almost podding	25·9	82·63	15·83	1·54	17·37
July 15.	Mineral and Ammoniacal manure; almost podding	30·9	70·55	20·71	8·74	29·45

The general accordance in the proportions of Nitrogen found throughout this Series, together with their general approximation to the amounts observed in Series 2 (Table II.), and the consequent similarity in range of the sums of the two remaining gases—carbonic acid and oxygen—point to the character of the change which has taken place, by virtue of which the proportion of carbonic acid is diminished, and that of oxygen increased. The variations in the amounts are, nevertheless, somewhat considerable; and we feel that it would be requisite to exercise considerable caution in attempting to refer them to any other than accidental circumstances beyond our control. There can be no doubt, however, that the carbonic acid, shown to exist in the plants in the shade, has yielded the oxygen evolved when in the sunlight. But the mutual relations of the two gases will be more clearly brought to view by a consideration of the results yet to be adduced.

Experiments, Series 4.

These experiments, as well as those of the succeeding Series, were arranged to show the influence of the time of action of the sunlight on the plant, upon the relative proportions of carbonic acid and oxygen.

In the Series of experiments now under consideration, duplicate quantities of the

plant were operated upon at the same time. Both were prepared in the shade; and then the vessels containing them were each entirely excluded from the light, by means of a thick paper covering. In this condition each was attached to a Torricellian exhauster*. The paper was then removed from one of the vessels so as to expose it, with its contents, to the direct rays of the sun; the other vessel, with its enclosed plant, remaining covered. The exhaustion of both was then commenced immediately, and the action continued for half an hour.

The following Table shows the results obtained in this manner, in sunlight, and in the dark, respectively.

TABLE IV.—Showing the amount and composition of the gas evolved, during half an hour, into a Torricellian vacuum, by duplicate quantities of plant, both kept in the dark for some time before commencing the exhaustion, then one exposed to sunlight, and the other kept in the dark, during the process.

(1858.)

Date.	Description of Plant.	Conditions during Exhaustion.	Total Gas collected.	Per cent.			
				Nitrogen.	Oxygen.	Carbonic acid.	Oxygen and Carbonic acid.
			cub. cents.				
	Beans ...	In dark.....	25·7	66·93	2·33	30·74	33·07
		In sunlight ...	36·4	69·78	8·24	21·98	30·22
July 22.	Oats.....	In dark.....	28·3	81·63	3·53	14·84	18·37
		In sunlight ...	25·9	70·27	13·13	16·60	29·73
July 23.	Oats.....	In dark.....	26·4	73·11	8·33	18·56	26·89
		In sunlight ...	22·7	72·25	16·74	11·01	27·75
July 23.	Oats.....	In dark.....	27·4	68·25	5·11	26·64	31·75
		In sunlight ...	29·2	67·47	19·86	12·67	32·53
July 23.	Oats.....	In dark.....	31·4	77·39	6·69	15·92	22·61
		In sunlight ...	21·7	76·50	16·59	6·91	23·50

The amounts of carbonic acid and oxygen recorded in the Table, indicate very clearly the ready transformation of the one into the other—or, rather, the transformation of carbonic acid into a solid carbon compound, and free oxygen. In reference to the question we are considering, these results have a high importance, as showing the great reducing-force manifested under the influence of the sun's rays, by which the carbonic acid is so suddenly reduced.

* This term, for convenience, we apply to the apparatus which has been described at p. 487, by which the plant in the vessel, fig. 7, Plate XII., is exhausted.

Experiments, Series 5.

This set of experiments was arranged to show how far the reduction of the carbonic acid, with the evolution of oxygen, was due to the action of the sunlight, in conjunction with the fluids of the plant, at the moment of the passage of the gas through the walls of the cells.

If the decomposition of the carbonic acid resulted from a physico-chemical action, in the presence of sunlight, upon this gas only as it passed through the cell-walls, then there might be no oxygen liberated in the growing cell. If, on the contrary, it were decomposed before passing out of the cell, free oxygen would exist within the latter.

To settle this question, a set of experiments was made exactly similar to those the results of which are given in Table IV., with the exception, that now the time of the exhaustion, and of the action of the sunlight, was reduced to four or five minutes, and the quantity of plant operated upon was increased, so as to give sufficient gas for analysis during this short period. The following Table gives the results obtained.

TABLE V.—Showing the amount and composition of the Gas evolved into a Torricellian vacuum, during four or five minutes only, by duplicate quantities of plant, both kept in the dark for some time before commencing the exhaustion, then one still kept in the dark, and the other exposed to sunlight during the short period of the operation.

(1858.)

Date.	Description of Plant.	Conditions during Exhaustion.	Total Gas collected.	Per cent.			
				Nitrogen.	Oxygen.	Carbonic acid.	Oxygen and Carbonic acid.
			cub. cent.				
July 30.	Oats.....	In dark.....	41·7	72·42	3·6	23·98	27·58
		In sunlight ...	42·5	72·23	4·71	23·06	27·77
July 30.	Oats.....	In dark.....	55·7	71·46	3·23	25·31	28·54
		In sunlight ...	43·3	69·98	3·23	26·79	30·02
July 30.	Oats.....	In dark.....	37·9	83·11	6·86	10·03	16·89
		In sunlight ...	38·5	77·14	9·09	13·77	22·86
July 31.	Oats.....	In dark.....	34·4	78·49	7·27	14·24	21·51
		In sunlight ...	41·8	75·84	7·89	16·27	24·16

The above results show that the carbonic acid can pass through the cell-wall, in the presence of sunlight, without suffering decomposition. It would hence appear that the free oxygen which a plant yields after it has been for some time under the influence of the direct rays of the sun, existed as such in the cells before the exhaustion. The slight preponderance of oxygen observed in the gas exhausted in sunlight is doubtless due to its action upon the carbonic acid within the cell, during the short period of its operation upon it before it passes out; precisely analogous to the action when the plant is subjected to ordinary atmospheric pressure.

Experiments, Series 6.

In order to bring out more clearly the influence of sunlight before the exhaustion, a series of experiments were made, in which two vessels, containing the duplicate quantities of plant, were each kept covered with paper for some time, and then, from twenty to thirty minutes before commencing the exhaustion, the paper was removed from one of them, both being then exhausted,—the process continuing ten, fifteen, or twenty minutes. The following results were obtained.

TABLE VI.—Showing the amount and composition of the Gas evolved into a Torricellian vacuum, by duplicate quantities of plant, both kept in the dark for some time, and then one exposed to sunlight for about twenty minutes, when both were submitted to exhaustion.

Date.	Description of Plant.	Conditions during Exhaustion.	Total Gas collected.	Per cent.			
				Nitrogen.	Oxygen.	Carbonic acid.	Oxygen and Carbonic acid.
			cub. cents.				
July 31.	Oats.....	In dark.....	24·0	77·08	3·75	19·17	22·92
		In sunlight ...	34·5	68·69	24·93	6·38	31·31
Aug. 2.	Oats.....	In dark.....	18·6	68·28	10·21	21·51	31·72
		In sunlight ...	39·2	67·86	25·25	6·89	32·14
Aug. 2.	Oats.....	In dark.....	30·7	76 87	8·14	14·99	23·13
		In sunlight ...	26·5	69·43	27·17	3·40	30·57
Aug. 2.	Oats.....	In dark.....	17·0	79·41	7·65	12·94	20·59
		In sunlight ...	28·6	76·22	18·53	5·25	23·78
Aug. 3.	Oats.....	In dark.....	29·8	81·88	6·38	11·74	18·12
		In sunlight ...	32·1	66·36	30·53	3·11	33·64
Aug. 3.	Oats.....	In dark.....	11·6	65·52	6·90	27·58	34·48
		In sunlight ...	23·1	70·56	20·35	9·09	29·44
Aug. 3.	Oats.....	In dark.....	17·0	80·00	5·88	14·12	20·00
		In sunlight ...	19·7	73·10	22·33	4·57	26·90

The comparison of the results in this Table with those in Table V., shows that the oxygen must have been liberated from the carbon, and been retained within the cells, until the instant of the exhaustion, as the gas was evolved from all parts of the leaf, and not from the surrounding water, as soon as the pressure was removed.

The conclusions to be drawn from the above several Series of experiments are not without an interesting bearing upon our present subject.

1. Carbonic acid, within growing vegetable cells, and intercellular passages, which are penetrated by the sun's rays, suffers decomposition with the evolution of oxygen, the latter remaining in the plant or being evolved from it. This takes place very rapidly after the penetration of the sun's rays.

2. Living vegetable cells, &c., which are in the dark, or are not penetrated by the direct rays of the sun, consume the oxygen they contained very rapidly after being placed in such circumstances, carbonic acid being formed.

3. There can hence be little or no oxygen in the living cells of plants during the night, or during cloudy days. The presence of oxygen in the cells of thick-leaved plants, or in the deeper layers of fruit, is also very problematical.

4. With every cloud that passes over the sun, the oxygen of the living cell will oscillate under the influence of the reducing-force of the carbon-matter, forming carbonic acid, on the one hand, and of the reducing-forces of the associated sun's rays, liberating pure oxygen and forming a carbon-compound containing less oxygen than carbonic acid, on the other.

5. The idea is suggested by the above considerations, that there may be in the outer cells, which are penetrated by the sun's rays, a reduction of carbonic acid, and a fixation of carbon, with the evolution of oxygen, at the same time that, in the deeper cells, the converse process of the oxidation of carbon and the formation of carbonic acid is taking place. If such be the case, the oxygen of the outer cells would, according to the laws in conformity with which the diffusion of gases and their passage through tissues are known to take place, be continually penetrating to the deeper cells, and there oxidizing their carbon-matter into carbonic acid; whilst the carbonic acid thus formed would pass in the opposite direction to be decomposed in the sunlight of the outer cells. As the process of cell-formation went forward, and the once outer cells became buried deeper by the still more recent ones above them, they would gradually pass from the state in which the sunlight was the greater reducing-agent, to that in which the carbon-matter of the cell became the greater—from the state in which there was a flow of carbonic acid to them and of oxygen from them, to that in which the reverse action took place. The effect of this action may be the formation of oxidized products—acids, or saccharine matter, &c.—in the deeper cells, whilst the great reducing-power of the sun's rays may form more highly carbonized substances in the outer cells, which in their turn become subject to oxidation when buried deeper. The physical and physiological phenomena of such interchanges are obviously worthy of a closer study; but the subject is too wide for any further development here.

6. The very great reducing-power operating in those parts of the plant where ozone is most likely, if at all, to be evolved, seems unfavourable to the idea of the oxidation of Nitrogen into nitric acid by its means—that is to say, under circumstances where the much more readily oxidizable substance, carbon, is not oxidized, but on the contrary its oxide, carbonic acid, is reduced; whilst, as has been seen, when beyond the influence of the direct rays of the sun, the cells seem to supply an abundance of the more easily oxidized carbon, in a condition of combination readily available for oxidation by free oxygen, or ozone, should it be present. The conclusion that free Nitrogen would not be likely to be oxidated into nitric acid within the structures of the plant, seems to be borne out by the well-known fact, that nitrates are as available a source of Nitrogen

to plants as ammonia; and hence, if we were to admit that Nitrogen can be oxidated into nitric acid in the plant, we must suppose, as in the case of carbon, that there are conditions under which the oxygen compound of Nitrogen is reduced within the organism, and that there are others in which the reverse action, namely, the oxidation of Nitrogen, can take place. In relation to this question, it may be mentioned that several specimens of green Wheat and Grass which had been liberally manured with nitrates were examined for nitric acid, but no trace of it was found in them.

7. To the foregoing six conclusions, another may be here added relating to this subject, though deduced from the results of experiments on the decomposition of organic matter, which will be referred to more fully presently (p. 509 *et seq.*). So great is the reducing power of certain carbon-compounds of vegetable substances, that when the vital (growing) process has ceased, and all the free oxygen in the cells has been consumed, in the formation of carbonic acid, water is decomposed, and hydrogen is evolved. This process does not, however, continue long, showing that the cell provides a certain amount of matter more easily oxidized than the remainder, or that the entire cell-matter, after becoming slightly oxidized, loses its energetic reducing-power. The former alternative is the more probable one.

The foregoing considerations with regard to the intensity of the reducing action of certain of the carbon-compounds in plants suggest the idea of a possible source of Ozone, very analogous to that by which it is ordinarily obtained by means of phosphorus. As is well known, the process consists in allowing oxygen to come into incomplete or only instantaneous contact with phosphorus. This substance having an intense avidity for oxygen, a part of the latter unites with it to form an oxygen-compound of phosphorus, when, if the contact be not too long, another part passes off in the state of Ozone. Certain carbon-compounds of the vegetable cell have also a great affinity for oxygen in the dark (p. 488); and the oscillations of the affinities, due to the degree of light (pages 489–492), and to the depth of the cell (p. 493), would afford conditions of molecular action somewhat similar to those under which Ozone is produced in the presence of phosphorus. According to this analogy the Ozone would be due to the action of the carbon-compounds of the cell on the common oxygen eliminated from carbonic acid by sunlight, and not to the direct action of the sunlight itself. The Ozone thus formed, if not instantly evolved from the plant, would be destroyed by the easily oxidizable carbon-compounds present. It is more probable, however, that the Ozone, stated by De Luca and others to be observable in the vicinity of vegetation, is due to the intense action of the oxygen of the air upon the minute quantities of volatile hydrocarbons emitted by the plants, and to which they owe their peculiar odours, than to any action going on within the cells. The rapidity of the oxidation in the air of the hydrocarbons, and the volatile principles of plants generally, goes to favour the view here suggested; so also does the fact, that Ozone has been observed most readily in the vicinity of such plants as are known to emit freely essential oils—as, for instance, those of the Labiate family.

Since it would appear that, under certain circumstances, Ozone is formed in the immediate vicinity of some plants, it remains to consider the possibility of its acting, in an indirect manner, as a source of combined Nitrogen to our experimental plants—that is, through the agency of the materials involved in the experiment—and thus compromising our result in regard to the question of the appropriation, by the plant itself, of free or uncombined Nitrogen. It might so act:—

1. By becoming absorbed by the water that condenses within the vessel enclosing the plant, and then oxidizing the free Nitrogen dissolved in the water.

2. By being absorbed by the soil—either directly from the air of the enclosing apparatus, or from the condensed water returned to the soil—and then, in connexion with it, as a moist, porous, and alkaline body, forming nitrates in the manner referred to by PELOUZE and FREMY*, in their remarks upon the experiments of CLOEZ which we have shortly described at p. 465 of this paper.

3. By passing down in solution in water, or in the gaseous state, to the older and decomposing parts of the roots, and there forming nitric acid by the oxidation either of the free nitrogen contained in the older cells, or of that evolved in decomposition.

These questions have not been so fully investigated as, considered as independent subjects of inquiry and with reference to the results obtained by SCHÖNBEIN and others, would be desirable. But so far as they can have a bearing upon the sources of error in our experiments upon the question of the assimilation of free Nitrogen by plants, they have received our careful consideration.

C.—*Experiments on the action of Ozonized air on decomposing Organic matter, and porous and alkaline substances.*

Experiments were made to ascertain the influence of Ozone upon organic matter, and certain porous and alkaline bodies, under various circumstances. The action of ordinary air upon sticks of phosphorus was had recourse to as the source of the Ozone. The arrangement was as follows:—Three large glass balloons (carboys), each of about 40 litres capacity, were connected together by glass tubes which passed through stone-ware stoppers fitted into their mouths, the joints being made tight with calcined gypsum cement. The bottom of each vessel was covered with water to the depth of about half an inch, so that, when pieces of phosphorus were put in, they were partly covered with the fluid. A tube, which could be opened or closed at pleasure, was fixed through each stopper for the supply of water, and fresh phosphorus, as needed. An Allen and Pepys gasometer, capable of holding about 2 cubic feet of air, was connected by a glass tube with the first of the series of vessels; and by its means, air could be forced in a continuous stream through the three vessels containing the phosphorus. On passing out of the last of them it was led through a wash-bottle, and then into a glass vessel, from which, by means of a number of glass tubes passing from it, it was distributed into bottles containing the substances to be submitted to the action

* Traité de Chimie Générale, tome sixième, p. 343 (1857).

of the Ozone. Thus, all the ozonized air passed the wash-bottle in its course from the balloons to the distributing apparatus.

The following substances were subjected to the action of the Ozone—each substance, or mixture, being enclosed in a glass bottle of about 1·5 litre capacity, fitted with an exit-tube in which were fragments of pumice saturated with sulphuric acid:—

(1) ¾ lb. of ignited soil, moistened with 100 cub. centims. water, this being just sufficient to make it slightly coherent.

(2) ¾ lb. of ignited soil, 300 cub. centims. water, 2·5 ounces boiled starch, and 2·5 ounces dry starch.

(3) ¾ lb. of ignited soil, 200 cub. cenitms. water, and 2·5 ounces saw-dust.

(4) 2·5 ounces saw-dust, and 100 cub. centims. water.

(5) ¾ lb. of ignited soil, 200 cub. centims. water, and 2·5 ounces bean-meal.

(6) ¾ lb. of ignited soil, 150 cub. centims. water, and 2·5 ounces bean-meal.

(7) 2·5 ounces bean-meal, and 50 cub. centims. water.

(8) 1 lb. garden-soil.

(9) ¾ lb. of slaked lime, and 2·5 ounces bean-meal, made slightly pasty with water.

(10) ¾ lb. of slaked lime, some starch, and saw-dust, made slightly pasty with water.

(11) 2·5 ounces of boiled starch, 2·5 ounces fresh starch, and 200 cub. centims. water.

All the bottles were placed before a window where the sun shone directly upon them for a considerable part of the day, as it did also for some hours daily upon the balloons.

Every day, about 9 o'clock in the morning, the cylinder of the gasometer was raised, and a slow current of air passed through the apparatus during about two hours. This process was generally repeated once or twice more during the day. The experiment commenced in April, and continued till the following autumn; that is, through all the warm weather of the summer, when a thermometer in the room frequently stood at 25° to 29° C. The amount of Ozone passing through the apparatus was so great, that the vulcanized caoutchouc which connected the tube from the last balloon with that passing into the wash-bottle was cut off with the passage of three or four gasometerfuls of air. The joint was then made by fixing a piece of larger glass tubing over the point of contact of the smaller connecting tubes, and closing the ends of the larger tube with corks well fitted upon the smaller ones.

Once every three or four days a small piece of phosphorus was dropped into each balloon. In this way the action was sufficiently maintained to produce a distinct odour of Ozone in the room whilst the air was passing.

During the first half of the period of the experiment, a wash-bottle filled with large lumps of pumice, and about half-full of a solution of caustic potash, was used; so that the ozonized air in bubbling rapidly through the solution continually threw it up, by which means the pumice was kept moistened with it.

A careful examination of this liquid, together with the washings of the pumice, failed to detect any nitric acid. About the 1st of July, the alkaline wash was replaced by a

bottle containing only pure water. The latter also, at the termination of the experiment, failed to give evidence of even traces of nitric acid.

At the termination of the experiment, the contents of each of the eleven bottles were also examined. A portion was exhausted with water, and the extract concentrated by boiling, after the addition of permanganate of potash to destroy the organic matter. The excess of permanganic acid was removed by carbonate of lead, and the clear solution filtered off from the precipitate. Each solution so obtained was tested for nitric acid; but in no case, excepting that of the garden soil, was there any indication of its presence. An examination of the original garden soil showed that it contained nitric acid before being subjected to the action of the Ozone.

Owing to the negative character of the above results, it is not necessary to describe the apparatus, and the circumstances of the experiments, in any more detail, which would have been desirable had the results been of a positive kind.

We are, however, by no means prepared to infer, from the evidence just adduced, that under no circumstances in nature is it possible for Ozone to transform nitrogenous compounds of the ammonia class, or the nascent nitrogen evolved during decomposition, into oxides of Nitrogen. We would not say that it may not be possible for Ozone to form such compounds when in connexion with non-nitrogenous bodies or porous substances permeated with gaseous Nitrogen, or even in the atmosphere. Nor are we prepared to maintain that the nitric acid in soils is not in part due to some of these causes. These questions will require much further investigation before they can be satisfactorily settled. To some of them we shall refer again presently.

But we wish particularly to call attention to the fact that, in the experiments just referred to, there was a very much larger quantity of Ozone, acting upon organic matter, soil, &c., in a very wide range of circumstances, and for a much longer period of time, than was involved in our experiments on the question of the assimilation of free Nitrogen by plants. Yet there was no appreciable quantity of nitric acid formed. It may therefore be concluded that there will be no error introduced into the results of the experiments on the question of the assimilation of free Nitrogen by plants themselves, arising from the action of Ozone upon free Nitrogen under the circumstances of the experiments, and so providing to the plants an unaccounted supply of combined Nitrogen.

D.—*Evolution of free Nitrogen in the decomposition of Nitrogenous organic compounds.*

Two obvious methods of investigating the question, whether or not free Nitrogen is given off in the decomposition of nitrogenous organic matter, present themselves.

1. To allow the organic matter to decompose under circumstances in which any free Nitrogen that may be evolved can be collected and estimated.

2. To allow the organic matter to decompose under circumstances in which the total amount of the compounds of Nitrogen formed in the process can be estimated—the loss of Nitrogen then representing the free Nitrogen evolved.

A number of experiments according to the first of these methods has been made by Reiset. He submitted nitrogenous animal and vegetable substances to decomposition under an enclosing vessel in ordinary air, into which he passed oxygen as that of the air was consumed. His result was, that the amount of Nitrogen in the air was gradually increased. He does not appear, however, to have completed the inquiries on this subject which he proposed to undertake.

The second method has been followed by M. G. Ville. The conclusion he arrived at was, that in the decomposition of several nitrogenous vegetable substances, about one-third of their total Nitrogen was evolved in the free state.

The losses of Nitrogen which M. Boussingault's experiments on the question of the assimilation of free Nitrogen by plants indicated, when he used nitrogenous organic matter as manure, rendered it desirable to investigate the subject in its bearings upon the conditions provided in our own experiments on that question. The following plan was adopted:—

A given weight of nitrogenous organic matter, the percentage of Nitrogen in which had been previously determined, was mixed with burnt soil, or pumice, prepared as for the experiments on the assimilation of Nitrogen by plants (p. 471), and put into a bottle of about 360 cub. centims. capacity, as shown at B, fig. 8, Plate XII. A proper quantity of water was added; and then the bottle was closed with a cork, through which were tightly fitted two bent glass tubes, which passed externally in opposite directions. One of these tubes was connected with an eight-bulbed apparatus A, containing sulphuric acid, for the purpose of washing air drawn through it into the rest of the apparatus. The other tube, passing from B in the opposite direction, was connected with a similar eight-bulbed apparatus C, containing a solution of oxalic acid. From this again passed a tube extending, through a cork, to the bottom of a second bottle D (similar to B), which contained some sulphuric acid. Through the cork of the bottle D another tube E also passed, but it did not dip into the sulphuric acid. It is obvious that, on drawing the air from D by means of the tube E, a current of air would pass inwards through the sulphuric acid in A, into the bottle B, then through the oxalic acid in C, and so on. In this way, the air of the vessel B, containing the decomposing organic matter, could be renewed at pleasure by fresh air, washed free from ammonia. At the same time, any ammonia evolved from the decomposing organic matter was drawn into the eight-bulbed apparatus C, and there absorbed by the oxalic acid. At the termination of the experiment, the combined Nitrogen remaining in B, and that retained in the form of ammonia in the oxalic acid in C, were determined. The difference between the total amount of combined Nitrogen so found in the products and that originally contained in the organic substance submitted to decomposition, is taken to represent the amount of nitrogen given off, in the free state, during the process.

SERIES 1.—*Experiments on the decomposition of nitrogenous organic matter, made in* 1857.

Wheat-meal, Barley-meal, and Bean-meal were the nitrogenous organic substances submitted to decomposition. A quantity of each of these was mixed respectively with burnt soil and with pumice, making in all six separate experiments. About 100 grammes of soil, or about 60 grammes of pumice, were used—these quantities, together with the meal, filling the bottles B to the depth of about 2 inches. Sufficient water was added to bring the mixture into an agglutinated condition. The materials being so prepared, the apparatus was put together according to the arrangement above described. The six sets were then placed in a light room before a large window, so that, during the middle of the day, the sun shone directly upon them.

The experiments commenced on June 10, and terminated on October 8, 1857. Several litres of air were drawn through each apparatus daily, by applying the mouth to the tube E. After the first day the gas possessed a more or less disagreeable taste, and the odour of decomposing organic matter.

The following statement of the condition of the several mixtures, at the termination of the experiment, is condensed from the notes then made:—

1. *Wheat-meal and ignited Pumice.*—The meal slightly mouldy; the odour that of decomposing organic matter; quite moist, so that the particles of pumice adhered together.

2. *Wheat-meal and ignited Soil.*—A slight mouldy coating on the surface; odour like that of No. 1; the mass moist, but not sufficiently so for the particles of soil to agglutinate.

3. *Barley-meal and ignited Pumice.*—No mouldy coating on the surface; odour similar to that of the wheat but more intense, and sour, much like that of fermenting malt; the mass wet and clammy.

4. *Barley-meal and ignited Soil.*—No mouldy coating on the surface; odour like that of barley No. 3; sufficiently moist to agglutinate.

5. *Bean-meal and ignited Pumice.*—A little mould upon the surface, but not quite so much as with the wheat and soil (No. 2); odour very disagreeable and putrescent; the mass wet and clammy.

6. *Bean-meal and ignited Soil.* Very similar to the bean-meal and ignited pumice (No 5), but a little more wet and pasty.

In every case, carbonic acid was evolved on the addition of oxalic acid, preparatory to evaporating to dryness. The evolution was the greatest from the bean-meal with soil.

A known proportion, about one-half, of each dried mass, was burnt with soda-lime, and the Nitrogen capable of estimation in that way determined. The remainder was reserved for the determination of nitrates, provided any were present. On examination, however, no nitric acid was detected. To put the validity of the qualitative test for nitric acid beyond doubt, 0·001 gramme of nitric acid was added to the portion of substance which had been already exhausted to test for nitric acid, and had yielded a nega-

tive result. The mass was then re-exhausted with water, and the extract submitted to precisely the same process as before, when the presence of nitric acid was made manifest.

In the following Table are given the numerical results of the six experiments:—

TABLE VII.—Showing the Numerical results of experiments on the Decomposition of Nitrogenous organic matter, made in 1857.

	Substances submitted to experiment.				Nitrogen after Decomposition.		
	Description of Organic matter.	Description of Matrix.	Quantity of "Meal" taken.	Quantity of Nitrogen.	Total by Soda-lime.	Not recovered.	
						Actual quantity.	Per cent.
			grammes.	grammes.	grammes.	grammes.	
1.	Wheat-meal...	Ignited pumice ...	2·0585	0·0370	0·0338	0·0032	8·51
2.	Wheat-meal...	Ignited soil	2·1282	0·0383	0·0335	0·0048	12·53
3.	Barley-meal...	Ignited pumice ...	2·2495	0·0380	0·0368	0·0012	3·16
4.	Barley-meal...	Ignited soil	2·0980	0·0355	0·0309	0·0046	12·96
5.	Bean-meal ...	Ignited pumice ...	2·0650	0·0803	0·0741	0·0062	7·72
6.	Bean-meal ...	Ignited soil	2·0800	0·0809	0·0823	(+0·0014)	+ (1·73)

The last two columns of this Table, which exhibit respectively the actual amount of Nitrogen not recoverable by the soda-lime process in the substance after decomposition, and the percentage proportion of this loss upon the Nitrogen submitted to experiment, are the most important to consider for our present purpose.

With one exception (the gain of Nitrogen in which is quite within the range of the error of analysis), all the experiments point to the fact, that a part of the Nitrogen of decomposing organic matter passes into a state in which it cannot be estimated by the soda-lime process. Neither did it exist as nitric acid. There appears, therefore, to be an evolution of free Nitrogen.

It is not a little remarkable, that although so large a proportion of the total Nitrogen present is lost, doubtless passing off as free Nitrogen, yet scarcely a trace of ammonia was given off from the mass; for the oxalic acid in the bulb-apparatus C was, in each case, separately rendered alkaline with caustic potash and distilled, the distillate being collected and examined quantitatively for ammonia, when, in only one case—that of the Bean-meal and Pumice—was there any ammonia indicated, and then only equal in amount to 0·0002 gramme Nitrogen. This was the case, notwithstanding that the Nitrogen in the mixtures amounted to from 0·03 to 0·08 per cent. of their entire quantity.

The questions here arise:—to what extent had the decomposition of the organic substance proceeded? what shall we accept as the measure of the amount of decomposition? what are the intermediate stages through which the substance has passed? what is the character of the organic compounds remaining in the mass? what is the nature of those that have been evolved? and what part does water play in the matter?

The subject of the character of the gradual changes which take place during the

decomposition of mixtures of nitrogenous and non-nitrogenous substances, in variable proportions, in connexion with soil and water, involves points so highly complicated, that we cannot pretend satisfactorily to answer all the above questions.

We may, however, ascertain the character of some of the final products of the decomposition, and from a knowledge of these draw conclusions as to the changes of which they are the result under various circumstances.

SERIES II.—*Experiments on the Decomposition of nitrogenous Organic Matter, made in* 1858.

The following series of experiments was made with a view to embrace a wider range of conditions as to degree of moisture;—to observe the different stages of decomposition as manifested by the odour, &c.;—to include the circumstances of sprouting, early growth, and subsequent decay of the products of the vegetation;—and to afford material for a more elaborate inquiry into the character of the products of the decomposition.

The results given above, in Table VII., do not show any difference between soil and pumice as matrix that we can safely refer to other than incidental causes independent of the action of the matrix itself. Yet we continue the use of the two substances, in order to see if, with a larger percentage of organic matter, and a more complete decomposition, the pumice will retain the ammonia formed as well as the soil.

About 175 to 200 grammes of soil, or 120 to 150 grammes of pumice, were used as matrix in each experiment, and the other conditions were as follow:—

Wheat.	*a.*	171 seeds, weighing 8·0475 grammes,	50 c. c. water, with ignited Soil.
	b.	171 seeds, weighing 8·0715 grammes,	100 c. c. water, with ignited Pumice.
	c.	Meal, weighing 9·8810 grammes,	40 c. c. water, with ignited Soil.
Barley.	*a.*	163 seeds, weighing 8·0440 grammes,	50 c. c. water, with ignited Soil.
	b.	163 seeds, weighing 8·1360 grammes,	100 c. c. water, with ignited Pumice.
	c.	Meal, weighing 8·9670 grammes,	40 c. c. water, with ignited Soil.
Bean.	*a.*	7 seeds, weighing 6·4700 grammes,	50 c. c. water, with ignited Soil.
	b.	7 seeds, weighing 5·7830 grammes,	50 c. c. water, with ignited Pumice.
	c.	Meal, weighing 6·1750 grammes,	40 c. c. water, with ignited Soil.

Those of the mixtures to which about 50 cub. cent. of water were added, were about as moist as soils when in a good condition for vegetable growth. Those with 40 cub. cent. were much drier in appearance, there being no tendency to agglutination of the particles. Those with 100 cub. cent. were very wet, there being some free water above the solid matters.

The seeds sown with 50 cub. cent. water showed growth in a few days after being put in, and the vessels (B, fig. 8, Plate XII.) were soon filled with a mass of vegetation. Those sown with double this quantity, or 100 cub. cent. water, showed no indications of sprouting; and in a few days, the odour evolved from them showed that decomposition had set in. The mixtures of meal and soil, also, soon gave odours indicative of

decomposition, though less foul than those from the whole seed and 100 cub. cent. water.

The following Notes, taken at different times during the experiments, will indicate the stages of growth, or decomposition, through which the several organic matters passed.

March 16.

Wheat (*a*)—*Seeds, in Soil with* 50 *cub. cent. water.*—Came up some days later than the corresponding Barley *a*; has not grown so rapidly; has kept green for a longer period; and is yet growing healthily, though much crowded in the small bottle. The air passing from the bottle has not the odour of decomposing organic matter. There is a slight mould on the soil due to a few seeds which did not grow.

Wheat (*b*)—*Seeds, in Pumice with* 100 *cub. cent. water.*—The Pumice in this case was covered with water to the depth of about one-fourth of an inch, and a few grains floated in the water. In a few days the air drawn through the bottle gave the odour and taste of decomposing organic matter. At the end of about a month the free water on the surface began to disappear rapidly, and in a short time it was all gone, leaving a grey mouldy coating of organic matter over the top of the pumice. This disappearance of water was too great to be due to simple evaporation in the air passed through the apparatus. It was doubtless consumed in the process of decomposition—a view which receives confirmation from our experiments on the nature of the gases evolved during decomposition.

Wheat (*c*)—*Meal, in Soil with* 40 *cub. cent. water.*—Gives little indication of decomposition by the air which passes from it. Compared with Wheat *b*, the difference in this respect is very marked.

Barley (*a*)—*Seeds, in Soil with* 50 *cub. cent. water* —Came up soon after being put in, grew rapidly, and in five weeks had grown to the top of the bottle, a height of about 5 inches. By the end of February the bottle was quite filled with green vegetable matter, and up to that time no odour of decomposition was distinguishable in the air which was passed through, but from that date the leaves became yellow, and decomposition has been manifested both by appearance and the taste of the air.

Barley (*b*)—*Seeds, in Pumice with* 100 *cub. cent. water.*—Progress almost exactly similar to that of the corresponding Wheat (*b*) described above.

Barley (*c*)—*Meal, in Soil with* 40 *cub. cent. water.*—Very like the corresponding Wheat (*c*) above.

Bean (*a*)—*Seeds, in Soil with* 50 *cub. cent. water.*—Came up a week after sowing. The sprouts pushed several seeds out of the soil, yet they have continued to grow up to the present time, lying upon the surface. At first there was a natural development of leaf and of roots; but soon the latter took a remarkable course, coming through the surface of the soil and extending through all parts of the bottle, mingling with the

stems and leaves, and forming a densely crowded mass of vegetable matter. The rootlets from the main branches extending through the mass commenced their growth in all directions indiscriminately, but after growing about one-fourth of an inch they invariably turned downwards.

Bean (*b*)—*Seeds, in Pumice with* 50 *cub. cent. water.*—Identical in appearance with the last (Bean *a*), excepting a little further developed.

Bean (*c*)—*Meal, in Soil with* 40 *cub. cent. water.*—Almost exactly like the Wheat (*c*) and Barley (*c*) meals, described above.

In no one of the above nine cases was there any Ozone reaction to test-paper.

April 28.

Wheat (*a*)—*Seeds, in Soil with* 50 *cub. cent. water.*—Twelve to fifteen stems; leaves not unrolled, and scarcely any tendency to expansion. The vegetation not nearly so much crowded as in the case of the corresponding Barley (*a*); yet most of the shoots show signs of dying. A thin coat of fungoid growth covers the stems to the height of from 1 to 1·5 inch. The stems are from 2 to 2·5 inches high, those of the corresponding Barley being from 3 to 4 and 5 inches high. The air passed through the apparatus is not disagreeable either in taste or odour.

Wheat (*b*)—*Seeds, in Pumice with* 100 *cub. cent. water.*—The Pumice moist, but without visible water, and the surface covered with a grey mouldy coating. The air has had an unpleasant odour ever since March 16, and now it is exceedingly nauseating.

Wheat (*c*)—*Meal, in Soil with* 40 *cub. cent. water.*—The soil apparently dry, but slightly mouldy, and the air passed over is almost without odour.

Barley (*a*)—*Seeds, in Soil with* 50 *cub. cent. water.*—The bottle full of vegetable matter, all quite yellow at the top where it touches the cork, and yellowish lower down. The plants covered with a coating of greyish fungus. The odour and taste of the air slightly disagreeable. The soil looks quite dry.

Barley (*b*)—*Seeds, in Pumice with* 100 *cub. cent. water.*—The soil is moist and mouldy. The mould on the surface appears to be decreasing, and is now less abundant than in the case of the corresponding Wheat (*b*). The odour of the air is much less disagreeable; indeed there is scarcely any at all.

Barley (*c*)—*Meal, in Soil with* 40 *cub. cent. water.*—The soil mouldy and apparently dry. The air from the vessel tasteless, and inodorous.

Bean (*a*)—*Seeds, in Soil with* 50 *cub. cent. water.*—Continued to grow vigorously for a long time, filling the bottle with a confused mass of stems, leaves, and roots, which has commenced to decay rapidly during the last two weeks. The upper portions of the mass are now entirely dead and black; but nearer the soil the stems and leaves are green and long, whilst healthy roots are intermingled with them. The soil is also tolerably filled with roots. The odour of the air is not disagreeable.

Bean (*b*)—*Seeds, in Pumice with* 50 *cub. cent. water.*—Very much like the last (Bean *a* with soil), excepting that the development of roots is scarcely so great.

Bean (*c*)—*Meal, in Soil with* 40 *cub. cent. water.*—A little mouldy matter on the surface of the soil, which appears dry.

July 1.

Wheat (*a*)—*Seeds, in Soil with* 50 *cub. cent. water.*—Plants all dead; the entire contents of the bottle apparently quite dry. The air has a slight musty odour.

Wheat (*b*)—*Seeds, in Pumice with* 100 *cub. cent. water.*—Odour rather more marked than that of the last (Wheat *a*); a coating of organic matter on the surface of the pumice.

Wheat (*c*)—*Meal, in Soil with* 40 *cub. cent. water.*—Soil quite dry; covered with mould; odour of air slight.

Barley (*a*)—*Seeds, in Soil with* 50 *cub. cent. water.*—Plants quite dead and dry; air inodorous.

Barley (*b*)—*Seeds, in Pumice with* 100 *cub. cent. water.*—Soil dry and covered with mould. Air like that of Wheat *b*; more foul than that of any of the others.

Barley (*c*)—*Meal, in Soil with* 40 *cub. cent. water.*—Surface dry. The air has a slightly musty odour.

Bean (*a*)—*Seeds, in Soil with* 50 *cub. cent. water.*—Plants all dead, and much decomposed; forming a black mouldy mass of organic matter on the surface of the apparently dry soil. The air has no perceptible odour.

Bean (*b*)—*Seeds, in Pumice with* 50 *cub. cent. water.*—The same as the last (Bean *a*).

Bean (*c*)—*Meal, in Soil with* 40 *cub. cent. water.*—Soil dry; slightly mouldy; the air from over it inodorous.

In order to see the effect upon the organic matter of an increased amount of moisture, 100 cub. cent. of water were added to each of the nine bottles of decomposing matter, at this date (July 1).

August 28.

Final Report, and termination of the Experiment.

Wheat (*a*)—*Seeds, in Soil with* 50 *cub. cent. water.*—Very little odour, and that not unpleasant. On removal from the bottle, it was found that the organic matter was well decomposed, only very indefinite remains of stems and leaves being visible in the soil. On the addition of oxalic acid to the mass, to retain the ammonia during the evaporation to dryness, a copious evolution of carbonic acid took place, and the surface of the fluid was constantly covered with a brown froth during the process.

Wheat (*b*)—*Seeds, in Pumice with* 100 *cub. cent. water.*—The mass has a disgusting mouldy odour. The form of the grain is retained, but all the contents are gone, and the

husk is filled with fluid. On evaporation with oxalic acid, there was evolution of carbonic acid, &c., as with the last; indeed it was the same with all those which follow.

Wheat (c)—Meal, in Soil with 40 *cub. cent. water.*—In this, as in all the other cases, owing to the water added on the 1st of July, the mass was covered to the depth of from $\frac{1}{4}$ to $\frac{1}{3}$ an inch with fluid. In both the above cases with Wheat, the supernatant water was colourless, but in this it had a dirty, muddy, yellowish colour. The mass emitted a foul disagreeable odour, though not so intense as that of the corresponding Barley.

Barley (a)—Seeds, in Soil with 50 *cub. cent. water.*—The organic matter thoroughly decomposed; stems, roots, and leaves no longer distinguishable in the soil; other conditions about as those with the corresponding Wheat *a*.

Barley (b)—Seeds, in Pumice with 100 *cub. cent. water.*—The pumice covered with a black coating of organic matter; supernatant water clear. The odour of the air above the mixture exceedingly disgusting, resembling that of decaying excrements; traces of sulphide of hydrogen perceptible. The form of the seeds is preserved, but the shell contains only fluid.

Barley (c)—Meal, in Soil with 40 *cub. cent. water.*—Supernatant water yellowish; odour musty, but not very disagreeable. Decomposition so complete that traces of organic matter are hardly perceptible.

Bean (a)—Seed, in Soil with 50 *cub. cent. water.*—The organic matter well decomposed. Odour musty.

Bean (b)—Seeds, in Pumice with 50 *cub. cent. water.*—Plants well decomposed; only very indefinite skeletons of stems, leaves, and roots remaining. Odour musty, but not disagreeable.

Bean (c)—Meal, in Soil with 40 *cub. cent. water.*—Supernatant water slightly yellow. Odour musty, but not offensive.

The last description, dated August 28, refers to the state of the respective masses just before being dried for analysis. After drying, any slight remains of organic matter had become brittle; and the substance, in every case excepting where 100 cub. cent. water had been added at the commencement, presented the appearance of clean soil or pumice, without organic matter. In the excepted cases the shell of the grain was still visible. If we take into consideration the amount of growth in several of the cases on April 28, it will be seen how great must have been the subsequent decomposition so entirely to get rid of the organic matter.

It is worthy of remark, that, in a few instances, the sulphuric acid in the bottle D, fig. 8, Plate I., became coloured slightly brown, indicating the passage into it, through the oxalic acid, of some carbon-compound more complicated than carbonic acid. In the course of other parts of our investigation, we have observed phenomena indicative of a similar result; but as we have not followed up the subject, we leave it with only this remark as to the fact of what we have observed.

The following Tables (VIII. and IX.) show the numerical results of the investigation now under consideration:—

TABLE VIII.—Showing the conditions provided in Experiments on the Decomposition of Nitrogenous Organic Matter; and the amount and proportion of the original *Carbon* of the substance remaining after the decomposition, or given off during the process.

Substances involved in the Experiment.				Weight of Organic Matter.		Carbon in Organic Matter.			
Organic Matter.		Matrix.	Water.	Fresh.	Dry.	Before Decomposition.	After Decomposition.	Loss in Decomposition.	
Description.	Condition.							Actual quantity.	Per cent.
			cub. cent.	grammes.	grammes.	grammes.	grammes.	grammes.	
1. Wheat	*a.* 171 seeds	Ignited soil	50	8·0475	6·7438	3·1089	0·9274	2·1815	70·17
	b. 171 seeds	Ignited pumice	100	8·0715	6·7639	3·1182	0·9178	2·2004	70·56
	c. Meal	Ignited soil	40	9·8810	8·2803	3·8172	1·3199	2·4973	65·42
2. Barley	*a.* 163 seeds	Ignited soil	50	8·0440	6·7127	3·0523	0·9598	2·0925	68·55
	b. 163 seeds	Ignited pumice	100	8·1360	6·7895	3·0872	1·1952	1·8920	61·28
	c. Meal	Ignited soil	40	8·9671	7·4830	3·4025	1·0995	2·3030	67·68
3. Beans	*a.* 7 seeds	Ignited soil	50	5·7830	4·5830	2·2915	0·8511	1·4404	62·86
	b. 7 seeds	Ignited pumice	50	6·4700	5·1275	2·5637			
	c. Meal	Ignited soil	40	6·1750	4·8937	2·4468	0·9778	1·4690	60·04

TABLE IX.—Showing the conditions provided in Experiments on the Decomposition of Nitrogenous Organic Matter; the amount and proportion of the original *Nitrogen* remaining after the decomposition, or given off during the process; together with the amount *evolved* as *Ammonia*, or remaining in the products *as such*.

Substances involved in the Experiment.				Total Nitrogen in Organic Matter.				Nitrogen in the form of Ammonia.			
Organic Matter.		Matrix.	Water.	Before Decomposition.	After Decomposition.	Loss (or Gain).		Total quantity.	Per cent.	Absorbed by Oxalic acid during Decomposition.	
Description.	Condition.					Actual quantity.	Per cent.			Actual quantity.	Per cent.
			cub. cent.	grammes.	grammes.	grammes.		grammes.		grammes.	
1. Wheat	*a.* 171 seeds	Ignited soil	50	0·1392	0·1398	+0·0006	+ 0·43	0·0429	30·83	·00038	0·273
	b. 171 seeds	Ignited pumice	100	0·1396	0·1214	0·0182	13·08	0·0573	41·06	·00092	0·614
	c. Meal	Ignited soil	40	0·1709	0·1680	0·0029	1·74	0·0197	11·49	·00040	0·234
2. Barley	*a.* 163 seeds	Ignited soil	50	0·1247	0·0746	0·0501	40·20	0·0157	12·64	·00055	0·441
	b. 163 seeds	Ignited pumice	100	0·1261	0·1052	0·0209	16·62	0·0294	23·39	·00092	0·016
	c. Meal	Ignited soil	40	0·1390	0·1311	0·0079	5·66	0·0166	11·97	·00039	0·280
3. Beans	*a.* 7 seeds	Ignited soil	50	0·2417	0·2107	0·0310	12·84	0·0140	57·91	·00341	1·424
	b. 7 seeds	Ignited pumice	50	0·2704	0·2380	0·0324	11·99	.....	.....	·00242	0·895
	c. Meal	Ignited soil	40	0·2581	0·2267	0·0314	12·16	0·1039	40·25	·00060	0·232

A comparison of the results in Tables VII. and IX. will show that they are confirmatory of each other as to their more general indications. Both series agree in the entire absence of any tangible relation between the varied circumstances of decomposition, and the products of that decomposition.

It is quite evident that, whilst in some instances there has been no evolution of Nitrogen, in others the amount of decomposition involving such evolution has been very great. Indeed, in some cases, the indication of the loss of Nitrogen in this way is so great that we could not have believed such a result possible had it not been attested by repeated analysis. But we have not been able to trace these differences to their ultimate causes.

The amount of decomposition, as indicated by the physical condition of the several substances at the termination of the experiment, as also by the proportion of carbon given off as shown in Table VIII., might lead to the conclusion that the process had gone about equally far, and attained about an equal completeness, in all the cases to which Tables VIII. and IX. refer. But here the equality of effect ceases. Thus, from 60 to 70 per cent. of the total carbon in the original organic matter has passed off; but the proportion of the original Nitrogen that is not recovered in the products varies, under the same circumstances, from 0 to 40 per cent. of it. The proportion of the Nitrogen of the original substance which was retained in the mass, or absorbed in the oxalic acid in the bulb-apparatus (C) in such form as to be given off as ammonia on distillation from a weak alkaline solution, and which probably existed, therefore, in the products as ammonia, ranged from 12 to 58 per cent. of the total quantity involved in the experiment. And, again, the proportion of the Nitrogen evolved from the mass as ammonia during the decomposition, and which was retained in the oxalic acid solution (C), varied from 0 to about 1·5 per cent. of the original or total Nitrogen.

If we attempt to trace a relation between the loss of carbon, the loss of nitrogen, the formation of ammonia, and the evolution of the small amounts of it during the decomposition, on the one hand, and the circumstances of matrix, moisture, growth, decay, &c., pointed out in the notes preceding the Tables, we fail to discover any connexion which we may with safety regard as exhibiting cause and consequence.

The most that we can venture to say is that, under a wide range of circumstances, a considerable loss of Nitrogen occurs in the decomposition of nitrogenous organic matter; that under particular, and apparently rather rare circumstances, this loss of Nitrogen does not occur; that the proportion of the Nitrogen taking, under the same circumstances, such form that it may be driven off as ammonia on the distillation of the products with a weak solution of alkali, varied from one-eighth to more than one-half of the total present; and that the amount of the Nitrogen evolved from the mass as ammonia during the process was quite inconsiderable.

These conclusions, though necessarily expressed in very general terms, have nevertheless a very important bearing on certain questions in practical agriculture. Whilst it would appear that there may be a very great loss of Nitrogen—a very important element in manure—under circumstances of decomposition of organic matter, closely allied to those to which, in practice, nitrogenous organic manures are subject, it is at the same time indicated that it is possible for such matters to pass through the process of decomposition without such loss. The importance of further investigation is hence

suggested, to ascertain the causes of the difference of effect, in order, if possible, to control them. The results also point to the insignificance of the loss of Nitrogen in the form of ammonia, a supposed evil to which the attention of agricultural chemists has specially been directed in order to find means of preventing it, though nothing has as yet been done to avoid the loss, in apparently much larger quantity, of free Nitrogen. But as these questions are more appropriate for consideration in a purely agricultural paper, we shall not follow them further in this place.

Other investigations, to which we have to call attention, will throw some light upon the character of the molecular forces by which the decomposition of nitrogenous organic compounds is effected under such circumstances as we have been considering. These forces might be one or both of two kinds.

1. They might be of an oxidizing character, analogous to that of the action of chlorine upon ammonia by which free Nitrogen is evolved.

2. They might be of a reducing character, similar to that of a great number of substances upon the oxygen-compounds of Nitrogen, by which the oxygen of the latter is appropriated, and free Nitrogen given off.

3. These two actions may operate in succession the one to the other.

It is well known that an oxidizing action may be so intense as to deprive a nitrogenous organic compound of all its carbon and hydrogen, converting it into oxygen compounds, as is done by permanganic acid. The converse action of the transformation of oxygen-compounds of Nitrogen into ammonia is also very well known. An intermediate stage in either of these converse actions may give free Nitrogen.

There can be little doubt that the Nitrogen in the organic substances which we have submitted to decomposition existed in them in a condition more analogous to a hydrogen than to an oxygen compound of it. The able researches of HOFMANN into the nature of compounds formed upon the ammonia type, would lead us to suppose that the Nitrogen compounds upon which we have been operating are of the ammonia class. They are more difficult to oxidize into nitric acid than is ammonia; yet their transition into ammonia is so easy, that it is effected in almost all the chemical changes to which they are ordinarily subjected. And, since ammonias yield free Nitrogen under the influence of oxidizing forces, it may be inferred that it has been under the influence of such forces that Nitrogen has been set free in the cases recorded above. PELOUZE has remarked* that salts of nitric acid are converted into ammonia, in contact with decomposing organic matter. Experiments of our own have shown that, during the decomposition of organic matters in contact with nitrates, free Nitrogen is not evolved. The evolution of free Nitrogen in the experiments quoted above must, therefore, be referred to the action of oxidizing forces.

The experiments next referred to bear upon these points.

* Comptes Rendus, xliv. p. 118.

E.—*Experiments on the action of the oxidizing and reducing forces, as manifested in the decomposition of organic matters containing Nitrogen.*

Several qualitative experiments showed that when cereal grains and leguminous seeds were placed in water, over mercury, an evolution of gas took place, after about thirty-six to forty-eight hours. This went on rapidly for a week or two, after which all action appeared to cease, no more gas being evolved. The total quantity of gas evolved varied between 20 and 50 cub. cent. from 3 to 4 grammes of the seeds. An examination of the gas proved it to be almost entirely carbonic acid and hydrogen, the quantity of Nitrogen being very small.

To examine this action more thoroughly, about half a pound of a mixture of Wheat, Barley, and Beans was taken, put into a long narrow glass vessel (fig. 7, Plate XII.) of about 500 cub. cent. capacity, which was then filled with well-boiled water, and closed with a cork, through which two glass tubes (*a* and *b*) passed. The external ends of these tubes were fitted with caoutchouc tubing, for closing with pinch-cocks, or connexion with the Torricellian exhauster as described at p. 487. One of the tubes being so connected with the exhauster, it was allowed so to remain for several hours, in order to remove all the gaseous Nitrogen from the seeds. The vessel was then inverted in mercury, with one of the tubes (*b*) open under that fluid, and the whole placed in sunlight to favour the decomposition. This was done on the 28th of August, 1858. The seeds commenced swelling very soon, and on the 30th of August well-marked decomposition had set in.

On September 13th the vessel was about two-thirds full of gas, the displaced water having passed out through the quicksilver. Part of the seed was now above the water, in the gas, which commenced bubbling out through the tube (*b*). The arrangement was allowed so to remain until October 5, when 400 cub. cent. of gas were collected, of which the percentage composition was as follows:—

	Carbonic acid.	Hydrogen.	Nitrogen.
Experiment 1	64·87	34·83	0·30
Experiment 2	64·54	35·46	traces.

The quantity of the gas evolved points to the extent of the decomposition; the amount of carbonic acid and hydrogen shows how great must have been the reducing force exerted; and the small quantity of Nitrogen, which was probably due to accident, indicates that free Nitrogen was not a product of the action.

The vessel was again filled with boiled water, again connected for some time with the Torricellian exhauster, and again placed in its former position in the sunlight.

October 9.—A small bubble of gas collected in the top of the vessel.

November 3.—Only a few bubbles of gas at the top of the vessel.

November 17.—The vessel was removed into the laboratory and placed in a room, the temperature of which varied from a few degrees above the freezing-point to about 24° C.

December 1.—Very little gas evolved.

December 12.—The gas collected without exhaustion measured only 6·1 cub. cent., of

which 4·6 were absorbed by potash, and the remainder proved to be combustible. Hence, up to this date, there has been no appreciable evolution of free Nitrogen. In order to see whether the organic matter present would reduce a nitrate, with the evolution of free Nitrogen, about 5 grammes of saltpetre were now put into the vessel, and it was replaced in the same room as before.

May 3, 1859.—Several times since December 12, 1858, when the nitrate of potash was put in, the vessel has been warmed up to 30° C.; but up to this date very little gas has been evolved.

May 25, 1859.—Still very little gas evolved; 4 cub. cent. only collected, one-fourth of which was carbonic acid, and the remainder was combustible. The vessel was now placed in the sunlight again, but up to the middle of June no further evolution of gas had taken place. The fluid still contained nitrate of potash. The vessel was then half filled with oxygen in order to see if this would cause a renewal of the decomposition. After ten days a portion of the gas was examined, when it was found that not one-fourth of the supplied oxygen had been consumed—a result which was quite unexpected. The total gas being removed, the vessel was again nearly filled with oxygen, driving out the greater part of the fluid, and leaving the partly decomposed seeds in an atmosphere of this gas. The apparatus so arranged was placed in the sunlight, and remained there during some very warm weather.

July 12, 1859.—The gas collected contained in 100 parts—

Carbonic acid.	Oxygen.	Nitrogen.
20	79	1

By accident a small quantity of air was admitted into the vessel, so that the analysis can only be taken to show how exceedingly slow was the oxidation of organic matter which had been treated as this had been.

On the removal of the matters from the vessel, the Beans were found to possess much of their original firmness and solidity. The other seeds, though they retained their form, were softer, and they had evidently undergone a more complete decomposition. They emitted very little odour, which was not unpleasant.

There can be no question as to the absence of any evolution of free Nitrogen during the long period that these three descriptions of seed were under experiment. A very small proportion of the combined Nitrogen present would, if set free, have been sufficient to fill the vessel with gas. But, as has been seen, only a few bubbles of gas were evolved during several months.

Several other experiments were made upon the products of the decomposition of organic matter, in the first stages of the process. In Table X., which follows, are given the amounts, and the composition, of the gas obtained from decomposing organic matter in a few out of a number of cases in which we have had occasion to observe them—including, for comparison, some of the results already referred to. The decomposition took place in water, in vessels similar to that used in the experiments last described (fig. 7, Plate XII.).

TABLE X.—Showing the products of the action of the reducing forces exercised in the decomposition of Nitrogenous organic matter, as exhibited by the composition of the gases evolved.

Description of Organic matter subjected to Decomposition.	Total Gas collected.	Composition of the Gas, Per Cent.		
		Carbonic acid.	Hydrogen.	Nitrogen.
	cub. cent.			
a. Wheat, Barley, and Bean seed ...	400	64·87	34·83	0·30
		64·54	35·46	traces.
b. Turnip plant; root with leaves ...	166·2	76·23	22·91	0·87
c. Turnip plant; root with leaves ...	162·2	68·83	23·93	7·24
d. Turnip plant; root with leaves ...	123·6	68·06	25·63	6·31
		67·52	25·43	7·05
e. Turnip plant; root with leaves ...	41·2	64·95	14·66	20·39

The first experiment (*a*) is that which has been considered above. In all the other cases about two ounces of young Turnip Plant, the root and leaves together, were operated upon. They were exposed in similar vessels to those used in the other experiments, from August 29 to October 5. At the termination of this period the structure of the plant was almost entirely destroyed; and there remained only a mass of decomposed matter deposited at the bottom of the vessels. The evolution of gas had entirely ceased.

The Turnip plant (*b*) was exhausted of its gas before exposure; and, as will be seen, there was, under these circumstances, a very small quantity of free Nitrogen found at the termination of the experiment.

All the other Turnip plants were submitted to decomposition without previous exhaustion; and hence the amount of Nitrogen eventually found. In the last experiment (*e*) there is a much larger percentage of Nitrogen than in the other cases. But the total quantity of gas was much less; and the comparison of this result with the others shows that there was an almost constant actual quantity of Nitrogen in the several cases, doubtless due to that existing within the plant at the commencement of the experiment. Hence it appears that, in the absence of free oxygen, no free Nitrogen is evolved from the nitrogenous compounds of the plant.

At all events the entire cessation of the evolution of gas after the decomposition has gone on for a few days, shows that the presence of free oxygen is essential to the evolution of Nitrogen, as it is conducive to that of carbonic acid. The loss of Nitrogen indicated in Tables VII. and IX. must be considered, therefore, to be the result of an oxidizing process.

We shall have to allude again to the results given in Table X. when we come to discuss the question of the formation of ammonia from the free Nitrogen of the air, and the nascent hydrogen evolved during the decomposition of organic matter.

In order to examine the character of the decomposition of organic matter in oxygen gas, an investigation was undertaken, which, owing to the difficulty of getting the requi-

site apparatus sufficiently air-tight, was not followed up to the extent which had been intended.

The plan proposed was, to place the organic matter in an atmosphere of pure oxygen, and to afford a constant supply of the gas as it became converted into carbonic acid, and was absorbed by a solution of caustic potash.

The results obtained go to show that, in the presence of free oxygen, Nitrogen gas is evolved. But as the investigation is as yet so incomplete, owing to the circumstance above alluded to, we prefer not to give the results until we can confirm them by a more extended series of experiments under more favourable conditions.

Taking together the results of all the experiments which have been made upon the decomposition of nitrogenous organic matters, they obviously point to a serious difficulty in the way of experiments made upon the question of the assimilation of free Nitrogen by plants. It is not possible to conduct any such experiments without exposing nitrogenous organic matter to conditions more or less analogous to those under which the loss of Nitrogen recorded in Tables VII. and IX. took place. For although, as Boussingault has shown, there may be no loss of Nitrogen during germination, yet, during the entire period of the growth of a plant, certain portions of the vegetable substance may be subjected to conditions favourable to the decomposition of its nitrogenous compounds, and to the evolution of free Nitrogen.

As illustrative of how far these conditions are likely to be operative in the manner indicated, the following results, made with Wheat, Barley, and Oats respectively, are very instructive. Seeds of the three plants were sown, each in precisely the same kind and amount of soil, &c., as employed in the experiments on the assimilation question. The three pots were placed beneath a large glass shade, 16 inches in diameter, which fitted into the groove of a stone-ware lute-vessel, into which sulphuric acid was poured to exclude the access of external air. The whole stood on a table in the diffused light of the laboratory. The plants were at first supplied with distilled water; but with no carbonic acid beyond that which might be contained in the water. These conditions afforded all that was necessary for germination and growth, with little opportunity for the assimilation of free Nitrogen, even were this possible in the more favourable conditions of sunlight. Yet the conditions were more than ordinarily favourable to the decomposition of nitrogenous compounds, provided this would take place, under certain circumstances, during the growth of the plant. The succulent character of the stems and leaves so grown in the shade, would render the nitrogenous matters more liable to decomposition than in the case of the more firm and hardened stems of plants grown in sunlight.

Eight seeds of each plant were sown; and in a few days all came up, and grew very rapidly in height, without much tendency to development and expansion of leaf. The plants were all very much alike—tall, slender, delicate, and having the peculiar pale-green colour common to plants deprived of sufficient sunlight. In several other expe-

riments it was found that plants which had proceeded for some time in this delicate form of growth, immediately ceased this predominant upward tendency when removed into sunlight: then, after remaining stationary for a few days, during which time the extremities of the long delicate leaves lost their vitality, the plants commenced a new order of growth, producing many more leaves, which were much shorter and broader than the earlier ones; the stems also became thicker and more dense than before.

The seeds were put in on May 17 (1858); and on June 10 following, the plants had ceased to grow. Several of the long slender stems were too delicate to support themselves, and began to fall over. All the plants presented much the same appearance, each with a small sheath without any leaf at the base, and three leaves higher up—the two lateral ones being very long, from 8 to 12 inches, and the terminal ones, not unrolled, from 3 to 4 inches long from the axial of the next leaf below, the whole plant being from 7 to 11 or 12 inches high. On removing them from the soil, it was found that the roots were distributed very little through it. They consisted of short fibrils, with divaricated branchlets, extending principally around the seeds, and seldom more than 2 or 3 inches through the soil. The plants were so very much alike, that it was difficult to distinguish the different kinds. Fig. 9, Plate XII., is reduced from a sketch of one of these attenuated plants.

The following Table gives the quantitative particulars of the experiments.

TABLE XI.—Showing the effect of Germination, and Growth without direct Sun-light, or extraneous supply of Carbonic Acid or combined Nitrogen, upon the combined Nitrogen originally provided in the Seed.

Duration of experiment twenty-four days—from May 17 to June 10, 1858.

Particulars of the seed sown.					Dry vegetable matter produced.			Nitrogen.			
Description.	No.	Weight, fresh.	Weight, dry.	Nitrogen.	Stems.	Roots.	Total.	In stems and roots.	In soil and pot.	In total products.	Gain or loss.
		gramme.	gramme.	gramme.	gramme.	gramme.	gramme.	gramme.	gramme.	gramme.	gramme.
Wheat	8	0·4865	0·4077	0·00790	0·320	0·140	0·460	0·00697	0·0012	0·00817	+0·00027
Barley	8	0·3875	0·3234	0·00573	0·290	0·160	0·450	0·00570	traces	0·00570	−0·00003
Oats	8	0·3475	0·2900	0·00640	0·355	0·060	0·415	0·00640	traces	0·00640	+ traces

The weights given for the roots are a little too high, owing to their not having been washed entirely free from soil, the principal object being to ensure a correct result with regard to the Nitrogen which long washing might have endangered, or at least rendered less easy. There is, however, evidently a slight gain of dry matter, which, so far as its carbon is concerned, was doubtless due to carbonic acid in the distilled water, of which about 500 cub. cent. were added to each pot at the commencement of the experiment. None was added during the progress of the experiment; but the soil was moist when the plants were taken up.

The rapid growth of the plants, the short period of their contact with the soil, the

very limited distribution of the roots, and the fact that no water was added during growth, which would tend to distribute any soluble or otherwise easily transportable matters, are conditions all consistent with the almost total absence of Nitrogen in the soil.

Lastly in regard to the results in the Table (XI.), the final column, showing the gain or loss of Nitrogen, affords us the means of judging how far the molecular actions by which free Nitrogen was given off in the cases of the experiments upon the decomposition of nitrogenous organic matter are likely to interfere with the results of our investigations on the question of assimilation of free Nitrogen by plants. It is seen that, in the experiments now under consideration, no free Nitrogen was given off during the process of germination and growth. At least, the assumption that free Nitrogen was given off implies the still more improbable one, that, under the circumstances detailed, assimilation of free Nitrogen has taken place; whilst the adoption of these two assumptions necessitates the yet more improbable one, that these two independent actions bear a most definite relation to each other—in fact, that the amount of free Nitrogen assimilated is exactly equal to that given off during decomposition.

It would appear, therefore, that we may rest satisfied that our results in regard to the question of assimilation will not be affected by a loss of free Nitrogen as the result of the decomposition of nitrogenous organic matter, so long as that matter is subjected to the ordinary process of germination, and exhaustion to supply materials for growth. Our results in regard to the products of decomposition of nitrogenous organic matter do, indeed, point to the danger of using nitrogenous organic manure in such experiments, and to the error that might occur from seeds decomposing in the soil instead of growing, or from the decomposition of dead leaves, of old roots, or of nitrogenous organic excretions; but they do not afford any evidence of what takes place within the range of the action of the living plant. And, judging from the amount of free Nitrogen evolved when, as in the experiments on decomposition, so large a proportion of the nitrogenous organic matter was decomposed, we may form some idea of the probable extent of such evolution when, as in experiments where vegetable growth is involved, and where the only nitrogenous organic matter supplied is that in the seed sown, but a small proportion of the total nitrogenous matter undergoes decomposition.

In relation to this question, it should be borne in mind that, in the cases where the large evolution of free Nitrogen took place, the organic substances were subjected to decomposition for a period of about six months, during which time they lost three-fourths of their carbon. In the experiments on the question of the assimilation of free Nitrogen, however, but a very small proportion of the total organic matter is subjected to decomposing actions apart from those associated with growth, and this for a comparatively short period of time, at the termination of which the organic form is retained, and therefore but little carbon is lost. It would appear, then, that we need not fear any serious error in our experiments in regard to the latter question, arising from the evolution of free Nitrogen in the decomposition of the nitrogenous organic matters

involved. On the other hand, the facts adduced afford a probable explanation of any small loss of Nitrogen which may occur when seeds have not grown, or when leaves, or other dead matter, have suffered partial decomposition.

F.—*The mutual relations of Gaseous Nitrogen and the Nascent Hydrogen evolved during the decomposition of organic matter.*

The importance attached by MULDER, and others after him, to the action of nascent hydrogen, evolved in the decomposition of organic matter, upon gaseous Nitrogen, as a source of ammonia, is such as to require that we should refer to the subject here, in the course of the discussion of the conditions possibly affecting the supply of combined Nitrogen to our experimental plants. The results given in the last sub-section (pp. 509–511), leave no doubt of the evolution of hydrogen during the decomposition of organic matter. They suggest, therefore, the possibility that such an evolution may take place in any decomposition of organic matter involved in our experiments on the assimilation of free Nitrogen by plants, and hence prove a source of ammonia to them.

That nascent hydrogen may, under certain circumstances, combine with gaseous Nitrogen, has long been admitted. But the view so prominently put forth by MULDER*, and some others, that those circumstances occur in the evolution of nascent hydrogen accompanying the decomposition of organic matter, requires confirmation.

If only a very small part of the hydrogen evolved in the decomposition of organic matter were to form ammonia with the Nitrogen gas which must always be in most intimate contact with it, the amount of ammonia formed in this way would be enormous. Peat bogs, cesspools, and all stagnant water pregnant with organic matter, as well as many soils, would be constantly so accumulating ammonia. The extensive forests in different parts of the world, which have been annually depositing a coating of leaves upon the surface of the soil for thousands of years, must also have been a very fertile source of ammonia, as the leaves have gradually decayed under the influence of moisture and confined air beneath the succeeding layers. And when we contemplate the amount of decomposition that must have corresponded to the very exuberant growth of former geological periods, as manifested in the remains exhibited in our coal beds and limestones, we see a source of ammonia, if formed in the manner now under consideration, which would be incalculable.

The results given in the last subsection (E), upon the decomposition of nitrogenous organic matter, favour the view that the hydrogen evolved in such decomposition does not form ammonia with the Nitrogen of the air. The assumption that it did so, implies that the nascent hydrogen was capable of uniting with free gaseous Nitrogen (forming ammonia) under circumstances in which its affinities were not sufficiently powerful to prevent Nitrogen compounds very similar to ammonias (and which are easily transformed into them) from giving up Nitrogen in the free state. It implies also, that the nascent hydrogen can act upon ordinary Nitrogen, when it cannot do so upon this nascent Nitro-

* Chemistry of Vegetable and Animal Physiology, pp. 111–114, 149–152, &c.

gen of the decomposing nitrogenous body. Or, if it did act upon the latter in preference to the former, there would either be no free Nitrogen finally evolved, or, in case of Nitrogen being lost in the free state, it would be obvious that there had been less nascent Nitrogen converted into ammonia than had been liberated from its combinations, and hence that, as a resultant, there would be a *loss* and *not a gain* of combined Nitrogen due to the decomposition.

The fact that, in our experiments upon the gas evolved by vegetable matters in a state of decomposition, both free Nitrogen and free hydrogen were given off, bears strongly upon this question. The Nitrogen evolved has been in most intimate contact with the hydrogen given off. It has, indeed, been in the identical cells by the decomposition of the walls or contents of which the hydrogen was set free; yet both appear as gas.

From the above considerations it would appear that we need be under little apprehension of error in the results of our experiments on the question of the assimilation of free Nitrogen by plants, arising from an unaccounted supply of ammonia formed under the influence of nascent hydrogen, given off in any decomposition of the organic matter involved in the experiment.

Summary Statement of the Results of the foregoing consideration of the conditions required, or involved, in Experiments on the question of the assimilation of free Nitrogen by Plants.

Before entering upon the discussion of the results of our direct experiments upon the question whether or not plants assimilate free Nitrogen, it will be well, for the sake of perspicuity, to give a very brief enumeration of the results arrived at in the foregoing Sections I. and II. (Part II.), relating to the conditions of experiment required, and to the collateral investigations involved, in the inquiry. They may be stated as follow:—

1. Conditions of soil or matrix which are both adapted for healthy growth and are consistent with the other requirements of the investigation can be attained (Section I. Sub-sections A, p. 470, and L, p. 484).

2. The requirements of the experiment in regard to the selection of seeds or plants for growth, to the nutriment to be supplied in the soil, to the water, to the atmosphere, to the carbonic acid, and to other conditions involved, can be satisfactorily met (Section I. Sub-sections B–J, inclusive, pp. 472–481; and L, p. 484).

3. The conditions of experiment adopted have several advantages over some of those which have been suggested, or adopted, by others (Section I. Sub-section K, pp. 481–483).

4. The mutual actions of the soil, air, organic matter in the soil or in the plant, are not such as to be likely to affect the result of the experiment, by yielding to the plants a quantity of combined Nitrogen not taken into account. The influence of Ozone as a possible element in these actions would be less, in the circumstances of the experiments

on assimilation, than in those of experiments the results of which showed no appreciable formation of compounds of Nitrogen (Section II. Sub-sections A–C, pp. 484–497).

5. The fact of the evolution of free Nitrogen during the decomposition of nitrogenous organic matter has been confirmed by experiment; but the circumstances of the decomposition in which the evolution of free Nitrogen was observed, when compared with those involved in an experiment on the question of assimilation, are not such as to lead to the conclusion that there would be a loss of Nitrogen from this source in experiments of the latter kind, unless in certain exceptional cases, in which it might be presupposed (Section II. Sub-section D, pp. 497–508).

6. The forces, by virtue of which free Nitrogen is eliminated from its compounds in organic matter, are of an oxidizing character; they are not exercised in the absence of oxygen. They are not likely to be operative in connexion with growing vegetable matter (Section II. Sub-section E, pp. 950, 951).

7. Although it is known that, under certain circumstances, nascent hydrogen may combine with free Nitrogen and form ammonia, it is questionable whether the nascent hydrogen eliminated during the decomposition of vegetable matter will be in the conditions to effect such a combination; nor are the circumstances of our experiments on the question of the assimilation of free Nitrogen by plants such as to lead to the supposition, that an error in the results can arise from the formation of any ammonia under the influence of the action supposed (Section II. Sub-section F, pp. 515, 516).

Section III.—CONDITIONS OF GROWTH UNDER WHICH ASSIMILATION OF FREE NITROGEN BY PLANTS IS MOST LIKELY TO TAKE PLACE; DIRECT EXPERIMENTS UPON THE QUESTION UNDER VARIOUS CIRCUMSTANCES OF GROWTH.

A.—*General consideration of conditions of growth.*

We have thus far discussed, in some detail, the arrangement adopted in our experiments on the question of the assimilation of free Nitrogen by plants, and the collateral points involved in the relation of gaseous Nitrogen to vegetation. In regard to the latter, we have dwelt particularly on those which relate to the sources of available Nitrogen to plants, and which, therefore, may tend to influence the quantitative results which we may obtain by the methods of experimenting followed. It remains to consider what are the circumstances under which it is most probable that free Nitrogen may be assimilated, provided the assimilation can take place at all.

The demonstration of the fact, that the process of cell-development could go on in the presence of free Nitrogen without the latter becoming incorporated into the cell wall, or into the contents of the cell, as a nitrogenous compound, would not carry with it the demonstration that free Nitrogen could, under no conditions of growth, undergo such change. Our aim should be, therefore, to seek the most probable circumstances

for such change; and if we find that in them free Nitrogen is assimilated, we should then trace up the question through the circumstances in which such assimilation is less likely to take place.

If, on the contrary, we find that free Nitrogen is not assimilated under the circumstances which appear the most favourable for such an action, we may either generalize for other conditions from the negative results so obtained, or we may extend our experiments in order to widen the basis of our generalizations.

In the consideration of what are the cases in which the assimilation of free Nitrogen is most likely to take place, two important classes of conditions present themselves:—

1. Those which relate to the supply of combined Nitrogen at the disposal of the plant.

2. Those which relate to the activity of growth and stage of development of the plant.

These two questions, though logically distinct, are physiologically blended; for it may happen that a certain activity of growth, or certain stages of development, can only be attained by a given supply of combined Nitrogen beyond that contained in the seed.

If we examine these conditions a little more closely, we see that they give us the following possible cases for the assimilation of free Nitrogen by the plant:—

1. The plant may be able, in the process of cell-formation, to derive the whole of its Nitrogen from that presented to it in the free state.

2. It may be capable of assimilating a part of its Nitrogen from that presented to it in the free state, provided it be supplied with only a part of its required amount in some form of combination.

3. It may assimilate free Nitrogen in the presence of an excess of combined Nitrogen.

Again:—

1. It may be capable of assimilating free Nitrogen in the earlier stages of its development.

2. It may be so at the most active period of its growth.

3. It may when near the period of its maturity.

Combinations of these several circumstances present at least nine special cases, in one of which, if at all, an assimilation of free Nitrogen might take place without its doing so in any of the others. The question arises, how are we so to arrange our experiments as to include the greatest number of these cases, and those in which the assimilation of free Nitrogen is the most likely to occur?

The obviously most probable circumstances for the assimilation of free Nitrogen at any stage of development of the plant, are those in which it is brought to that stage in a healthy condition, and then deprived of all sources of combined Nitrogen. It is hardly to be supposed that an assimilation of free Nitrogen would take place if there were an excess of combined Nitrogen at the disposal of the plant; for, if we suppose that the molecular and vital forces are at the same time acting upon Nitrogen supplied by these two sources, in a manner tending to force that from both into the constitution

of the living organism, it is only consistent with our established notions of force, that the form which yields with the greatest ease will yield first, and that, if its supplies be in sufficient quantity, it only will yield in an appreciable degree to the force applied.

If, on the other hand, the forces involved in vegetable growth, tending to form nitrogenous compounds, are capable of appropriating free Nitrogen only in the presence of a certain amount of assimilable combined Nitrogen, then the question of deciding upon the proper proportion of combined Nitrogen to effect the assimilation of that provided in the free state would seem, *à priori*, to present serious difficulty. For if the plant cannot assimilate free Nitrogen either in the presence of an excess of combined Nitrogen, or without the aid of a certain amount of it, it would, at first sight, appear that there might be some difficulty in so arranging an experiment as to hit the proper medium.

But within a certain range of conditions this supposed difficulty would not occur. If the assimilation of free Nitrogen be possible only as the result of the assimilating forces acting upon it in the presence, or with the aid, of a certain amount of combined Nitrogen, then, when the quantity of combined Nitrogen has become too small, the point must have been passed at which the maximum amount of free Nitrogen would be assimilated in relation to the then existing supply of combined Nitrogen. Hence, the analysis of a plant at the period at which its growth ceased in consequence of the falling short of the relative supply of Nitrogen in the combined form, would show whether or not an assimilation of free Nitrogen had taken place as the result of either of the conditions referred to in the last paragraph.

If, however, the plant cannot assimilate free Nitrogen under the conditions of the supply of combined Nitrogen just referred to, unless it has attained a certain vigour of growth, or reached a certain stage of its development, and the supply of combined Nitrogen has been insufficient to bring it to the supposed requisite point, then no assimilation of Nitrogen would take place, even though it might do so provided the proper stage of growth had been passed. To the cases here supposed we shall recur further on.

If the assimilation of free Nitrogen can take place at all periods of the growth of the plant, and in the absence of all sources of combined Nitrogen, the solution of our question becomes much more simple than in either of the cases above referred to.

In illustration of the fact that, within a certain range of other conditions, there can be no difficulty in securing in an experiment those involved in the presence of an excess, of a certain limited quantity, or of no combined Nitrogen, attention may be directed to the phenomena of vegetable growth when seeds are grown in a soil and atmosphere free from combined Nitrogen.

Under the circumstances supposed, all the conditions with regard simply to the relative quantity of combined Nitrogen are afforded. Thus, when the seed is first sown, it contains within itself an excess of combined Nitrogen, so far as the demands of the plant at the time are concerned. The rapidity with which the Nitrogen of the seed can

be used, in the growing process, is seen in the results of the experiment in regard to the question of the decomposition of Nitrogenous matter during growth, as given in Table XI. (p. 513); and the extent to which it can carry the growth of the plant is illustrated in that experiment, as well as in others, to which we shall presently refer, relating to the question of assimilation itself. It is obvious that, during a part of the time at the end of which the plant has reached the limit of its supply of combined Nitrogen, it has had at its disposal an excess of combined Nitrogen for its immediate wants. It has then passed through a stage in which the particular relation of combined to free Nitrogen implied in another of our assumed conditions must have existed. It must finally have reached a point at which only free Nitrogen was presented to it.

If an analysis of the plant at the termination of the last-mentioned period showed no increase of Nitrogen, the result would afford conclusive evidence against the possibility of the assimilation of free Nitrogen under a wide range of conditions. If, on the contrary, a gain of Nitrogen were indicated, the question would still be open, to which of the several conditions to which the plant had been subjected it owed the increase found. But this question we need not discuss until we have recorded the results of our experiments on the point.

B.—*Direct experiments on the question of the assimilation of free Nitrogen by plants.*

We have thus far discussed the methods of experimenting to be adopted, the results of certain collateral inquiries, and the several conditions under which the assimilation of free Nitrogen by plants may be the more or the less likely to take place. We have thus endeavoured to eliminate all known sources of error, and to acquire the means of forming an estimate of the possible influence of certain unknown quantities, and so, as far as practicable, to reduce the solution of our question to that of a single point to be tested by direct experiment. It remains to consider the experimental evidence relating to this last and final point.

An investigation requiring several hundred analyses, and a series of observations made at intervals of a few days, through periods of several months, involves an amount of recorded detail much too voluminous for full publication. An abstract of the most important portions of the records will, however, be given for reference in the Appendix. A statement of the methods of analysis adopted, with illustrations of the limits of accuracy reached, together with a condensed summary of the details of growth of the plants, will there be given.

In the selection of the plants to submit to our adopted conditions of experiment, we have been guided by several considerations:—

1. To have such as would be adapted to the conditions of temperature, moisture, &c., to which they were to be subjected.

2. To have such as were of importance in an agricultural point of view.

3. To acquire the means of studying any difference, in reference to the point in question, between plants which belong respectively to the two great Natural Orders

the Graminaceæ and the Leguminosæ, which, in some points of view, appear to differ so widely in their demands upon combined Nitrogen provided within the soil.

4. To take such as had already been experimented upon, with such conflicting results, by M. Boussingault and M. G. Ville.

We shall first consider the results obtained with plants grown without any other supply of combined Nitrogen than that contained in the seed sown.

I.—*Experiments in which the plants had no other supply of combined Nitrogen than that contained in the seed sown.*

The following Table (XII.) gives, at one view, a summary of the numerical results obtained under this head; see also figs. 1–6, Plate XV., which are reduced from careful drawings taken of six out of the nine Graminaceæ experimented upon, and illustrate the character and extent of growth attained under the conditions in question.

After the full discussion in the foregoing pages of the circumstances under which the results recorded in the Table just given were obtained, but little need be said in pointing out their bearings upon the question at issue. The column showing the gain or loss in each experiment speaks for itself. In judging of the results of the experiments of 1857, the remarks made in discussing the results of Table XIV. (p. 532), with regard to the slates used as lute-vessels in that year, must be taken into consideration. The source of error referred to being obviated in the experiments of 1858, the results of 1857 acquire a greater value, as confirming those of the latter year, than, standing alone, they would possess.

The difference between the results obtained with soil and with pumice as matrix, in 1857, are not such as to lead us to attach any importance to them, or to attribute them in any way to the difference of matrix in question. The two experiments may therefore simply be considered as duplicates. Indeed, the character of the results in the one experiment with Wheat, and in the two with Barley, in 1857, was so similar, that the three experiments may be considered as triplicates.

Graminaceous Plants.

It will be observed that the largest gain of Nitrogen in the three experiments with Graminaceæ in 1857 was 0·0026 gramme. Keeping in view the probable source of error due to the use of slates in that year, and the difference of result in 1858 when slates were not employed, and, again, considering the fact that so small an amount of Nitrogen had to be determined in such a large amount of soil (0·003 gramme or less of Nitrogen in about 1500 grammes of soil), it seems indeed more than questionable whether the gain should not be attributed to the errors of experiment or analysis alluded to. In fact, we can but conclude that, under the circumstances of growth of the Graminaceous plants to which Table XII. relates, there has been no assimilation of free Nitrogen.

It should also be noticed that, even when a gain of Nitrogen in the total products is observed, there is, in no case, more Nitrogen in the plant itself than in the original

TABLE XII.—Showing the Numerical Results of Experiments to determine whether Plants supplied with no other combined Nitrogen than that contained in the original seed assimilate Free or uncombined Nitrogen.

General particulars of the Experiments.				Numerical particulars of the seeds sown.					Numerical results at the termination of the Experiments.					Summary.		
				Number.		Weights.			Dry weights of Plants produced.	Nitrogen.				Nitrogen in seeds sown.	Nitrogen in total products.	Gain or loss of combined Nitrogen.
Year, &c.	Description of Plant.†	Description of soil or Matrix.	Period of Experiment.	Sown.	That grew.	Fresh.	Dry.	Nitrogen.		In total Plant.	In Soil.	In Pot.	In condensed water.			
Graminaceæ.						gramme.	gramme.	gramme.	gramme.	gramme.	gramme.	gramme.	gramme.	gramme.	gramme.	gramme.
1857 ...	Wheat (1) ...	Prepared soil ...	May 16—Oct. 3	6	5	0·3658	0·3065	0·0080	1·412	0·0072	traces	traces	.....	0·0080	0·0072	−·0008
	Barley (2) ...	Prepared soil ...	May 20—Aug. 24	6	6	0·3230	0·2698	0·0056	0·810	0·0047	0·0024	0·0001	.....	0·0056	0·0072	+·0016
	Barley (3) ...	Prepared pumice	May 20—Aug. 25	6	6	0·3233	0·2698	0·0056	0·925	0·0045	0·0030	0·0007	.....	0·0056	0·0082	+·0026
1858 ...	Wheat (1) ...	Prepared soil ...	April 27—Oct. 25	8	8	0·4815	0·4035	0·0078	1·740	0·0056	0·0025		traces	0·0078	0·0081	+·0003
	Barley (2) ...	Prepared soil ...	April 27—Aug. 18	8	6	0·3860	0·3221	0·0057	0·560	0·0031	0·0027		traces	0·0057	0·0058	+·0001
	Oats (3)	Prepared soil ...	April 27—July 13	8	8	0·3425	0·2858	0·0063	1·148	0·0042	0·0011		0·0003	0·0063	0·0056	−·0007
1858, A.*	Wheat	Prepared soil ...	June 11—Nov. 6	8	7	0·4810	0·4031	0·0078	1·060	0·0041	0·0033		0·0004	0·0078	0·0078	0·000
	Barley	Prepared soil ...	June 11—Nov. 6	8	8	0·3840	0·3240	0·0057	0·710							
	Oats	Prepared soil ...	June 11—Nov. 6	8	7	0·3450	0·2879	0·0064	0·690	0·0038	0·0021		0·0004	0·0064	0·0063	−·0001
Leguminosæ.																
1857	Bean (4)	Prepared soil ...	May 16—July 5	2	2	1·8907	1·4984	0·0796	7·028	0·0629	0·0146	0·0016	.....	0·0796	0·0791	−·0005
1858 ...	Bean (5)	Prepared soil ...	June 11—Aug. 23	3	3	1·8700	1·4820	0·0750	4·875	0·0735	0·0016		0·0006	0·0750	0·0757	+·0007
	Pea (6)	Prepared soil ...	June 5—Aug. 24	3	3	0·6436	0·5405	0·0188	0·970	0·0102	0·0056		0·0009	0·0188	0·0167	−·0021
Other Plants.																
1858	Buckwheat (7)	Prepared soil ...	Aug. 20—Oct. 28	24	13	1·0000	.....	0·0200	0·450	0·0070	0·0108		0·0004	0·0200	0·0182	−·0018

* These experiments were conducted in the apparatus of M. G. VILLE.

† The numbers given in brackets are those under which the respective plants are described in the "Abstract of the Records of growth of the Plants," given in the Appendix, p. 548 *et seq.*

‡ The percentage of dry matter in the seed was not determined in this case; it is therefore assumed to be the same as in the wheat used in 1858, from which it would certainly not differ at all materially.

seed,—the gain appearing only when the Nitrogen in the soil and pot is taken into account. It will be remembered that the results of the experiments on the question whether there was an evolution of Nitrogen during germination and growth (Table XI., p. 513) showed how completely the plants could appropriate the Nitrogen of the seed from which they grew, leaving only traces of it in the soil. Again, the experiments on the decomposition of Nitrogenous organic matter (Tables VIII. and IX., p. 506) have shown how thorough was the decomposition coincident with the passage of any large percentage of the combined Nitrogen of the substance into the soluble state of ammonia. Taking together these facts, we have strong grounds for assuming that at least a part of the Nitrogen found in the soil, in the cases where there was a gain of it in the total products, has never been in actual connexion with the plant at all. Indeed, in view of the facts just referred to, any gain of Nitrogen in connexion with the plant, without there being a larger quantity of Nitrogen in the plant itself than that provided in the seed, would be very questionable evidence upon which to establish the fact of the assimilation of free Nitrogen.

But the results obtained with Graminaceæ in 1858, when all possible sources of error which the experience of the previous year had suggested had been eliminated, point, without exception, to the fact that, under the circumstances of growth to which the plants were subjected, no assimilation of free Nitrogen has taken place. The regular process of cell-formation has gone on; carbonic acid has been decomposed, and carbon and the elements of water have been transformed into cellulose; the plants have drawn the nitrogenous compounds from the older cells to perform the mysterious office of the formation of new cells (see Notes on growth, Appendix, pp. 559, 561); those parts have been developed which required the smallest amount of Nitrogen; and all the stages of growth have been passed through to the formation of glumes, pales, and awns for the seed. In fact, the plants have performed all the functions that it is possible for a plant to perform when deprived of a sufficient supply of combined Nitrogen. They have gone on thus increasing their organic constituents with one constant amount of combined Nitrogen, until the percentage of that element in the vegetable matter is far below the ordinary amount of it—that is, until the composition indicates that further development had ceased for want of a supply of available Nitrogen.

Throughout all these phases, water saturated with free Nitrogen has been passing through the plant; nitrogen dissolved in the fluid of the cells has constantly been in the most intimate contact with the contents of the cells and with the cell-walls. The newly forming cell, stunted in its development for want of assimilable Nitrogen, has nevertheless been surrounded by free Nitrogen. Its delicate membranes have been saturated with water, itself saturated with free Nitrogen; and such are the laws in accordance with which the absorption of gases, and the transmission of liquids through membranes take place, that the instant a part of the Nitrogen of the saturated fluid became assimilated, the equilibrium would be restored, by the penetration into the cell of other saturated liquid, and the re-saturation of that from which Nitrogen had been with-

drawn. It would hardly be supposed that, under such circumstances, the process of cell-formation could go on without the assimilation of free Nitrogen, provided any forces were exerted in the cell the tendency of which was to fix free Nitrogen in the organism of the plant.

One fact, briefly alluded to above, we wish to call more special attention to, as affording strong evidence of the absence of the power on the part of these cereal plants to appropriate free Nitrogen—namely, the very large development of the root, requiring but little Nitrogen compared with that of other parts. It was observed, in the experiments of 1857, that several of the cereal plants developed a large proportion of root; but the danger of accident in analysis was such, that we hesitated to double the risk of losing the entire result by analysing the root and the portion of the plant above ground separately. They were, therefore, thoroughly mixed, and the mixture was carefully divided; so that, in case of accident, a duplicate was at our disposal, and in case of all going well, confirmatory evidence was obtained. So very marked, however, was the great development of root in the cereals of 1858, that, in several cases, it was analysed separately from the other parts of the plant. The remarkable result was obtained, that this great root-development was carried on (in two, at least, out of the three instances in question) with a consumption of an almost incredibly small amount of Nitrogen, as the figures given in the following Table will show:—

TABLE XIII.

Description of Plant. 1858.	Dry Matter in Produce (at 100° C.), grammes.			Nitrogen in Produce (grammes).			Per cent. of Total Dry Matter in Roots.	Per cent. of Total Nitrogen in Roots.
	In Stems, &c.	In Roots.	In Total Produce.	In Stems, &c.	In Roots.	In Total Produce.		
Wheat (1)	0·890	0·850	1·740	0·0039	0·0017	0·0056	48·85	30·36
Barley (2)	0·400	0·160	0·560	0·0027	0·0004	0·0031	28·57	12·90
Oats (3)	0·798	0·350	1·148	0·0040	0·0002	0·0042	30·49	4·76

The large proportion of root and its small proportion of Nitrogen, as here exhibited, are equally remarkable. Whether this great power of the plant to develop root be due to the fact that the process of cell-formation in the root requires less of the nitrogenous protoplasmic compound, or to the fact that, floating in water as these roots generally were, that fluid facilitated the withdrawal of the nitrogenous constituents resulting from the decomposition of protoplasma from the old cells, to form new protoplasma for the more active cells, is a question which, though foreign to our present subject, is of considerable interest in a physiological point of view. The fact that the roots from the base of the stem penetrated the soil, giving off very few branches into it, but immediately on reaching the water at the bottom of the pot exhibited such a remarkable development (see Notes on taking up the Wheat Plants, Appendix, p. 560), is in favour of the inference that the water afforded the necessary conditions for the character of growth referred to.

But, apart from the physiological points just referred to, as already said, this great development of a part of the plant requiring a minimum amount of Nitrogen affords strong evidence of its inability to assimilate free Nitrogen within the range of development possible when no combined Nitrogen is provided beyond that contained in the original seed. It exhibits the great tenacity of growth of the plant, and shows the activity of the vital force, long after the demands of the organism had begun to require more available Nitrogen than was at its disposal. When it is considered how great was the length of time during which the growing cells were exposed to the conditions in question, there would seem to be a combination of circumstances favourable to the exercise of any force tending to bring free Nitrogen into the constitution of the plant. But no such effect is manifested in the results.

The Graminaceæ referred to in the Table (XII.) under the Title of "1858, A.," and which were grown in the enclosing apparatus of M. G. VILLE, as already alluded to, give results quite similar in their bearings on the main question to those of 1857 and 1858 already discussed. Being sown later, however, and their period of growth being shorter, they did not manifest such an extraordinary development of root; nor was there so large an amount of vegetable matter produced. Unfortunately the barley grown in M. VILLE'S Case without artificial supply of combined Nitrogen, was lost by the giving way of the tube in the combustion for the determination of Nitrogen. In its case, therefore, we can only give the amount of the dry matter of the plants produced. But, comparing this with that of the seed sown, and looking to the proportions of Nitrogen in the produce of barley in the other cases, there is no reason to believe that the result would have formed any exception to that indicated in the other experiments.

In concluding our remarks on the results with the Graminaceæ grown without any further supply of combined Nitrogen than that contained in the seed sown, we would beg to refer the reader to the foregoing consideration of the conditions possibly favourable to the assimilation of free Nitrogen (p. 517 *et seq.*).

It will be remembered that, in experimenting with Graminaceæ, including some of the same description as those experimented upon by ourselves, M. BOUSSINGAULT and M. G. VILLE obtained most unaccountably discordant results. It will be seen that our own results, from nine experiments with such plants, go to confirm those of M. BOUSSINGAULT. In fact, so far as our labours with these plants bear upon their experiments, they could not have given a more decided result.

For representations of some of the Graminaceæ grown without any supply of combined Nitrogen beyond that contained in the original seed, see figs. 1 to 6, Plate XV.

Leguminous Plants.

It still remains to consider the results of our experiments with Leguminous plants grown under similar conditions to those of the Graminaceous ones above discussed, and to see how far they serve to explain the known characteristics of such plants when grown in practical agriculture, to which attention has been directed in Part First of this Paper.

It will be remembered that, under equal circumstances of soil and season, Leguminous crops yield two, three, or more times as much Nitrogen per acre as Graminaceous ones. Yet, whilst the latter are very characteristically benefited by the use of direct nitrogenous manures, the former, yielding so much more Nitrogen, are not so. Again, the Graminaceous crop, requiring for full produce such direct supply of available Nitrogen within the soil, is very much increased, beyond what it would be if it succeeded a crop of the same description, when it follows a Leguminous crop, in which has been carried off so much Nitrogen.

Experiments such as those now specially under consideration can obviously bear upon a few only of the circumstances with which may be connected the causes of this difference between the Graminaceous and the Leguminous crops. Without, therefore, pretending adequately to discuss this wide subject, we will consider it only so far as our immediate facts appear to bear upon it; they seem to limit us to the consideration of the following cases:—

1. The difference may be due to the decomposition of nitrogenous compounds during the growth of the Graminaceous plants, and to the evolution of free Nitrogen.

2. The Leguminous plants may assimilate the free Nitrogen of the air, and thus, not only allow the resources of the soil to accumulate, but also leave within it an additional quantity, in roots and other vegetable débris, from that which has been assimilated, as above supposed.

3. It may be due to the operation of both these causes.

So far as the facts we have already considered go, the difference in question cannot be explained according to the first of the above suppositions; and others, to which we shall have presently to refer, will be seen to afford confirmatory evidence on the point.

With regard to the second supposed explanation, the results we have now to record of our experiments with Leguminous plants are not of themselves sufficient to settle every point which it involves. Reference to the Appendix will show that, in several cases, we failed to get healthy growth with Leguminous plants. A doubt might hence be raised, as to the value of those experiments in which we were successful under circumstances so nearly identical with those of our failures that it was not easy to account for the difference of result obtained. In those cases, however, in which we have succeeded in getting Leguminous plants to grow pretty healthily for a considerable length of time, the results, so far as they go, confirm those obtained with Graminaceæ, not showing in their case, any more than with the latter, an assimilation of free Nitrogen.

In 1857, we commenced several experiments with beans, but they grew well in only one of the shades. These, however (especially one plant out of the two in the same pot), progressed remarkably well for a period of 10 weeks, during which time the amount of carbon was increased five-fold, more than three-fourths of the total Nitrogen of the seed was appropriated, and the plants probably only ceased to grow when the remainder of the latter became so distributed in the soil as not to be available to them.

A reference to Table XII. will show the numerical results of this experiment with beans in 1857.

The beans and peas of 1858, the particulars of which are also given in Table XII., did not grow so satisfactorily as the beans of 1857, last noticed. Yet the beans of 1858 gave more than three times as much organic matter in the produce as was contained in the seed, and they appropriated even a much larger proportion of the Nitrogen of the seed than did those of 1857. The result with the peas was not so satisfactory, owing to the less healthy character and the more limited amount of their growth.

From the fact that these Leguminous plants did not go through a complete course of growth to the flowering process, it may be objected that hence they did not pass certain stages of growth in which they might possibly assimilate free Nitrogen. We shall refer to this objection again further on. At present we confine attention to the important fact, that active growth has taken place—that the process of cell-formation, with the accompanying one of the decomposition of carbonic acid and the fixation of carbon, has gone forward with a deficient supply of combined Nitrogen, and in the immediate presence of free Nitrogen, and yet none of it has been assimilated. The plants have in fact been subjected to a considerable range of the conditions which were considered, *à priori*, to be favourable to the assimilation of free Nitrogen; and yet this has not taken place.

It is a fact observed in agriculture, that manures rich in organic matter frequently favour the growth of Leguminous crops. We shall not here discuss the question whether these organic manures, as such, act simply as a source of carbonic acid, or of carbon compounds of a more complicated character. We would, however, call attention to the fact that, in the case of the experiments now under consideration, the vital forces were sufficiently energetic to perform the function of cell-development and multiplication, from carbonic acid as its source of carbon; yet these forces, capable of effecting this result, have been incapable of effecting the appropriation of free Nitrogen.

Buckwheat.

The evidence afforded by the numerical results in the Table XII. relating to this plant is not of so decisive a character as that with regard to the cereals, or even to the Leguminous plants; for the quantity of dry matter in the produced plants is less than that in the seed sown, whilst the Nitrogen in the plants is little more than one-third that of the seed. But when we come to compare the results of the experiments with Buckwheat grown with and without the supply of ammonia, it will be found that the physiological evidence of the dependence of vegetable growth upon a constant supply of combined Nitrogen is stronger in the case of these plants than in that of the cereals. The small proportion of the total Nitrogen of the seeds which the buckwheat seemed capable of appropriating might lead to the inference that, ceasing to grow with an abundance of combined Nitrogen apparently at its disposal, it had done so for some other reason than the want of available Nitrogen. But this question was set at rest by the fact that, on the addition of an amount of ammonia very small in its contents of

Nitrogen compared with the seed, to plants at the time in a precisely similar condition to those now under consideration, the increase in the rapidity of growth was most marked.

Most of the buckwheat seed sown came up; but about half of the plants lived for only a few days. The remainder, which survived, went through all the stages of development to flowering; but the entire amount of growth was on a very limited scale.

Reference to the last column of Table XII. will show that, under the conditions of growth above described, the buckwheat, like the plants already discussed, indicated no gain of Nitrogen. In fact there appeared to be a loss in the experiment of nearly 2 milligrammes of Nitrogen; and that the result should be to a small extent in this direction may, perhaps, be accounted for by the fact of some of the plants dying early, in consequence of which there may have been a slight evolution of free Nitrogen due to decomposition.

Bearing of the above results on the question of the evolution of free Nitrogen from the Nitrogenous Constituents of plants during growth.

We have thus far only considered the above results so far as they bear upon the question of the assimilation of free Nitrogen by plants. But from the constancy of the amount of combined Nitrogen maintained in relation to that supplied, throughout the experiments, they afford evidence of an important kind in regard to the converse question of whether plants give off free Nitrogen during growth. With no less force than they point to the absence of any assimilation of free Nitrogen, do these results show that, under the circumstances of growth involved, there has been no evolution of free Nitrogen from the nitrogenous compounds of the growing plant. At all events, the assumption that an evolution of free Nitrogen has taken place implies, as in the case of the experiments discussed at pp. 513, 514, the still more improbable one, that there has been an exactly compensating amount assimilated. But since the conditions of the experiments now under consideration were arranged with special reference to the question of assimilation, they necessarily do not embrace all the circumstances which, *à priori*, would be considered the most favourable for the evolution of free Nitrogen during growth.

Various experimenters, from the time of De Saussure until quite recently, have entertained the idea of the probability of the decomposition of nitrogenous compounds, and the concomitant evolution of free Nitrogen, during the growth of plants. We are ourselves engaged in following up the subject, by methods better qualified to settle the question than those adopted in regard to the question of assimilation of Nitrogen. We shall therefore not treat of this subject any further here, than to call attention to the incidental bearing upon it of the results now under consideration.

The fact that there has been no decomposition of nitrogenous compounds and loss of Nitrogen as the result of growth, in the particular conditions to which these experimental plants were subjected, affords little evidence that no such decomposition could take

place under any other circumstances. When supplied with an insufficient quantity of nitrogenous matter, the vegetable organism might not decompose any of that matter; and yet, when an excess of combined Nitrogen was supplied, the decomposition might occur. The results we have given, therefore, afford evidence against the fact of such decomposition only within a very limited range of circumstances of growth. In discussing the results of the experiments the consideration of which we are now about to enter upon, we shall refer to this question again, in connexion with circumstances of growth which we should suppose would be more favourable to an evolution of free Nitrogen by the plant.

II.—*Experiments in which the plants had a known supply of combined Nitrogen beyond that contained in the Original seed.*

We have thus far considered the subject of the assimilation of free Nitrogen, by reference to the results of experiments upon plants grown without any supply of combined Nitrogen beyond that contained in the seed sown. We have found that, under these conditions, we have only been able to study the results of growth of a very limited character. The wheat, and barley, and oat plants, grown in 1858, did indeed progress so far as to produce glumes and pales for seed; but they did not afford the opportunity of studying the results of growth during the period of the formation and the ripening of seeds themselves.

It yet remains to consider, therefore, what may take place under circumstances of a more active and vigorous growth, and at a later stage of development of the plant. When considering the conditions apparently the most favourable for the assimilation of free Nitrogen by plants (p. 517 *et seq.*), we suggested the improbability of such an assimilation taking place in the presence of an abundant supply of combined Nitrogen. If the force of our remarks on this point be admitted, and it be still supposed that an assimilation of free Nitrogen is possible with vigorous growth, only attainable by means of a liberal supply of combined Nitrogen, we seem to be led to the following paradoxical conclusions:—

1. Healthy, active, and vigorous growth are favourable conditions for the assimilation of free Nitrogen by plants.

2. Healthy, active, and vigorous growth can only be attained by keeping within the reach of the plant an excess of combined Nitrogen.

3. Assimilation of free Nitrogen cannot take place in the presence of an excess of combined Nitrogen.

À priori conclusions with regard to the effect of molecular forces, and particularly of those which give rise to vital phenomena, are, however, very unsafe; and we have not been satisfied to rely upon such evidence only, in reference to the question under investigation, as could be afforded by experimenting with plants grown without an extraneous supply of combined Nitrogen. We have found that active and vigorous growth cannot be attained under the conditions provided, when no more combined Nitrogen than that

contained in the seed sown is supplied. We have made a series of experiments, in which such growth was attained by means of a supply of combined Nitrogen beyond that contained in the seed. It remains to see whether, under these conditions of growth, the assimilation of free Nitrogen can take place, and thus the above paradox be obviated by the proof that the last of the three suppositions is incorrect.

It is true that we have pointed out the improbability of an assimilation of free Nitrogen in the presence of an excess of combined Nitrogen only so far as the vital process of the vegetable cell is concerned. In that intermediate process by which oxygen is taken up and carbonic acid formed in the cell, the results due to an excess of combined Nitrogen might be different.

Thus, the more active the growth, the greater must be the amount of newly-formed carbon-matter capable of consuming oxygen, when the plant is removed from the influence of sunlight into the dark. That is to say, the more vigorous the growth in the sunlight, the greater might be the reducing power of the plant in the dark. The greater the reducing power of the plant, the more nearly will the tendency of its molecular forces approximate to an evolution of hydrogen which, in the presence of free Nitrogen dissolved in the fluids of the cell, may tend to form ammoniacal compounds, to be, on the return of light, appropriated by the plant in the exercise of its growing functions. In connexion with this point, it may be here mentioned that in our investigation of the gases given off by plants under different circumstances, we have had an evolution of oxygen one day as a coincident of growth, and an evolution of hydrogen the next as the result of decomposition.

Our experiments in which the plants have been manured with limited amounts of combined Nitrogen will not only enable us to meet some of the questions above suggested, but they will also prove whether or not the conditions of soil, atmosphere, temperature, &c., to which our experimental plants have been subjected were consistent with active and vigorous growth.

The fact of the evolution of Nitrogen in the decomposition of nitrogenous organic matter, illustrated in Sub-section D, p. 497 *et seq.*, indicated the danger of using such matter as a source of supply of Nitrogen. We have therefore used solutions of sulphate of ammonia (see Appendix, p. 542), by means of which we have been enabled to supply the plants with known quantities of combined Nitrogen at pleasure, as the progress of growth seemed to require.

In the following Table (XIV.) are given the numerical results of the experiments on the question of the assimilation of free Nitrogen in which the plants were supplied with combined Nitrogen beyond that contained in the seed sown. See also figs. 7, 8, 9, 10, 11, and 12, Plate XV., showing the character and extent of growth of six Graminaceous plants with extraneous supply of combined Nitrogen, corresponding to the six above them without such supply.

TABLE XIV.—Showing the Numerical Results of Experiments to determine whether Plants supplied with known and limited quantities of combined Nitrogen beyond that contained in the original Seed assimilate free Nitrogen.

General particulars of the Experiments.				Numerical particulars of the seed sown, and of the combined Nitrogen added.							Numerical Results at the termination of the Experiment.					Summary.		
				Number of seeds		Weights of the seed sown		Nitrogen in seeds sown, and in manure.			Dry weights of Plants produced.	Nitrogen				Nitrogen in Seeds sown, and in Manure.	Nitrogen in Total Products.	Gain or Loss of Combined Nitrogen.
Year, &c.	Description of Plant.†	Description of soil or Matrix.	Period of Experiment.	Sown	That grew.	Fresh.	Dry (at 100° C.).	In seeds.	In manure.	Total.		In total Plant.	In Soil.	In Pot.	In condensed water.			
						grm.	grm.	grm.	grm.	grm.	grm.	grm.	grm.	grm.	grm.	grm.	grm.	grm.
Graminaceæ.																		
1857	Wheat (6)	Prepared soil	May 16—Oct. 2	3	2	0·1830	0·1536‡	0·0040	0·0289	0·0329	6·835	0·0241	0·0133	0·0099	.....	0·0329	0·0383	+0·0054
	Wheat (7)	Prepared pumice	May 16—Sept. 20	3	3	0·1838	0·1543‡	0·0040	0·0289	0·0329	3·822	0·0213	0·0102	0·0016	.....	0·0329	0·0331	+0·0002
	Barley (8)	Prepared soil	May 20—Oct. 8	4	3	0·2160	0·1803	0·0037	0·0289	0·0326	3·034	0·0154	0·0171	0·0003	.....	0·0326	0·0328	+0·0002
	Barley (9)	Prepared pumice	May 20—Sept. 24	4	4	0·2178	0·1818	0·0037	0·0231	0·0268	4·401	0·0145	0·0171	0·0021	.....	0·0268	0·0337	+0·0069
1858	Wheat (9)	Prepared soil	April 27—Oct. 26	4	4	0·243	0·204	0·0040	0·0508	0·0548	7·31	0·0398	0·0138		traces	0·0548	0·0536	−0·0012
	Barley (10)	Prepared soil	April 27—Oct. 26	4	2	0·190	0·158	0·0028	0·0468	0·0496	5·47	0·0349	0·0113		0·0002	0·0496	0·0464	−0·0032
	Oats (11)	Prepared soil	April 27—July 30	4	3	0·174	0·145	0·0032	0·0280	0·0312	1·204	0·0125	0·0040		0·0051	0·0312	0·0216	−0·0096
1858, A.*	Wheat	Prepared soil	June 29—Dec. 9	4	4	0·246	0·206	0·0040	0·0228	0·0268	3·82	0·0181	0·0092		0·0001	0·0268	0·0274	+0·0006
	Barley	Prepared soil	June 29—Dec. 9	4	3	0·194	0·162	0·0029	0·0228	0·0257	2·98	0·0160	0·0081		0·0001	0·0257	0·0242	−0·0015
	Oats	Prepared soil	June 29—Dec. 9	4	2	0·173	0·144	0·0032	0·0228	0·0260	1·28	0·0146	0·0051		0·0001	0·0260	0·0198	−0·0062
Leguminosæ.																		
1858	Pea (13)	Prepared soil	April 27—Aug. 24	3	3	0·642	0·539	0·0187	0·0040	0·0227	1·01	0·0114	0·0086		0·0011	0·0227	0·0211	−0·0016
	Clover (14)	Prepared soil	June 6—Oct. 26	...	...	0·5015	.....	0·0284	0·0428	0·0712	4·20	0·0313	0·0339		0·0013	0·0712	0·0665	−0·0047
1858, A.*	Bean	Prepared soil	June 29—Dec. 9	3	3	1·380	1·094	0·0523	0·0188	0·0711	4·30	0·0401	0·0253		0·0001	0·0711	0·0655	−0·0056
Other Plants.																		
1858	Buckwheat (16)	Prepared soil	Aug. 20—Nov. 22	42	24	1·0000	0·850	0·0200	0·0402	0·0308	1·97	0·0183	0·0101		0·0008	0·0308	0·0292	−0·0016

* These experiments were conducted in the apparatus of M. G. VILLE.

† The numbers given in brackets are those under which the respective plants are described in the "Abstract of the Records of growth of the Plants" given in the Appendix, p. 548 *et seq.*

‡ The percentage of dry matter in the seed was not determined in these two cases; it is therefore assumed to be the same as in the wheat used in 1858, from which it would certainly not differ at all materially.

As in the case of the experiments already considered, so again with those to which the Table just given relates, it is seen, by reference to the last column, that there was a slight gain of Nitrogen in the experiments of 1857, but, almost without exception, a loss rather than a gain in those of 1858. Considering that there was a possible source of gain in 1857 in connexion with the slates used in that year (as explained below), and with the results of 1858 showing generally a loss rather than a gain when slates were not employed, we can interpret the whole in but one way.

In order to bring out fully the evidence afforded by these results of experiments in which the plants were supplied with more or less of combined Nitrogen during the progress of growth, we must consider them in three separate aspects:—

1. As regards the actual gain or loss of Nitrogen, as indicated by the figures given in the last column of the Table (XIV.).

2. As presented in the physiological evidence afforded during growth.

3. As exhibited on comparison with the experiments in which the plants had no other supply of combined Nitrogen than that of the original seed.

1. *The Numerical Results of Table XIV.*

Much that has been said with respect to the plants grown without extraneous supply of combined Nitrogen applies with equal force to those now under consideration; and, so far as the evidence relating to the latter is of a different character, owing to the amount of combined Nitrogen at the disposal of the plants, it still is no more indicative of an assimilation of free Nitrogen than was that obtained with the plants grown without any artificial supply of combined Nitrogen.

In illustration of the probability that the slates used as lute-vessels were a source of Nitrogen to the plants grown in 1857, some of the observations made during growth should be adverted to. It is seen that the barley grown in pumice (1857) gives the largest gain of Nitrogen; and it was observed that, soon after watering with the fluid drawn off from the surface of the slate, the pumice became covered with a slight coating of green matter. And nearly all the slates were found at the end of the experiment to have a slight coating of similar character beneath the pans in which the pots which contained the plants stood; whilst, in the experiments of 1858, when glazed earthenware lute-vessels were employed, no such phenomenon was observed.

The slight loss of Nitrogen exhibited in the experiments of 1858 is easily accounted for on a consideration of the conditions involved. With regard to the peas, clover, and beans, the physiological circumstances of growth detailed in the Appendix, taken in connexion with the evidence that has been adduced as to the loss of Nitrogen during the decomposition of nitrogenous organic matter, must be supposed to explain the loss in their case, as in some of the experiments in which no extraneous supply of combined Nitrogen was employed.

The loss of Nitrogen indicated in the cases of the wheat, barley, oats, and buck-

wheat (1858) would not be so easily explained, had not the Nitrogen in the drain-water remaining at the end of the experiment been determined. Our object in doing this was twofold:—

1. To ascertain whether the luting at the bottom of the shade had allowed rain-water to pass, thus affording a source of combined Nitrogen to the plants.

2. To see if the plants growing in soil to which combined Nitrogen was added, had evolved any ammonia.

It was, of course, not possible to accomplish both these purposes. But the fact that ammonia was found in the condensed water only in the cases where there was a *loss* in the total quantity of combined Nitrogen would lead to the inference that both the presence of ammonia in this water, and the loss of combined Nitrogen in the experiment, were due to the same cause.

The condensed water showing the amount of combined Nitrogen recorded in the Table (XIV.) was that which had been evaporated and condensed during the last four weeks of growth (1858); and during this period the high temperature, and the advanced stage of the plants, were favourable to the evaporation of ammoniacal water. A considerable part would condense on the interior of the shade, owing to its comparatively low temperature; but a certain quantity of that which was in the state of vapour during the passage of the air through the apparatus would be borne forward into the sulphuric acid in the bulb-apparatus M, and thus occasion a loss in the amount of combined Nitrogen determined in connexion with the plants. The reason why the loss is greater with the oats (as it is in both experiments) than with the other cereals is not perfectly clear; but the circumstances of growth seemed to afford some explanation of the fact. In one case, at least, they ripened at a much warmer period of the season, and they became much drier in stem and leaf, and were therefore more liable to evolve ammonia. On these points, the circumstances of growth detailed in the Appendix should be consulted.

In considering the column of gain or loss of Nitrogen, it is very desirable to take into account the total quantity of Nitrogen at the disposal of the plant, in the different series of experiments. It is also important to consider the amount of growth in the experiments made under the different conditions. The following Tables (XV. and XVI.) bring out the character of the results in these respects more clearly than they can be gathered from Tables XII. and XIV. Table XV. shows, for the plants grown without supply of combined Nitrogen beyond that contained in the seed, and Table XVI. for those grown with such supply, the dry matter, and the Nitrogen, per seed sown,—the dry matter, and the Nitrogen, in the total produce of each seed that grew,—and the per cent. of the total Nitrogen at the disposal of the plant which it appropriated. Finally, the last two columns of Table XVI. show the amounts of dry matter, and of Nitrogen, in the produce grown with the extraneous supply of combined Nitrogen, in relation to those in the produce grown without such supply.

TABLE XV.

General particulars of the Experiments.			Number of Seeds.		Dry Matter.			Nitrogen.			
Year, &c.	Description of Plants.	Description of Soil or Matrix.	Sown.	That grew.	Per seed sown.	In the Produce, per seed that grew.	In Produce, that in seed taken as 1.	Per seed sown.	In the Produce, per seed that grew.	In Produce, that in seed taken as 100.	Per cent. in Dry Produce.
Graminaceæ.											
					grm.	grm.		grm.	grm.		
1857	Wheat	Prepared soil	6	5	0·0511	0·2824	.....	0·00133	0·00144	108·3†	0·51
	Barley	Prepared soil	6	6	0·0449	0·1350	3·01	0·00093	0·00078	83·9	0·58
	Barley	Prepared pumice	6	6	0·0449	0·1542	3·43	0·00093	0·00075	80·6	0·48
1858	Wheat	Prepared soil	8	8	0·0504	0·2175	4·31	0·00098	0·00070	71·4	0·32
	Barley	Prepared soil	8	6	0·0403	0·0933	2·31	0·00071	0·00052	73·2	0·54
	Oats	Prepared soil	8	8	0·0357	0·1435	4·02	0·00080	0·000525	65·6	0·36
1858, A.*	Wheat	Prepared soil	8	7	0·0504	0·15143	3·00	0·00098	0·00058	59·2	0·38
	Barley	Prepared soil	8	8	0·0401	0·0888					
	Oats	Prepared soil	8	7	0·0360	0·09857	2·74	0·00080	0·00054	67·5	0·55
Leguminosæ.											
1857	Bean	Prepared soil	2	2	0·7492	3·514	4·69	0·03980	0·03145	79·0	0·89
1858	Bean	Prepared soil	3	3	0·4940	1·6250	3·29	0·02500	0·0245	98·0	1·51
	Pea	Prepared soil	3	3	0·1802	0·3233	1·79	0·00630	0·0034	54·0	1·05
Other Plants.											
1858	Buckwheat	Prepared soil	24	13	.....	0·03461	.....	0·00083	0·00054	65·1	1·56

TABLE XVI.

General particulars of the Experiments.			Number of Seeds.		Dry Matter.			Nitrogen.				Relation of Produce per seed with Ammonia, to that without it taken as 1.	
Year. &c.	Description of Plants.	Description of Soil or Matrix.	Sown.	That grew.	Per seed sown.	In Produce, per seed that grew.	In Produce, that in seed taken as 1.	Per seed sown.	In the Produce, per seed that grew.	In Produce, that in seed and manure taken as 100.	Per cent. in Dry Produce.	Dry Matter.	Nitrogen.
Graminaceæ.													
					grm.	grm.		grm.	grm.				
1857	Wheat	Prepared soil	3	2	0·0512	3·4175	66·75	0·00133	0·01205	73·2	0·35	12·10	8·36
	Wheat	Prepared pumice	3	3	0·0514	1·2740	24·79	0·00133	0·00710	64·7	0·55	4·51	4·93
	Barley	Prepared soil	4	3	0·0451	1·0113	22·42	0·00092	0·00513	47·2	0·51	7·49	6·58
	Barley	Prepared pumice	4	4	0·0455	1·1002	24·18	0·00092	0·00362	54·1	0·33	7·13	4·83
1858	Wheat	Prepared soil	4	4	0·0510	1·8275	35·83	0·00103	0·00995	72·8	0·54	8·40	14·21
	Barley	Prepared soil	4	2	0·0395	2·7350	69·24	0·00070	0·01745	70·5	0·64	29·31	34·21
	Oats	Prepared soil	4	3	0·0362	0·4013	11·08	0·00080	0·00416	45·9	1·04	2·80	7·92
1858, A.*	Wheat	Prepared soil	4	4	0·0515	0·9550	18·54	0·00100	0·00452	67·5	0·47	6·31	7·79
	Barley	Prepared soil	4	3	0·0405	0·9933	24·52	0·00072	0·00533	62·2	0·54		
	Oats	Prepared soil	4	2	0·0360	0·6400	17·78	0·00080	0·00730	56·1	1·14	6·49	13·52
Leguminosæ.													
1858	Pea	Prepared soil	3	3	0·1797	0·3366	1·87	0·00623	0·00380	50·2	1·13		
	Clover	Prepared soil	...	...	.....	.....	.....	.....	.....	44·0			
1858, A.*	Bean	Prepared soil	3	3	0·3646	1·4333	3·93	0·01743	0·01337	56·4	0·93		
Other Plants.													
1858	Buckwheat	Prepared soil	42	24	0·0202	0·0821	4·06	0·00047	0·00076	57·6	0·92	2·37	1·41

* These experiments were conducted in the apparatus of M. G. VILLE.

† There is here evidence that a part of the Nitrogen of the seed that did not grow was appropriated by the plants growing from the other seeds.

There are several obvious inferences to be drawn from the figures in these Tables. To some we shall refer further on, in the proper order of the discussion. We here simply call attention to the very great increase of growth when an extraneous supply of combined Nitrogen was provided, as exhibited in the last two columns of Table XVI.

2. *Consideration of the Physiological Evidence as bearing upon the question of the assimilation of free Nitrogen.*

However directly the quantitative details given in the Tables may bear upon the question at issue, it is very important to consider them in connexion with the physiological details of the experiments. In order to estimate the value of the evidence afforded in this particular, the indications manifested from the earliest period of growth should be noticed.

Reference to the Notes of the progress of the plants, given in the Appendix, will show that all the plants when they first came up looked green and vigorous, indicative of their being at that period in circumstances embracing all the conditions essential to healthy growth. As already pointed out, they at that time were probably supplied with an excess of combined Nitrogen in relation to their immediate wants. After some days, varying with the nature of the plants, they began to lose their deep-green colour, and to assume a lighter-green, or pale-yellow tint, indicative of a want of combined Nitrogen. We have already pointed out how favourable, probably, would be the conditions here afforded for the assimilation of free Nitrogen, when the plant was passing from the state in which it had an excess to that in which it had a deficiency of combined Nitrogen for the demands of growth. The vigorous development of the plants grown in garden soil, but under the same conditions as to atmosphere, &c. as the other experimental plants, indicates that the conditions of atmosphere provided in the experiments were not at fault (see Appendix, Experiments Nos. 12, 1857, and 15, 1858; also fig. 13, Plate XV.). In order to test whether the sum of all the conditions, excepting those connected with a sufficient supply of combined Nitrogen, were appropriate for vigorous growth, we have only to provide some combined Nitrogen when the plants show the declining vigour just described; and if this be all they require, they will resume their healthy green colour. Or if we add the combined Nitrogen before the plants arrive at the period in question, it will prevent them assuming the pale-green or yellow colour. We have had recourse to both of these expedients; and each, so far as the Cereals, buckwheat, and clover are concerned, has yielded a result indicating that all the conditions of the experiments, excepting those connected with a sufficient supply of combined Nitrogen, were adapted for healthy growth.

The plants to which ammonia was given in 1857, were allowed to suffer more before they received it than those of 1858; yet in thirty-six hours after the addition of combined Nitrogen to the soil, in amount not exceeding $1\frac{1}{3}$ milligramme of the element to each plant, they began to manifest an improved appearance. In two or three days the improvement was quite marked; but at the termination of periods varying from nine to

eighteen days, the plants seemed to have consumed all the combined Nitrogen supplied to them—or rather all of it that had not become inaccessible to them in the soil. They then began to manifest the same indications of defective supply as before. Plants so circumstanced must therefore, at a more advanced stage of growth than before they had been supplied with ammonia, have passed from a point at which they had an excess of combined Nitrogen, to that in which they had an insufficiency. They must hence, again, have been subjected to those conditions which we have assumed to be probably very favourable to the assimilation of free Nitrogen.

Reference to the details of growth given in the Appendix will show that several times during the progress of the plants the above phenomena were manifested. A new increment of combined Nitrogen caused a new increment of growth, a greener colour, and a more vigorous appearance generally. This was soon followed by the recurrence of the pale colour. In some instances, more ammonia was not supplied until the plants seemed almost past recovery: in a few cases they were quite so. The addition of ammonia now (excepting in the few cases just referred to) produced a revivification, to be followed in a short time by the indications of some want, and so on.

A considerable range of conditions of growth was thus provided. Just after each addition of combined Nitrogen the plants must have been supplied with an excess of this element in an available form. The evidence of this was afforded in the obviously increased means of consumption, evinced in the formation of new shoots from the base of the plants, or from their nodes. But these new shoots were too vigorous to allow the plants to go on long without suffering for want of a new supply of combined Nitrogen. In passing to this point, the newly-formed and vigorously-growing portion of the vegetable matter would be in the condition we have assumed to be the most favourable for assimilating free Nitrogen. Instead of doing this, however, it soon began to suffer, and continued to do so until a new supply of combined Nitrogen was added, when new vigour succeeded, to be followed again shortly by a cessation of growth. This cycle of conditions, repeated several times during the growth of the same plant, and the experiment similarly conducted with a number of pots of plants of different kinds, with like results in all the cases, affords a wide range of circumstances such as we have assumed to be favourable to the assimilation of free Nitrogen; but such an assimilation has not taken place.

Without the physiological details, it might not have been clear that the plant had not an excess of combined Nitrogen at its disposal during the greater period of its growth after the addition of the artificial supplies of it, since a considerable proportion of that added remained in the soil at the termination of the experiments, as Tables XIV. and XVI. show. But it is not difficult to imagine that a few milligrammes of ammonia intermingled with 1500 or 1600 grammes of soil (and pot), might become distributed over such an extent of surface, and be so completely absorbed, as that a considerable proportion should remain inaccessible to the plant. The physiological evidence leaves no doubt this was the case.

The Graminaceous plants of the experiments of 1858 were supplied with a considerable quantity of combined Nitrogen at an earlier period of growth than those of 1857 (see Tables showing the dates of addition, Appendix, pp. 542, 543), and they were not allowed to exhibit such marked signs of decline of vigour before receiving their fresh supplies. There is, however, no marked distinction in the proportion of the total supply appropriated by the plants, and left in the soil, respectively, in the two cases.

The Graminaceæ under the title of "1858, A" (those grown in M. G. VILLE'S case) were treated similarly to the others of 1858, excepting that the combined Nitrogen was given to them at an earlier period of their growth, and they were not allowed to suffer at any time for want of it. We shall notice the difference in result presently.

In addition to the evidence of the physiological phenomena as bearing upon the amount of growth due to the supply of ammonia, attention should be called to the remarkable character of growth which was manifested. The evidence afforded on this head, is of interest in considering the question of the character of the conditions most favourable to the assimilation of free Nitrogen; and it also brings to view some remarkable features in vegetable physiology.

It will be seen, by reference to the Notes in the Appendix, that, shortly after the addition of ammonia for the first time to the Graminaceæ (1857 and 1858), the plants began to throw out new shoots at the base of the principal stem. It would thus appear that the plant, being supplied at the commencement of its growth with only the limited quantity of combined Nitrogen contained in its seed, had developed a stem commensurate with that quantity. But when new quantities of combined Nitrogen were placed at the disposal of the plant, forces were thus called into activity which were greater than could operate through the medium of the original stem. Some of the new shoots have come forth close to the surface of the soil, some at the first, and some at the second nodes. The character of growth in this respect can be best studied by reference to the drawings of the plants given in Plate XV.

Another and no less remarkable feature was the formation of roots at the second and third nodes above the ground in the case of most of the Graminaceous plants to which ammonia-salt was added as manure (see Plate XV.). These roots came out around the node, and extended downwards—several of them reaching the soil from heights varying from $\frac{1}{2}$ to $1\frac{1}{2}$, or even 2 inches, and penetrating it to the bottom of the pot. The most marked instance of this kind of growth was that of the barley represented in fig. 11, Plate XV., and in more detail, with special reference to the points now under consideration, in fig. 16, Plate XV. As will be seen in the figures, roots and new stems come from the same node, making the latter a veritable starting-point, or new axis of growth, like the seed in the first instance. The original stems, below these nodes, did not increase much in size beyond what they had attained before the addition of ammonia; but the stems above the nodes became much larger than the portions below them, as also did those of the new shoots.

Finally, so long as the conditions of growth of the plants were such that an addi-

tional supply of combined Nitrogen would cause increased development, so long must the physiological conditions have been such as to require available Nitrogen, and they must therefore have been more or less favourable to the assimilation of free Nitrogen, provided such assimilation were possible. Hence, the fact that this did not take place under the circumstances which have been described, seems to show that, at least in the case of these Graminaceæ, it is not possible.

Some of the remarks which we have made with regard to the influence of a supply of combined Nitrogen upon the growth of the Graminaceæ, apply also, in a greater or less degree, to the other plants experimented upon. We shall not comment here in detail upon the value of each experiment, but simply call attention to the columns of gain or loss of Nitrogen, in the Tables, and to the notes in the Appendix indicating the circumstances of growth of the plants.

With regard to the Leguminosæ experimented upon, it is to be observed that the development was by no means so satisfactory as in the case of the Graminaceæ. Hence the evidence which the results relating to them afford against the fact of assimilation of free Nitrogen must be admitted to apply to a more limited range of conditions of growth, and, therefore, to be less conclusive against the possibility of such assimilation. Still, so far as they go, the results with these plants, and also those with buckwheat, tend to confirm those obtained under the more favourable circumstances of growth with the cereals. It will be remembered, however, that M. Boussingault experimented with a great many Leguminous plants, and generally succeeded in getting much more healthy growth than we were able to do in the cases to which the figures in the Tables refer. Yet in no case did he find any such gain of Nitrogen as to lead him to the conclusion that these plants, any more than the Graminaceæ, assimilated free or uncombined Nitrogen. Our own experiments with Leguminous plants are, however, not yet concluded; so that we hope to supply some additional evidence on this subject, on a future occasion.

Relations of the Plants grown with a supply of ammonia to those grown without it.

We have already called attention to the fact that the physiological phenomena exhibited in the progress of the plants grown under the two different conditions as regards the supply of combined Nitrogen at their disposal, afford satisfactory evidence that the conditions provided in soil and atmosphere were all that were requisite in experiments for the solution of the question at issue with regard to the Cereals. The great development of these plants when ammonia was supplied (which was in fact almost in proportion to the amount supplied), the cessation of growth with the limit of the supply, together with the contrast between the growth with the aid of the ammonia and that without it, all afford evidence in one direction in regard to the question at issue, so far as these plants are concerned.

In Table XIV., relating to the plants to which ammonia was supplied, an experiment with clover is recorded. Reference to the remarks in the Appendix, p. 573, will show

that we failed to get any growth with clover without the addition of ammonia. Hence, excepting so far as this fact is itself a point for remark, no contrast can be drawn between the growth of this plant with and without an extraneous supply of combined Nitrogen.

From what has already been said, it will be easily understood that the contrast between the beans and peas grown with and without the addition of ammonia is not very satisfactory. These plants proved to be so sensitive, under the conditions provided in the experiments, that it was obvious that, in many cases, they suffered from other causes than a want of combined Nitrogen, which we were not able to control. In but one experiment with such plants, that with the bean "1858, A." (Table XIV.), was the influence of a supply of combined Nitrogen so marked as to indicate that the plants were previously suffering for want of such supply. It will be seen, by reference to the Table, that, in the case here referred to, the seeds sown contained 0·0523 gramme of Nitrogen, and that 0·0188 gramme was added in the form of ammonia-salt—making in all 0·0711 gramme of combined Nitrogen involved in the experiment. Of this the plants appropriated 0·0401 gramme—about one-fifth less, therefore, than was supplied in the seeds alone. Yet, although the numerical results, taken by themselves, thus afford but little evidence of the effect of the 0·0188 gramme of Nitrogen added in the form of ammonia, the increased vigour of growth on the addition did afford such evidence. In contrast with this single result, however, attention may be called to the results with the beans grown without any other supply of combined Nitrogen than that contained in the seed sown. The bean plants so grown in 1857, appropriated nearly four-fifths of the Nitrogen of their seed; and those grown in a similar way in 1858, appropriated a considerably larger proportion of the combined Nitrogen so provided to them.

From a review of the whole of the results considered in this Section, it appears, then, that in the case of the Graminaceous plants experimented upon the growth was the most healthy, and such as provided a wide range of conditions for the assimilation of free Nitrogen, provided this were at all possible. The growth of the Leguminous plants was not so healthy, and did not, therefore, provide such a wide range of conditions for the possible assimilation of free Nitrogen. Nor was the growth of other plants so satisfactory as that of the Graminaceous ones. In all, the growth was more or less increased by the supply of combined Nitrogen beyond that contained in the seed. The effect of such supply was the most marked with the Graminaceous plants—the increase in the produce of dry vegetable substance due to extraneous supply of combined Nitrogen being, in their case, eight, twelve, and even nearly thirty-fold, according to the amount of Nitrogen so provided. Yet, with nineteen experiments with Graminaceous plants, six with Leguminous ones, and some with plants of other descriptions—with such great variation in the amount and character of growth in the several cases—and with such great variation in the amount of combined Nitrogen involved in the experiments, in

no case have the results been such as to lead to the conclusion that there was an assimilation of free, or uncombined, Nitrogen.

The results of the whole inquiry may be very briefly enumerated as follow:—

The yield of Nitrogen in the vegetation over a given area of land, within a given time, especially in the case of Leguminous crops, is not satisfactorily explained by reference to the hitherto quantitatively determined periodical supplies of *combined* Nitrogen.

Numerous experiments have been made by M. Boussingault, from which he concludes that free or uncombined Nitrogen is not a direct source of the Nitrogen of vegetation. M. G. Ville, on the other hand, concludes, from his results, that free Nitrogen may be a source of a considerable proportion of the Nitrogen of growing plants. The views, or explanations, of other experimenters, on this disputed point, are various, and inconclusive.

It was found that the conditions of growth adopted in our own experiments, on the question of the assimilation of free Nitrogen by plants, were consistent with the healthy development of various Graminaceous plants, but not so much so for that of the Leguminous plants experimented upon.

From the results of various investigations, as well as from other considerations, we think it may be concluded that, under the circumstances of our experiments on the question of the assimilation of free Nitrogen by plants, there would not be any supply to them of an unaccounted quantity of combined Nitrogen, due either to the formation of oxygen-compounds of it under the influence of ozone, or to that of ammonia under the influence of nascent hydrogen.

We have found that free Nitrogen is given off in the decomposition of nitrogenous organic matter, under certain circumstances. But, considering the circumstances of such evolution, and those to which the nitrogenous organic matter necessarily involved in experiments on the question of the assimilation of free Nitrogen by plants is subjected, it may, we think, be concluded that there would be no loss of combined Nitrogen from this cause in such an experiment, excepting in certain cases, when it might be presupposed.

Our experimental evidence, so far as it goes, does not favour the supposition that there would be any loss of combined Nitrogen in our experiments on the question of assimilation, due to the evolution of free Nitrogen from the nitrogenous constituents of the plants during growth.

In numerous experiments with Graminaceous plants, grown both with and without a supply of combined Nitrogen beyond that contained in the seed sown, in which there was great variation in the amount of combined nitrogen involved, and a wide range in the conditions, character, and amount of growth, we have in no case found any evidence of an assimilation of free or uncombined Nitrogen.

In our experiments with Leguminous plants the growth was less satisfactory; and

the range of conditions possibly favourable for the assimilation of free Nitrogen was, therefore, more limited. But the results recorded with these plants, so far as they go, do not indicate any assimilation of free Nitrogen. Since, however, in practice, Leguminous crops assimilate, from some source, so very much more Nitrogen than Graminaceous ones, under ostensibly equal circumstances of supply of combined Nitrogen, it is desirable that the evidence of further experiments with these plants, under conditions of more healthy growth, should be obtained.

Results obtained with some other plants are in the same sense as those obtained with Graminaceæ and Leguminosæ, in regard to the question of the assimilation of free Nitrogen.

In view of the evidence afforded of the non-assimilation of free Nitrogen by plants under the wide range of circumstances provided in the experiments, it is desirable that the several actual or possible sources of *combined* Nitrogen to plants should be more fully investigated, both qualitatively and quantitatively.

If it be established that the processes of vegetation do not bring free Nitrogen into combination, it still remains not very obvious to what actions a large proportion of the existing combined Nitrogen may be attributed.

APPENDIX.

Received subsequently to the reading of the paper.

A.—*Preparation of solutions for manuring the Plants, dates of application, and quantities applied.*

Sulphate-of-Ammonia solution.—Ordinary ammonia-water was distilled from a flask, the vapour condensed in a receiver containing pure distilled water, and the strength of the solution determined by the volumetric method, by means of dilute sulphuric acid of known strength, the preparation of which is described further on, at p. 545. A given volume of the ammoniacal liquid thus prepared was neutralized by pure dilute sulphuric acid, of which the quantity added was determined by measurement, and the strength of the solution calculated accordingly. It was intended that each cubic centimetre should supply about one-tenth of a milligramme of combined nitrogen. The exact strength of the sulphate-of-ammonia solutions used in the course of the experiments was as under:—

TABLE I.

When used.	Volume of the pipette measure employed.	Combined nitrogen in a pipette measure of the solution.
	septems*.	gramme.
In the experiments of 1857	111·2	0·00578
In the experiments of 1858, to August 10 inclusive	100·0	0·004
In the experiments of 1858, after August 10	100·0	0·00359

Tables II. and III. show the dates of the application of the above solutions to the different plants, and the amounts of nitrogen so supplied.

TABLE II.—Showing the supply of combined Nitrogen, as Sulphate-of-Ammonia solution, to plants grown in 1857.

Dates.	Nitrogen supplied.			
	Wheat, in prepared soil.	Wheat, in prepared pumice.	Barley, in prepared soil.	Barley, in prepared pumice.
	gramme.	gramme.	gramme.	gramme.
June 10	·00578	·00578	·00578	·00578
July 4	·00578	·00578	·00578	·00578
July 11	·00578	·00578	·00578	·00578
July 22	·00578	·00578	·00578	·00578
July 29	·00578	·00578	·00578	
Total	·02890	·02890	·02890	·02312

* A septem measure is that of 7 grains ($=\frac{1}{1000}$ of a pound avoirdupois) of water; that is, rather less than half a cubic centimetre, which is equal to 15·43235 grains (or 1 gramme) of water.

TABLE III.—Showing the supply of combined Nitrogen, as Sulphate-of-Ammonia solution, to plants grown in 1858.

Dates.	Nitrogen supplied.									
	Wheat.	Barley.	Oats.	Wheat*.	Barley*.	Oats*.	Pea.	Clover.	Bean*.	Buck-wheat.
	grm.	grm.	grm.	grm.	grm.	grm.	grm.	grm.	grm.	grm.
May 22	·0040	·0040	·0040							
June 7	·0040	·0040	·0040	.....	.....	.....	·0040	·0040		
June 21	·0040	·0040	·0040							
June 26	·0040	·0040	·0040	.....	.....	.....	.....	·0040		
July 3	·0040	·0040	·0040	.....	.....	.....	.....	·0040		
July 12	·0040	·0040	·0040	.....	.....	.....	.....	·0040		
July 14	·0040	·0040	·0040	·0040	·0040	·0040	.. ...	·0040	·0040	
July 19	·0040	·0040	.....	·0040	·0040	·0040	.....	·0040	·0040	
July 28	.....	·0040	.....	·0040	·0040	·0040	.....	·0040		
July 29	·0040									
August 10	·0040	.....	.....	.....	.....	.....	.....	·0040		
August 17	·0036	·0036	.....	·0036	·0036	·0036	.....	·0036	·0036	
August 24	.....	·0036								
August 26	·0036									
September 7	·0036	·0036	.....	·0036	·0036	·0036	.....	·0036	·0036	·0036
October 5	.....	.....	.....	.....	.....	.....	.....	·0036		·0036
October 24	.....	.....	.....	·0036	·0036	·0036	.....	.....	·0036	·0036
Total	·0508	·0468	·0280	·0228	·0228	·0228	·0040	·0428	·0188	·0108

Phosphate-of-Soda solution.—The strength of a dilute solution of phosphoric acid was determined by means of a titrated alkali-solution (for the preparation of which see page 545); and it was then neutralized by carbonate of soda. Each pipette measure of this solution given to the plants supplied about ·01 gramme phosphate of soda. It was only employed in the experiments of 1858. In the records of growth of the plants, it is stated whenever they were manured with this solution.

Sulphuric-Acid solution.—The strength of some very dilute pure sulphuric acid was determined in the same manner as was that of the phosphoric acid, as stated above. It was then so far reduced, that the pipette measure by which it was applied to the plants contained exactly as much SO_3 as the pipette of sulphate-of-ammonia solution then in use, namely, ·0114 gramme SO_3, corresponding to ·004 gramme N. For the application of this solution see the records of growth of the plants.

The value of each of the above solutions was determined by analysis, to ensure that it was such as was supposed.

B.—*Taking up the Plants, preparation for analysis, methods of analysis, &c.*

At the termination of growth the glass shade was washed outside, quicksilver was poured into the groove to displace from it the condensed water not removable by the arrangement of apparatus of 1857, or already collected in the drain-water bottle adopted in that of 1858, as the case might be, and the shade was then removed. The

* These plants were grown in M. G. VILLE's case.

previously covered portions of the slate or stone-ware lute were then washed with pure distilled water, and the wash-water was added to the condensed or drain-water. In the experiments of 1858 this fluid was analysed separately, but in those of 1857 it was mixed and dried down with the soil.

The pot, with the soil and plants, was removed to a clean table covered with white paper, the plants measured in all their parts and then cut off at the surface of the soil; the roots were removed, slightly washed from soil, and observed. The plants were then put into a small wide-mouthed bottle, generally stem and root together, but sometimes they were put into separate bottles. In the experiments of 1857 the contents of the bottles were dried in a water-bath, with a current of air, previously washed through sulphuric acid, passing through the bottle and thence through a solution of a known quantity of pure oxalic acid. But it was found that no appreciable amount of ammonia was thus accumulated. Hence, in 1858, a little oxalic acid (in solution) was added to the vegetable matter, and the whole dried in the water-bath without the above precaution.

When dry, the vegetable matter was cut small by means of a pair of clean long scissors, reaching to the bottom of the bottle. In this way the substance was reduced to a considerable degree of fineness, and it was still further ground up in the mortar when mixed with soda-lime for analysis. When duplicate analyses were to be made, the matter was carefully divided so as to ensure equal proportions of stem, fine leafy matter, &c., in each half. Hence, if both analyses were successfully conducted, the results were mutually confirmatory; or if one portion were lost, the other still represented a proportionate amount of the whole material.

The *soil* was removed from the pot to a porcelain dish, and a sufficient amount of a solution of oxalic acid added to keep it acid. The mixture was then heated on a sand-bath (stirring constantly) until most of the water was expelled, more fully dried in a water-bath, and then preserved in well-corked bottles for analysis. The pots were pounded up; those of 1857 being preserved and analysed separately, and those of 1858 mixed with the soil before it was dried with oxalic acid. The pieces of flint at the bottom of the pot were also pounded and mixed with the soil.

For analysis, 150 to 200 grammes of the soil, pot, or mixture, were mixed with about half the volume of soda-lime, the whole put into a large combustion-tube, some soda-lime put in advance of the mixture, and then asbestos, as usual. The combustions were made in charcoal furnaces, and the ammonia collected in titrated sulphuric acid, of which the strength, and the amounts employed, are described at pp. 545, 546. When very small quantities of nitrogen were involved, the ammonia from two or three tubes of substance was sometimes collected in the same quantity of acid, so as to diminish the error of titration. It was found, however, to be better to use very small quantities of acid, and to estimate the product of each combustion separately; for, by the former method, if any accident occurred in the second or third combustion, it involved the loss of the determination of the products previously collected.

Preparation of the titrated solutions.

A weighed quantity of pure, dry carbonate of soda was dissolved in water, and to the solution water added to a given volume. As a preliminary step, the strength of some dilute sulphuric acid was tested against a given volume of the carbonate-of-soda solution; and from the data thus obtained, by further dilution a large quantity of acid was made of about the strength desired. The exact value of this acid was then ascertained by repeated trials with the standard carbonate-of-soda solution. To accomplish this, a given volume of the soda-solution was put into a beaker, a little litmus added, and the mixture heated over a spirit-lamp. The acid to be tested was then allowed to flow from a burette until a wine-red colour (indicating that the carbonate is converted into sulphate and bicarbonate with carbonic acid in solution) was produced. On boiling, the blue colour is restored; acid is added until red; the boiling is repeated, till the blue returns; acid again added, and so on, until the solution remains red on the addition of the last drop. The point at which the permanent change takes place in the first trial being known, the experiment is easily repeated so as to ensure great accuracy.

Thus, 50 septems of a solution of carbonate of soda, of which 1000 septems contained 6·652 grammes of the salt, required, for neutralization as above, the following number of septems of the dilute acid, in six different trials—

58·3, 58·2, 58·3, 58·3, 58·2, 58·2; mean 58·25.

Hence—

$$\frac{6\cdot652}{1000}\times\frac{50}{58\cdot25}\times\frac{N}{NaO,\ CO_2}=\frac{6\cdot652}{1000}\times\frac{50}{58\cdot25}\times\frac{14}{52\cdot98}=0\cdot001508 \text{ gramme N.}$$

The mean of six experiments with a solution of carbonate of soda of another strength gave in the same way 0·0015008 gramme N; and we adopted the mean, or 0·001504 gramme, as the amount corresponding to one septem of the titrated acid.

It remained to prepare an alkaline solution to test against this standard acid. At first a solution of sugar-lime was employed; but this being found to be liable to constant change, due doubtless to fermentation, a solution of caustic soda was had recourse to. This solution was prepared of such dilution that the extreme error possible in reading off a unit of volume on the burette should be much less than would be admissible as the maximum error of analysis. The burette was of small enough diameter to allow of one-tenth of a septem being read off on it; and the alkali-solution was so dilute that it required about three septems of it to neutralize one septem of the titrated acid. Hence one septem of the alkali-solution corresponded to only about one-half of a milligramme of nitrogen, and the probable error of reading would therefore amount to only about one-twentieth of a milligramme.

In the case of the sugar-lime solution, it was found necessary to test its strength against that of the acid every day that it was employed. But the soda-solution, if properly prepared, and well preserved, remained for months unchanged; so that, when its value was once established against that of the standard acid, it could be expressed

by a number of four or five digits, which, multiplied by the number of septems of alkali representing the product in an analysis, gave the actual quantity, in grammes, of the nitrogen to be estimated.

Amount, and measurement, of the titrated acid used in Nitrogen determinations.

It was desirable that at least three times as much acid should be used as would be neutralized by the ammonia formed. The acid being more concentrated than the alkali, it required a more exact method of measurement than was afforded by the burette used for the latter. Pipettes, of which the diameter at the point of reading off is comparatively small, and which hence admit of a higher degree of accuracy, were therefore employed. In the construction of those to be used, care was taken to maintain the same relation of the diameter of the neck at the point of reading to the entire volume in instruments of different sizes—a condition seldom observed by makers of pipettes. When the quantity of nitrogen involved in an analysis was very small—as in the case of the soils and pots in the experiments without nitrogenous manure—only about six septems of the titrated acid, measured in a small pipette with a very narrow neck, were used. The exact volume of the pipette-ful of acid was not a matter of any consequence. It was only essential to ascertain its exact value expressed in septems of the titrated alkali-solution. When the amount of nitrogen involved was larger, and more under control—as for example when grains were to be analysed—care was taken to operate on such a quantity of nitrogenous material that the number of septems of the alkali representing its nitrogen should be sufficiently large to render the constant errors of titrating, reading, &c., inappreciable. This end was attained when the substance experimented upon contained 5 to 8 milligrammes, or more, of nitrogen.

Combustion-tubes, bulbs, &c.

The combustion-tubes used in the determinations of nitrogen in the soils, pots, &c., were about 3 feet long and about 1 inch in diameter. The bulb-apparatus was capable of holding two-and-a-half to three times as much fluid as that usually employed; but the central and lowest bulb, and particularly its tubular connexions with the other bulbs, were very small, so that a small quantity of liquid could close the passage. This arrangement was necessary owing to the small quantity of acid frequently used, and the large amount of water driven off in the combustion from the large quantities of soil and soda-lime. For the combustion of the experimentally grown plants smaller tubes were employed; and for seeds, &c., ordinary combustion-tubing was used.

The Soda-lime.

Before use, the soda-lime was ignited with 2 per cent. of pure sugar, in order to ensure its freedom from ammonia-yielding matter. It was then slaked with pure distilled water, dried, and kept in well-corked bottles.

Accuracy of the method for the determination of nitrogen by combustion with soda-lime, &c.

In order to ascertain the accuracy of the method before relying upon it for the purposes of the investigation, a few preliminary experiments were made upon the determination of small and known quantities of nitrogen, mixed with large quantities of soil, which had been previously freed from combined nitrogen as in the preparation of the soils for the plant-experiments. The nitrogenous substance taken for the purpose was the powdered crystals of purified quadroxalate of ammonia, $\left\{\begin{matrix}NH_3\\H\end{matrix}\right\}O,(C_2O_3)_4+7HO$. The results were as follow—

Experiment 1.—50 grammes of the prepared soil were mixed with quadroxalate containing by calculation 0·0024 gramme nitrogen; and on burning with soda-lime, and determining as above described, 0·0027 gramme nitrogen was found.

Experiment 2.—100 grammes of the soil mixed with quadroxalate equal, by calculation, to 0·0035 gramme nitrogen, gave on combustion 0·0037 gramme nitrogen.

The error of analysis was, therefore, three-tenths of a milligramme of nitrogen with the 50 grammes, and two-tenths with the 100 grammes of soil. These results were obtained at the commencement of the inquiry, with comparatively large quantities of titrated acid, and therefore before experience had suggested the precautions to be adopted to reduce the errors of determination to the minimum. They may hence be taken as examples of the maximum errors of analysis, but they are less than would affect the bearing of the results in the investigation on the question of assimilation.

Testing for Nitric acid.

The indigo test, as recently refined by Boussingault*, and the protosulphate-of-iron test, were both employed. When nitric acid was sought for and not found, if practicable the negative result was always confirmed by the addition to some of the substance under examination of a quantity of nitric acid (in the form of nitrate) less than could affect any conclusions to be drawn from the fact of its presence or absence in the substance in question. In all the cases of such addition the re-examination showed the presence of nitric acid.

The method of Boussingault was much more delicate than the protosulphate-of-iron test; but, on the other hand, the latter was much less liable to give deceptive indications, dependent on other circumstances than the presence of nitric acid. In using the protosulphate test, the aqueous extract of the substance under examination was evaporated to a small volume with excess of fixed alkali, then transferred to a test-tube, and further evaporated till only a few drops remained. A considerable excess of concentrated sulphuric acid was then added, and on the surface of the liquid a concentrated solution of protosulphate of iron was carefully poured without agitation, by means of a small pipette with a mouth of almost capillary fineness. The characteristic brown tinge indicated the presence of nitric acid.

* Ann. de Chim. et de Phys., vol. xlviii. (1856) p. 153 *et seq.*

C.—Abstract of the Records of Growth of the Plants.

I.—Plants grown in 1857*.

The following list indicates the original arrangement of the experiments in 1857; but, as the records will show, beans sown and resown under shades Nos. 5, 10, and 11 died before they had attained any material amount of growth; and hence the products in these cases were not submitted to analysis.

Series 1. With no other combined nitrogen than that contained in the seed:—

1. Wheat; in prepared soil.
2. Barley; in prepared soil.
3. Barley; in prepared pumice.
4. Beans; in prepared soil.
5. Beans; in prepared pumice.

Series 2. With a supply of known quantities of combined nitrogen beyond that contained in the seed:—

6. Wheat; in prepared soil.
7. Wheat; in prepared pumice.
8. Barley; in prepared soil.
9. Barley; in prepared pumice.
10. Beans; in prepared soil.
11. Beans; in prepared pumice.

And also—

12. Wheat, Barley, and Beans, together; in rich garden soil.

Records of Sowing, and Early Stages of Growth, of all the Plants collectively.

May 12.—The weighed seeds of wheat (Nos. 1, 6, & 7), of barley (Nos. 2, 3, 8, & 9), and of beans (Nos. 4, 5, 10, & 11) were respectively put into small bottles, a few septems of pure distilled water added to soak them, and then corked up.

May 16.—The wheats (Nos. 1, 6, & 7), and the beans (Nos. 4, 5, 10, & 11), were sown, and the pots removed to their places on the stand, and covered with the shades; seeds all swelled; some sprouting.

May 20.—The barleys (Nos. 2, 3, 8, & 9), freshly weighed seeds (the soaked ones being abandoned), were set, and the pots removed to their position under the shades.

May 27.—Nearly all show shoots above the surface, all of which look green and healthy.

June 2.—Wheat and barley plants two or three leaves each, healthy, but pale green. No. 4 beans (soil) healthy and vigorous. No. 5 beans (pumice) one plant up, with three leaves speckled with black spots; the other plant blackened and apparently dead. Beans No. 10 (soil) and No. 11 (pumice) slightly speckled with black spots.

June 3.—Commenced the daily passage of washed air over the plants, in quantity

* The figures (Plate XV.) of the plants grown in 1857 are reduced from drawings taken, for the most part, about the middle of August.

equal to about 2½ times the volume of the shade. Carbonic acid also daily supplied, in amount as described at pp. 480, 481.

June 6.—Graminaceous plants (Nos. 1, 2, 3, 6, 7, 8, & 9) all healthy, though with a tendency to turn yellow at the tips of the leaves. Of the Leguminous plants, Nos. 5, 10, and 11 give indications of dying.

June 8.—Some of the wheat and barley plants turning yellow. Beans Nos. 5, 10, and 11 obviously dying; probably injured by the causticity of the ash added to the soil, as No. 4 beans, the seeds and roots of which happen to be washed when water is supplied, are healthy and vigorous.

RECORDS FOR EACH EXPERIMENT GIVEN SEPARATELY.

No. 1.—*Wheat* (1857); *six seeds; prepared soil; without nitrogenous manure.*

(See Plate XV. fig. 1.)

June 9.—Five plants up; one quite small, the others 2 to 4 inches high, with two leaves developed and a third appearing; yellowish at the tips of some of the leaves.

June 15.—Five healthy plants, each with three fully developed leaves; tips of the lower leaves slightly yellow.

June 24.—Plants 5 inches high; lower leaves dead and dry, upper pale green; with some of the tips yellow, but general appearance of the upper leaves healthy.

July 4.—Plants 6 to 7 inches high; 5 leaves on each; upper ones pale green, lower ones yellow.

[Note.—Drops of water condense rapidly on the tips of the leaves of all the Cereals, but not of the Leguminous plants; they also form and run down the inner surface of all the shades, casting focal rays apparently injurious to the plants when not shaded from direct sunlight.]

July 11.—Same number of leaves; very little further growth; lower leaves more dried up.

July 22.—Very little improvement.

July 29.—Very little growth, though upper leaves continue green; but little tendency to form stem.

[Note.—Shade opened a few seconds to substitute a tube for one accidentally broken.]

August 10.—Green colour maintained, but no apparent increase in size.

August 24.—Five plants, 6 to 9 inches high, with eight or nine leaves each, all dried up but the two upper ones, which are green and healthy, one expanded, the other folded in the axis of growth. The healthy appearance of the upper leaves has been maintained several weeks, with otherwise almost total cessation of growth.

October 3.—*Plants taken up*:—

The plants have been almost stationary since the last report; termination of the ascending axis keeps green; no indication of heading. (See Plate XV. fig. 1.)

Soil moist, soft, and spongy.

Roots not distributed generally throughout the soil; a few isolated ramifications

extended to the lower part of the pot; but the great mass remained near the base of the stem. Total quantity of root very small compared with that of wheat No. 6 manured with ammonia-salts. For general character of root-development, see Plate XV. fig. 15.

For method of further treatment see pp. 543, 544.

No. 2.—*Barley* (1857); *six seeds; prepared soil; without nitrogenous manure.*

(See Plate XV. fig. 2.)

June 9.—Six plants; 2 to 3 inches high, with two fully developed leaves; tips of some of the leaves slightly yellow.

June 15.—Three plants with three leaves, and three with two leaves each; tips of lower leaves slightly yellow, but general appearance healthy.

June 24.—Plants 4 to 6 inches high, with three or four leaves each; much the same condition as wheat No. 1 at this date.

July 4.—6 to 7 inches high, with four or five leaves; paler than wheat No. 1; looking sickly. Drops of water on tips of leaves and inner surface of shade: see Note thereon to wheat No. 1, same date.

July 11.—Lower leaves drying up; upper ones growing a little, apparently at expense of the lower. Stems of these and the other barley plants reddish, and have been so since the formation of true stems with nodes. The barleys form stem more readily than the wheats, which are more leafy.

July 22.—Not much improvement.

July 29.—Only two small leaves at the top green; the amount green at one time does not increase; lower leaves dry up as new ones form.

August 10.—Very little change, except that one stem shows slight indications of heading.

August 24.—*Plants taken up*:—

Six plants, 5 to 17 inches high, with six to nine leaves on each plant. Two indicate slight tendency to heading, the sheath being swollen; but growth obviously ceased, the two upper leaves having at last lost colour and dried up. On opening, one head showed a rachis 2 inches long. The plant was very dry, so no fresh weight taken.

Prepared and analysed as described at pp. 543, 544.

No. 3.—*Barley* (1857); *six seeds; prepared pumice; without nitrogenous manure.*

(See Plate XV. fig. 3.)

June 9.—Six plants, 2½ to 4 inches high; more developed, but more slender than the barleys in soil (Nos. 2 & 8). Leaves turning yellow at the tips.

June 15.—Six plants, 6 inches high, each with three fully developed leaves; tips of lower leaves dried up; middle leaves have yellow tips; upper ones pale green but healthy. Plants appear to have almost done growing.

June 24.—Height about the same; three or four leaves each plant; lowest dried up, next drying, and upper ones green.

July 4.—Plants 6 to 7 inches high; five or six leaves each; upper leaves only pale green.

[Drops of water collect as described in reference to No. 1 Wheat at this date.]

July 11.—Plants 6 to 8 inches high; five or six leaves each; upper ones green and growing a little as the lower ones dry up; general aspect stationary.

July 22.—Very little growth.

July 29.—Plants 8 to 10 inches high, very slender, like mere threads; all lower leaves dried up; upper ones 1 to 2 inches long and pale yellow. The six plants show twenty nodes. Slight tendency to form very small heads.

August 10.—Plants quite dried up.

August 25.—*Plants taken up*:—

Six very slender plants, mere filaments, 8 to 20 inches long; with four to six nodes, and six to eight leaves each. Stems zigzag at the nodes; leaves dried up and brown. The top sheath of five of the plants indicates an excessively small head with zigzag rachis, at the upper part of which is a well-defined husk but no seed; the lower parts have beards and small rudimentary husks.

Preparation and analysis as described at pp. 543, 544.

No. 4.—*Beans* (1857); *two seeds; prepared soil; without nitrogenous manure.*

June 9.—Two plants up; one 6 inches high, four leaves with two leaflets each and two large stipules; the other smaller; both healthy and vigorous.

June 15.—One plant 7½ inches high, with five leaves, each with two or three leaflets and two stipules; the other 3½ inches high, with four leaves and corresponding stipules. Tips of some of the lower leaves slightly speckled, but the upper ones green, and both plants healthy and vigorous.

June 24.—One plant 15 inches high, with seven leaves, each with two or three leaflets and two stipules; lower leaves yellow, with dark specks at the edge, upper leaves and stem light green; the other plant 9 inches high, four or five leaves with two to three leaflets, &c., each; lower leaves as on the other plant, but upper ones greener. Plants appear to have nearly done growing.

July 4.—One plant 19 inches high; five leaves fallen off within two days, three upper ones remain, these green, appear to live on nutriment drawn from the lower ones. The other plant 12 inches high, seven leaves, and a small sprout just at the surface of the soil; lower leaves dead, upper ones nearly done growing.

July 5.—*Plants taken up**:—

Preparation and analysis as described at pp. 543, 544.

* After removal of the beans, a barley plant from the field was potted with its own soil which was comparatively dry, and placed under the shade without being watered, in order to see whether water was given off and condensed within the glass as freely as in the case of the experimental plants. It was so; and hence it was concluded that the experimental soils were not too wet.

No. 5.—*Beans* (1857); *two seeds; prepared pumice; without nitrogenous manure.*

June 9.—One plant 1½ inch high, blackened, and dying; the other smaller and already dead.

As will be seen by the records (p. 557), Beans Nos. 10 & 11 showed equally unhealthy growth; all were therefore removed and re-planted. It was obvious that the failure was too early to be due to want of available nitrogen; especially, as No. 4 Beans with a similar amount of nitrogen lived. The result was considered to be due to the causticity of the ash, as beans set in ash-free soil and pumice flourished much longer, and in the case of No. 4 the seeds happened to be so placed as to be washed when water was applied.

It was found on examination that all showed signs of recommencement of growth; new roots and stems were forming. The seeds, &c. were removed; a little sulphuric acid added to the soil (or pumice) to neutralize the ash, and it was then ignited as originally, put into fresh red-hot pots, and cooled and moistened over sulphuric acid. Before putting in fresh seeds, holes were made for them in the soil, and water poured in to remove soluble matter from the neighbourhood of the young rootlets. The experiments were then continued as before.

Report of No. 5 Beans continued.

June 24.—One plant just up.

July 1.—An accident occurred to this experiment. A fresh pot of soil, prepared precisely as above, was planted with beans that had been set in small glass tubes ready for any contingency, and the experiment continued.

July 4.—One plant, leaves just opening.

July 11.—Still only one plant up, and it looks very unhealthy.

July 22.—One plant, obviously dying.

July 29.—Dead.

No. 6.—*Wheat* (1857); *three seeds; prepared soil; with nitrogenous manure.*

(See Plate XV. fig. 7.)

June 9.—Two plants up; one 2½, the other 4½ inches high; three leaves each. Tips of leaves slightly yellower than those of Wheat No. 1.

June 10.—A pipette-ful of the solution of sulphate of ammonia (=·00578 gramme N.) added to the soil.

June 15.—Two plants; green and vigorous; marked improvement since the addition of ammonia-salt; the leaves wider and of a deeper green. Three leaves each plant.

June 24.—Two plants; 7 inches high; four or five leaves each; lower ones dried up, upper ones deeper green than Wheats No. 1.

July 4.—Two plants; 9 inches high; six leaves each; lower ones yellow, upper ones broad, long, and of a healthy deep green; but the vigour due to the first addition of

ammonia appears to have ceased. Second pipette-ful of ammonia-solution (same quantity) added.

July 11.—Two plants, 10 inches high; seven leaves each; upper ones deep green, broad, and vigorous. Third pipette-ful of the ammonia-solution added.

July 22.—Growth vigorous; shooting out at the base of the stems. Fourth pipette-ful of the ammonia-solution added.

July 29.—Much greater tendency to form leaf than stem. One plant with four, and the other with two subdivisions. 12 to 16 inches high, the height greatly due to the length of the leaves. Not a single node clear of the sheath of the one below it; thus essentially different from the barleys, which have great tendency to form nodes and stem. Fifth pipette-ful of the ammonia-solution added.

August 10.—Green and flourishing.

August 24.—Plants 17 to 20 inches high; ten to twelve leaves on each; upper ones long, broad, and green; lower ones dried up. But little tendency to form stem; leaves larger than on plants in the field; some 12 inches long and $\frac{1}{2}$ inch wide; no nodes clear; the leaves spring out so close together as to appear almost opposite. Five stems from the two seeds.

October 2.—*Plants taken up*:—

One seed has given three strong and one small stem; another one stem; the third did not grow. Leaves very numerous and close together, giving several thicknesses of sheath around the stem, and hiding all the nodes; lower leaves dried up; upper leaves and central axis of growth green. Condition nearly stationary for the last two or three weeks. Average height of plants about 18 inches.

Soil quite moist throughout; also soft, and spongy, rather more so than the pumice soils; a little water remained in the plate below the pot.

Roots much, but very irregularly distributed—a large bunch around the base of the stem; small, long, isolated roots extended to the bottom and up the sides of the pot; quite a mass of ramified roots over the bottom, and somewhat up the sides of the pot; and a greater mass in the dish under the pot, forming a circular web the size of the bottom of the pot. A crack in the bottom of the pot was penetrated with roots throughout, showing, perhaps, that more openings than the one hole at the bottom might be advantageous. For representation of the root-development, see Plate XV. fig. 14.

Preparation and analysis as described at pp. 543, 544.

No. 7.—*Wheat* (1857); *three seeds; prepared pumice; with nitrogenous manure.*

June 9.—Three plants up, 3 to 4 inches high; each with three leaves completely formed, of which the tips are slightly yellower than those of Nos. 1 and 6, but no appearance of diseased condition in any of the wheats.

June 10.—A pipette-ful of the ammonia-solution (=·00578 gramme N.) added to the soil.

June 15.—Plants 5 to 6 inches high, with four leaves each; the tips of the lower

ones yellow; the newer and upper leaves green, healthy, and vigorous; marked improvement since adding the ammonia-solution on June 10, the effect of which was manifest within two days after the addition.

June 24.—Plants 5 to 7 inches high, with four leaves each; lower leaves dried up, but upper ones green and vigorous; obviously improving; forming stem with nodes.

July 4.—Plants 7 to 8 inches high, with five to seven leaves each; the newer ones broad, well developed, and of a deep green colour; upon the whole vigorous. Second pipette-ful of the ammonia-solution added.

[Drops of water accumulate as described in reference to No. 1 of this date.]

July 11.—Plants 8 to 9 inches high, with six or seven leaves each; lower ones pale yellow, upper ones green and vigorous. One of the stems sending out a shoot at its base. Third pipette-ful of ammonia-solution added.

July 22.—Growing very well; tillering very much. Fourth pipette-ful of the ammonia-solution added.

July 29.—Plants 12 to 16 inches high; one with six shoots 4 to 8 inches long; one with one shoot 3 inches long; and the other with two shoots just forming; shoots, and upper leaves, green. The ammonia seems to induce multiplication of shoots instead of upward growth; no nodes clear of the sheath. Fifth pipette-ful of the ammonia-solution added.

August 10.—Green and flourishing.

August 24.—Very similar to Wheat No. 6 at this date.

September 20.—*Plants taken up*:—

The lower leaves begin to lose colour considerably, no increase of growth apparent for some days, nor any tendency to form seed; hence, the season being far advanced, the plants taken up.

Great development of root; the plate under the pot covered with a dense network ramified from a few fibres extended to the bottom of the pot; a similar network at the bottom and partially up the sides within the pot; comparatively little in the centre of the soil.

Preparation and analysis as described at pp. 543, 544.

No. 8.—*Barley* (1857); *four seeds; prepared soil; with nitrogenous manure.*
(See Plate XV. fig. 8.)

June 9.—Three plants up; two 1½ inch and one 3½ inches high; colour pale.

June 10.—A pipette-ful of ammonia-solution (=·00578 gramme N.) added to the soil.

June 15.—Three plants; about 4½ inches high; each with two leaves and another forming. Improved by the ammonia added June 10, but not so much as the Wheat No. 6.

June 19–20.—During thenight the shade was cracked, from the bottom in the quicksilver, 9 inches upwards. The pot with its contents was removed and put under a shade over sulphuric acid. After four days it was returned to its place, and covered with the

shade of Experiment No. 12 (with plants in garden soil), the latter being replaced by the damaged shade after the crack had been mended with strips of bladder cemented with albumen and lime-water. All the circumstances of this accident were carefully considered, and it was concluded that no appreciable error could arise from it.

June 24.—Three plants, 3 to 5 inches high; three or four leaves each; lower ones dried up, upper ones pale green; plants slender, but improved since the addition of the ammonia-solution.

July 4.—Plants 6 to 7 inches high; five leaves each; the most delicate and slender of the plants that have had ammonia-solution; upper leaves darker green than those without ammonia; lower leaves yellow. Second pipette-ful of ammonia-solution added.

[The same remarks apply here, as were made to No. 1 at this date, in reference to condensation of drops of water.]

July 11.—Plants 7 to 9 inches high; six or seven leaves each; stem reddish; upper leaves healthy and deep green. Third pipette-ful of ammonia-solution added.

July 22.—Growing vigorously. Fourth pipette-ful of ammonia-solution added.

July 29.—Four plants, 16 to 20 inches high. Since the last two additions of ammonia-solution, two of the plants have sent out at the base two new shoots, 6 to 8 inches high; one, two new shoots 2 to 4 inches high; and the other, one shoot. All these shoots are deep green and growing vigorously. A great tendency to develope new foliage; and though some of the stems were just beginning to swell, indicative of heading, and one showed a beard, yet this growth was arrested, and the energies of the plant directed to the new growths at the base. In all, seventeen nodes clear of the sheaths. Fifth pipette-ful of the ammonia-solution added.

August 10.—Since the last three additions of ammonia the old stems ceased to develope, but some of the new ones are on the point of heading.

August 24.—Eight plants from the three seeds. One seed has given one plant 24 inches high, with seven nodes clear, and nine leaves, of which the seven lower ones are dried up; the plant terminated by a well-formed head. Another seed has four stems, 16 to 20 inches high; one dried up just as it was heading; the three others green and healthy, and two just commencing to head; each stem four to six nodes, and six to ten leaves. The third seed has three stems 12 to 24 inches high, each with three to five nodes and five to ten leaves; one stem dried up.

October 8.—*Plants taken up*:—

Eight stems from three seeds, as under:—

(*a*) Seed with one stem; 18 to 20 inches high; seven nodes. This was the first plant that headed; all ripe and dry; six glumes, containing only rudimentary or undeveloped seeds.

(*b*) Seed with three stems. One 17 inches high; head ripe, and rather decaying. Another 25 inches high; grown several inches, and formed head, since August 24; head green, with five soft milky unripe grains. The third stem green at top, and upper sheath swollen with the head.

(*c*) Seed with four stems:—(1) 22 to 23 inches high, with green head and six unripe grains; leaves dry and ripe; (2) stem 15 inches high, dried up, head-sheath formed; (3) 19 inches high; yellowish-green head, with nine glumes, and undeveloped seeds; (4) about 15 inches high; rather green, sheath swollen, and beard appearing.

During the last three weeks some heads came out more, and indications of others developed; otherwise not much change. From the low temperature and lateness of the season, it was thought the plants would not mature further.

Preparation and analysis as described at pp. 543, 544.

No. 9.—*Barley* (1857); *four seeds; prepared pumice; with nitrogenous manure.*
(See Plate XV. fig. 9.)

June 9.—Four plants; one quite small; the others 3 to 4 inches high. These more grown than the Barley plants Nos. 2, 3 & 8; but the leaves, particularly the lower ones, yellower at the ends.

June 10.—A pipette-ful of ammonia-solution (=·00578 gramme N.) added to the soil.

June 15.—Four plants; 5 to 6 inches high; four leaves each; lower ones losing vitality. Lower leaves were too far gone, but a most marked improvement in the upper ones since the ammonia-salt was added; it was manifest in two to three days after the addition.

June 24.—Four plants; height 6 to 8 inches; improved very much by the addition of the ammonia-solution.

July 4.—Plants 8 to 13 inches high; six or seven leaves each; stems very slender, but show well-formed nodes. Second pipette-ful of ammonia-solution added.

[Drops of water accumulate as described in reference to No. 1 of this date.]

July 11.—Plants 9 to 14 inches high; seven or eight leaves each; upper ones deep green; lower ones yellow; stems red. Third pipette-ful of ammonia-solution added.

July 22.—Growing very well; showing indications of heading. Fourth pipette-ful of ammonia-solution added.

July 29.—The four plants all out in head; about 30 inches high; each stem six nodes; two of the plants have shoots 5 inches high. The ammonia seems to tend more to new growth than to the development of the old.

August 10.—Heads well developed.

August 24.—The plants appear to be ripening; heads turning brown; but one new stem is still green and growing.

September 24.—*Plants taken up:*—

Seven plants; five 2 to 2½ feet high, one green; one 1½ foot high, green head; one 14 inches high, green. Six with heads, four ripe and two green; the shortest plant with green leaves and without head. Heads 1½ inch long; glumes all along the rachis, but only some with grains.

Roots by no means so abundant as those of Wheat with ammonia-salt; only a few fibres extended through the hole at the bottom, or to the sides of the pot.

Preparation and analysis as described at pp. 543, 544.

No. 10.—*Beans* (1857); *two seeds; prepared soil; intended to have nitrogenous manure.*

June 9.—Only one plant up; 2 inches high; turning black and obviously dying.

For particulars of taking up, setting fresh seeds and recommencement of the experiment, see remarks made on June 9 to Bean No. 5, p. 552.

June 15.—Not yet up.

June 24.—Two plants just appearing.

July 4.—Two plants well up and growing; leaves just opening.

July 11.—Two plants; 6 to 8 inches high; leaves deep green.

July 22.—Green, healthy, and vigorous.

July 29.—Nearly as at last date, but somewhat declining.

August 10.—Obviously dying.

August 24.—Dead.

The season too far advanced to repeat this experiment.

No. 11.—*Beans* (1857); *two seeds; prepared pumice; intended to have nitrogenous manure.*

June 9.—One up; slender; black spots on the leaves; obviously unhealthy. Taken up, and the experiment recommenced; for particulars of resetting, &c., see remarks to Bean No. 5 of this date, p. 552.

June 15.—Not yet up.

June 24.—Two plants just up.

July 11.—Apparently not going to grow.

July 22.—Dead; the season too far advanced to repeat this experiment.

No. 12.—*Wheat, Barley, and Beans* (1857); *Wheat and Barley three seeds each, Beans two seeds; in rich Garden soil.* (See Plate XV. fig. 13.)

May 18.—Seeds of wheat, barley, and beans, all sown together in a single pot of good garden soil, and placed under a shade (No. 12), to be supplied with washed air, &c., just as in the other experiments. The seeds germinated well.

May 28–29.—During the night, owing to a leakage of water from the reservoir into the vessel A (see description at p. 476 *et seq.*, and Plate XIII.), it passed over into the sulphuric acid and carbonate of soda wash-bottles, and the mixed liquid passed into the shade to the depth of some inches, and destroyed the experiment.

May 30.—Plants from seeds which had been set at the same date as the foregoing, were transplanted into a fresh pot of garden soil, which was placed under the shade, and the experiment recommenced. The wheat and barley plants were about 5 inches, and the beans about 4 inches high.

June 15.—Healthy, and growing vigorously.

June 24.—Three wheats, three barleys, and two beans. Wheat 14 inches, barley

13 inches, and beans 11 inches high. Wheat and barley much branched at the base, giving fourteen stems from the six seeds; all a deep green colour. Beans deep green, and growing well, excepting that one has a few black specks on the lower leaves. So much growth that the plants are considerably crowded in the shade.

July 4.—Much crowded. Graminaceæ 20 inches, Leguminosæ 15 inches high. The former growing as well as in the open air. The latter appear to suffer from crowding; their lower leaves dying.

July 12.—The Graminaceæ growing very healthily; Leguminosæ apparently not so.

July 22.—The Graminaceæ growing vigorously; Leguminosæ revived, and also growing vigorously at the top. During the last few days they have been protected from the direct sun by a sheet of paper tied round the shade.

July 29.—Four barleys in head; wheat not so advanced, but nearly as high; the beans had again suffered, but one is recovering. Too much crowded.

August 10.—About as at last date.

August 24.—About as at last date; barley slowly ripening.

The object of the experiment being attained, which was to determine whether the conditions of atmosphere were suited to healthy growth, provided the soil supplied sufficient nutriment, no further records of growth were made.

II. Plants grown in 1858*.

As in the experiments of 1857, so in those of 1858, the plants grown may be divided into two Series, as under:—

Series 1. With no other combined Nitrogen than that contained in the seed sown.

Series 2. With a supply of known quantities of combined Nitrogen beyond that contained in the seed.

The notes of growth of the plants grown without any extraneous supply of combined nitrogen are given first, and then those of the plants grown with such supply. As before, in several experiments instituted with Leguminous plants they died before attaining a sufficient amount of growth to render it of any use to analyse the products. The records of their progress, such as it was, are, nevertheless, shortly given.

No. 1.—*Wheat* (1858); *eight seeds; prepared soil; without nitrogenous manure.*

(See Plate XV. fig. 4.)

April 27.—Seeds set, and the pot placed under a shade over sulphuric acid.

May 7.—All the plants up; the pot removed to its shade on the stand.

May 20.—Eight plants; all of a healthy green colour; seven 4 inches high, one just above the soil.

* The figures (Plate XV.) of the plants grown in 1858 are reduced from drawings taken, in most cases, not many days before the plants were taken up.

May 22.—A pipette-ful of the sulphuric-acid solution added.

May 29.—Eight plants, 4 to 6 inches high; each with four leaves, the two lower yellow, the two upper green and healthy. A drop of water appears on the tip of the upper leaves in the morning, but it disappears before midday, as the air is passed through the shade. A pipette-ful of the phosphate-solution added.

June 7.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added.

June 19.—Plants 5 to 7 inches high; two lowest leaves on each dried up; upper ones yellowish green.

June 26.—Eight plants, 6 to 7 inches high; six leaves each, three lower ones dried up, next two pale green, only upper central one green and healthy. Apparently at limit of growth without more combined nitrogen; very much as last year without nitrogenous manure.

July 3.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added.

July 14.—Plants 6 to 8 inches high, with six or seven leaves each; only the two upper ones yellowish green; apparent stagnation of growth.

July 29.—Much as last; two upper leaves seem to sustain life at the expense of the rest.

August 17.—After long inactivity several plants show tendency to grow in *stem*. In this, somewhat more like the barley than wheat of last year. Some disposition to heading.

September 7.—Still developing stem; nodes and internodes distinctly marked. Plant (*a*) 13 inches high, ten leaves, three nodes bare, slightly swelled at top as if heading; new stem-leaves, only 2 to 3 inches long. Plants (*b* and *c*) 9½ inches high, nine leaves, two or three bare nodes; slight indication of heading. Plants (*d*, *e*, and *f*) 7½ inches high, two bare nodes; stems shorter, leaves eight or nine, a little longer than above. Plant (*g*) two branches; the first short, and dried up; a new one formed from its base, green, but only 4½ inches high, with four green leaves. Plant (*h*), dried up stem with three long leaves; but a new green shoot with two leaves, though little growth. General remark:—all lower and first-formed leaves dried up, the next yellowish, and only the two upper ones green. Drops of water collect at the tips, and axils, of the green leaves. The later growth obviously at the expense of the earlier.

October 5.—Little change, except riper. Plant (*a*) 14 inches high, eleven leaves, nearly all dried up, four bare nodes, a head with indications of seeding: (*b*) 10½ inches high, eleven leaves, all ripe but the uppermost, three bare nodes, and indication of heading: (*c*) 9½ inches high, nine leaves, three nodes: (*d* and *e*) 8½ inches high, eleven leaves each: (*f* and *g*) 4 to 7 inches high, dead stems with eight to ten leaves each, but green shoots at the base: (*h*) 7 inches high and seven leaves, dead ripe.

October 24.—Weather much warmer again lately, and slight renewal of growth; drops of water again appear on the green top leaves. The chief growth is further deve-

lopment of the rudimentary head; a definite rachis formed, with joints and rudimentary husks, but no indication of seed.

October 25.—*Plants taken up*:—

Soil quite wet, loose, and open, to the bottom; roots pass through the pot at nearly all the bottom holes, and at some of the side ones; long roots distributed among the flints; very few roots come to the sides of the pot (see Plate XV. fig. 17).

Plant 1. Dead ripe, 7 inches high, seven long leaves, one dead shoot; roots long, apparently going to the bottom, very little distributed.

Plant 2. Seven inches high; two stems; one with six leaves, dead ripe; the other with three leaves, one still slightly green; no nodes visible; each, a moderate amount of root.

Plant 3. Eight inches high; ten leaves, lower long and dead, two upper green; no nodes visible. Many roots at the base, some extending downwards. Roots of this and all the plants have short forked branches, $\frac{1}{4}$ to $\frac{1}{2}$ inch long, blunt and thick, and generally forked at the end; strikingly different from the roots among the loose flints at the bottom, and those under the pot.

Plant 4. Height $10\frac{1}{2}$ inches; thirteen leaves; six visible nodes; slight swelling at the head. Fewer roots branched and distributed in the soil near the base of the stem; most go to the bottom, or even under the pot, thus taking nutriment from the water in the dish rather than from the soil;—perhaps associated with this the superior growth over plants 1, 2, and 3.

Plant 5. Very similar to No. 4.

Plant 6. Very similar to Nos. 4 and 5; but the head rather more developed, and visible through the transparent sheath, and the roots with rather more the character of pot or *soil* roots.

Plant 7. Eleven inches high; twelve leaves; five nodes visible; head with chaff without grain, and beard $\frac{3}{4}$ inch long; rachis 1 inch long. Roots but little branched, going down and developed more at the bottom and in the dish than in the soil.

Plant 8. The largest and most developed plant. Fourteen inches high; twelve leaves; lower ones long and crowded, upper ones shorter and further apart (as in all); four nodes; head with rachis $1\frac{1}{4}$ inch long, with glumes and pales. Roots very similar to No. 7, forming under the pot a thick matted mass, running round the dish, some of which, when untangled, are 3 to 4 feet long; white, transparent, and with many small thread-like branches; the whole somewhat resembling a mass of white thread.

Preparation and analysis as described at pp. 543, 544.

No. 2.—*Barley* (1858); *eight seeds*; *prepared soil*; *without nitrogenous manure*.
(See Plate XV. fig. 5.)

April 27.—Seeds set, and the pot placed under a shade over sulphuric acid.

May 7.—Pot removed to its shade on the stand.

May 20.—Five plants 4 inches high, and one 1 inch. Were at first quite green and healthy, but the last few days turning yellowish green.

May 22.—A pipette-ful of the sulphuric-acid solution added.

May 29.—Five plants 4 to 5 inches high, with three or four leaves each; lower ones yellow and dried up; upper pale yellowish green. A sixth plant, smaller. A pipette-ful of the phosphate-solution added.

June 7.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added.

June 19.—One plant dead; two about 4 inches high with shoots at the base; other two about 8 inches high.

June 26.—Plant (*a*) dead; cause not obvious. Plant (*b*) 10 inches high; as last year, forming stem well. Plant (*c*) 8 inches high. Plant (*d*) a main stem which is dead, and a new shoot which is green (each 3 to 4 inches high). Plant (*e*) a good deal like (*d*).

July 3.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added.

July 14.—Plant (*b*) 9 to 10 inches high; six dried up, and two green leaves; swelling apparently for heading. Plant (*c*) about 7 inches high; seven dried up and two green leaves. Plant (*d*) two stems 4 to 6 inches high; six dried up and two green leaves. Plant (*e*) two stems 4 to 6 inches high, with five dead and two green leaves.

The upper leaves quite short (1–1½ inch long), and apparently live at the expense of the lower.

July 29.—Plant (*a*) dead; six leaves, becoming brown-yellow; a black mildew has attacked the leaves and stem; and a white gossamer-like fungus has attached itself in places to the stem and leaves. Leaves 3¼ to 4 inches long; the upper thread-like and drooping. Plant (*b*) the most flourishing; 14 inches high; but very spindly; six nodes, which, with portions of the adjoining culm, especially the upper part, are dark purplish; eight leaves; lower ones yellow, and the lowest two, which are in contact with plant (*a*), affected with the mildew; all but the uppermost leaf 2 to 2½ inches long; the upper one 1½ inch long, pale green, and quite erect, apparently the last effort of the plant, no new leaves forming. Plant (*c*), divided just beneath the soil into three shoots; two apparently suckers from the other, each 3 inches high, and dead. The main plant 6 inches high; has seven leaves; the four lower dead, and the three upper, making up half the plant, pale green; the uppermost only ½ an inch long, in the fold of the second. Only one node visible; the culm, where seen, is purplish. The white fungus occurs, but no mildew. Plant (*d*) much like the main plant (*c*); evidence of early effort to put out shoots at the base. Twelve leaves; ten lower ones dead; two upper ones living; all 2 to 2½ inches long. Plant (*e*) the second in size. Eleven inches high; ten leaves; eight lower ones dead, two upper ones living; all erect but the lowest two; each 2 to 3 inches long.

August 18.—*Plants taken up*:—

Evidently done growing; four stems swelled for head; all leaves except the uppermost dried up. Roots not much distributed; general characters much like those of barley without nitrogenous manure last year (1857). Soil moist, loose, and open.

Preparation and analysis as described at pp. 543, 544.

No. 3.—*Oats* (1858); *eight seeds; prepared soil; without nitrogenous manure.*
(See Plate XV. fig. 6.)

April 27.—Seeds set, and the pot placed under a shade over sulphuric acid.

May 7.—The pot removed to its shade on the stand.

May 22.—A pipette-ful of the sulphuric-acid solution added.

May 29.—Eight plants, 4 to 6 inches high; four or five leaves each; lower ones yellow, upper ones green and growing. These Oats growing rather better than either No. 1 Wheat, or No. 2 Barley. A pipette-ful of the phosphate-solution added.

June 7.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added.

June 19.—Eight plants, 6 to 9 inches high; five or six leaves each, lower yellow and dead, upper green. Tips of some of the leaves injured by action of direct sun-rays.

[General note.—White paper had been tied over all the shades to screen from the direct rays of the sun; but in this case not quite high enough.]

June 26.—Eight plants; five 10 to 11 inches high, and in head; three 8 to 9 inches high; no appearance of heading, and two of them a green shoot at the base. Six or seven leaves on each plant. The rachis of the seeding plants long and crooked, with one or two seeds at top, without signs of seed below. All the plants apparently at termination of growth; remain only to see how far they will ripen.

July 3.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added.

July 13.—*Plants taken up*:—

Eight plants, quite dead ripe for some days, having had a hot sun.

Plant (1) 13½ inches high; five leaves; rachis 1½ inch long, with one seed. Plant (2) 11½ inches high; five leaves; rachis 1½ inch. Plant (3) 12½ inches high; five leaves; rachis 1½ inch long, with two seeds. Plant (4) 12½ inches high; with shoot appearing at base; rachis 1½ inch long, with two seeds. Plant (5) 11½ inches high; five leaves; with shoot appearing at base. Plant (6) 9 inches high; five leaves; and shoot at the base, 4 inches long. Plant (7) 10 inches high; five leaves; and shoot at the base 4 inches long. Plant (8) 10½ inches high; five leaves; rachis 1½ inch long, two seeds. Roots only extended about 2 inches deep in the pot. Soil wet and soft; the lower part firm, but not hard.

Preparation and analysis as described at pp. 543, 544.

No. 4.—*Beans* (1858); *three seeds; prepared soil; without nitrogenous manure.*

April 27.—Seeds set, and the pot placed under a shade over sulphuric acid.

May 20.—Pot removed to its shade on the stand. Three plants up, 2½ inches high; three leaves on each; dark green and healthy.

May 22.—A pipette-ful of the sulphuric-acid solution added.

May 29.—Plants 3 to 4 inches high; one looks to be dying; the others have specks on their leaves. A pipette-ful of the phosphate-solution added.

June 7.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added to the soil.

June 19.—One plant dead; another looking unhealthy; the third 4 to 5 inches high, with five leaves, growing pretty well.

June 26.—Two plants dead; the other growing, 7 to 8 inches high, with nine leaves, each with two stipules.

July 3.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added.

July 14.—The third or only surviving plant has ten leaves, but looks unhealthy.

July 29.—The two dead plants fallen, moulded, and dried up. The other blackened and mouldy at the base of the stem, and thence to the top yellow; three top leaves partly yellow, but the remainder black.

August 17.—All three entirely dead. Pot removed, but products not analysed, as there had not been sufficient healthy growth. It is difficult to account for this failure; but it is possibly due to the very hot weather.

No. 5.—*Beans* (1858); *three seeds; prepared soil; without (but intended to have) nitrogenous manure.*

June 11.—Seeds set in prepared soil, with ash that had been neutralized with sulphuric acid, and gently re-ignited; and the pot placed over sulphuric acid and covered with a glass shade.

June 21.—The pot removed to its place on the stand.

June 26.—Three plants up; green and healthy; four leaves, each with two leaflets, and two stipules. Plants delicate, but healthy green colour; one shows air-roots.

July 3.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added.

July 14.—Three plants, healthy and vigorous; 8 to 12 inches high; eight leaves on each; a few black specks on some of the leaves, otherwise healthy. The weather has been comparatively cool since planting till now; but now hotter with bright sun. A few air-roots at the base of the stems.

July 29.—Three plants; 8, 8½, and 12 inches high. Plant (*a*) lost all its leaves, except rudimentary ones at the top. A shoot 2 inches long with four small leaves about an inch from the base, more growing than the parent plant; another shoot appearing about an inch above. Plant (*b*) very unhealthy; lost all leaves but six small and partly black ones at the top; a vigorous shoot 5 inches long, springing an inch from the base, seems to exhaust its strength; another small shoot 1 inch long, about 2 inches higher up. Plant (*c*), most of the leaves dropped; but several of the petioles remain, and are green; some small withering leaves at the top; two shoots starting near the base.

August 17.—Three plants; the main stem of each lost nearly all the leaves. Each plant has living shoots with several leaves each near the base.

August 23.—*Plants taken up*:—

There has been scarcely perceptible growth for two or three weeks; leaves nearly all off. Soil moist. Roots extend only a little way, and consist of a thick mat around the

base, much divided; none reached the flint; the upper ones dead, the lower living; less root than last year.

Preparation and analysis as described at pp. 543, 544.

No. 6.—*Pea* (1858); *three seeds; prepared soil; without nitrogenous manure.*

June 5.—Three peas previously set died. Three more set to-day in prepared soil, with ash that had been neutralized with sulphuric acid, and gently re-ignited; and the pot placed over sulphuric acid and covered with a glass shade.

June 7.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added.

June 19.—Pot removed to its place on the stand.

June 26.—Three plants, 6 to 7 inches high, with four leaves each; not growing well.

July 3.—A pipette-ful of the phosphate-solution, and a pipette-ful of the sulphuric-acid solution added.

July 14.—Three plants; doubtful whether they will live.

July 29.—Three plants; two 6 inches, and one 7 inches high; apparently dead some days, yellow, and a few spots of mould.

August 24.—*Plants taken up*:—

All dead for some time past; probably owing to the heat. Products submitted to analysis, but the results can only be of confirmatory value.

No. 7.—*Buckwheat* (1858); *seed*, 1 *gramme; prepared soil; without nitrogenous manure.*

August 20.—Seed sown, and the pot placed over sulphuric acid, and covered with a glass shade.

August 24.—Removed to its shade on the stand. Several plants up.

September 7.—Growing well.

October 5.—Sixteen plants, 2 to 4 inches high, four to six leaves each.

October 24.—Eighteen plants, 3 to 4 inches high, with four to six leaves each; leaves $\frac{1}{4}$ to $\frac{3}{4}$ inch wide, but have begun to look yellow and curl up; some plants dead. The plants appear to have attained their maximum growth without nitrogenous manure.

October 28.—*Plants taken up*:—

Eighteen plants with four to six leaves each, including the seminal opposite ones; 2 to 3 inches high; obviously done growing; only five or six with green leaves remaining. Roots only 2 to 3 inches long, slim, delicate, and very little distributed. Soil quite loose, porous, and friable.

Preparation and analysis as described at pp. 543, 544.

No. 8 (1858).—Plants grown without Nitrogenous Manure in M. G. Ville's Case*.

M. Ville kindly forwarded porous pots, and glazed white pans to set them in, such

* The experiments conducted in M. G. Ville's cases were commenced later in the season than those with the shades, as we waited some time in the hope that M. Ville might be able to come over and superintend the arrangement himself.

as he used in his experiments; but as too many were broken in transit to use these entirely, it was decided to use pots and pans the same as for our other experiments in 1858. The soil, ash, &c., were also prepared in the same way as for the other experiments of 1858—the ash, however, being saturated with sulphuric acid and re-ignited before being used, as in a few only of the other cases; for description, see pp. 470–472.

Pot 1—Wheat; eight seeds.
Pot 2—Barley; eight seeds.
Pot 3—Oats; eight seeds.
Pot 4—Beans; three seeds.

June 11.—Wheat, Barley, and Oats; seeds set, and the pots placed over sulphuric acid, and covered with a shade.

June 12.—The three pots removed to M. VILLE's Case.

June 19.—Wheat, Barley, and Oats all up, and looking green and healthy.

June 25.—Cereals all about the same size, with three leaves each; wheat quite pale, barley and oats green at the top, and lower leaves dead.

The Case was opened, and the pot of three beans put in.

July 1.—Wheat pale and blanched; barley and oats, lower leaves dead, upper ones green and growing.

July 8.—All the Cereals getting quite pale; beans healthy.

July 14.—Bean, three plants up; two cotyledons and two leaves on each, healthy. Wheat, seven plants; 4 to 6 inches high; four leaves on each, upper green, lower dead. Oats, seven plants; very like the wheat; but little more disposition to form stem. Barley, eight plants; much like the oats.

August 2.—Beans, 4 inches high; lower leaves dead, and show a white mould; small stems putting out languidly. Barley, eight plants; 4 to 5 inches high; lower leaves yellow and dead; upper blades green; stems appear mouldy. Oats, seven plants about 5 inches high; lower leaves dead and mouldy; upper part of stem green, and slightly swelled as if going to head. Wheat, very much like the barley.

August 17.—Cereals appear to have nearly done growing; beans dying.

September 7.—Beans dead. Wheat green at the tops, but dead below; 4 to 6 inches high; about ten leaves each. Oats nearly dead. Barley nearly dead.

October 24.—Wheat still green at the top; oats and barley dead. Beans dead (not analysed).

November 6.—*Plants taken up.* Notes as under:—

Wheat. Seven plants; 4 to 5 inches high; no nodes visible; little tendency to form stem; lower leaves dead; top leaf green, and on some the next greenish yellow. Soil moist. Very little root.

Oats. Seven plants, 4 to 6 inches high; about six leaves each; generally four nodes visible; most forming head. Roots but little distributed; very little below 1½ inch; fewer and more slender than those of the wheat. All the plants dead or ripe, but not decomposed; the stems firm and elastic.

Barley. Eight plants, 4 to 6½ inches high; two to four nodes visible in all; six or

seven leaves on each; most have a leaf-like heading stretching upwards; lower leaves generally very long, 4 or 5 inches. Roots very little distributed; perhaps a little deeper than the oats; but few deeper than 1½ to 2 inches. Soil dry, loose, and porous; flints quite open.

Wheat, Oats, and Barley prepared and analysed as described at pp. 543, 544.

Plants grown in 1858, *with a supply of combined Nitrogen beyond that contained in the seed sown.*

No. 9.—*Wheat* (1858); *four seeds; prepared soil; with nitrogenous manure.*
(See Plate XV. fig. 10.)

April 27. Seeds set, and the pot placed over sulphuric acid, and covered with a glass shade.

May 20.—Four plants up; healthy, green, and growing; about 4 inches high; very similar to Wheats No. 1 of this date.

May 22.—A pipette-ful of the sulphate-of-ammonia solution (=0·004 gramme N.) added to the soil.

May 29.—Four plants, from 4 to 6 inches high. Greener and fresher since the addition of the ammonia-solution; new shoots appearing; only the lowest leaf on each yellow. A pipette-ful of the phosphate-solution added.

June 7.—Second pipette-ful of the sulphate-of-ammonia solution added; and a pipette-ful of the phosphate-solution.

June 19.—Four plants; one with four shoots or stems, and two others with two stems each; in all nine stems or plants; 5 to 7 inches high; leaves more healthy, green and vigorous.

June 21.—Third pipette-ful of ammonia-solution added. Plants had begun to show the want of available nitrogen.

June 26.—Fourth pipette-ful of the ammonia-solution added. As intimated May 29, the ammonia tends much to new shoots from the base. The four seeds have given—No. 1, one stem, with seven leaves: No. 2, two stems, each with six leaves: No. 3, three stems, each with five or six leaves: No. 4, four stems, with four, five, or six leaves each: in all ten stems. The two lowest leaves dead, the others green and vigorous. More vegetable matter from these four seeds, with ammonia, than from the eight (No. 1 Wheat) without it. Plants 8 to 11 inches high, and improve with each addition of ammonia.

July 1.—The shade slightly cracked at the bottom during the night, but still sufficiently air-tight for changes of temperature to affect the level of the sulphuric acid in the bulb-apparatus. Shade replaced by a small one temporarily. An immaterial amount of condensed water lost. Many roots found to be distributed through the bottom of the pot, and growing in the dish beneath it.

July 3.—Fifth pipette-ful of ammonia-solution added. [It is intended that none of the plants shall suffer so much for want of combined nitrogen, as in 1857.] Also a pipette-ful of the phosphate-solution added.

July 12.—Sixth pipette-ful of ammonia-solution added.

July 14.—Seventh pipette-ful of ammonia-solution added. Details of growth from the four seeds as follow:—

No. 1, one stem only, 8 to 10 inches high; three upper leaves green and vigorous, four lower yellow. No. 2, three stems: (*a*) 3 to 4 inches high, two yellow and two green leaves; (*b*) 4 to 6 inches high, two yellow and two green leaves; (*c*) 12 to 14 inches high, three yellow and three green leaves. No. 3, three stems: (*a*) 4 to 6 inches high, two yellow and two green leaves; (*b*) 8 to 10 inches high, two yellow and three green leaves; (*c*) 13 to 15 inches high, two yellow and three green leaves. No. 4, four stems, 8 to 10 inches high, three yellow and three green leaves each.

The "yellow" leaves are small ones at the base, developed before ammonia was added, and are dead. The ammonia strikingly developes the new shoots, and their leaves are much larger. As last year, much more tendency to form leaf than run to stem. The height given above includes that of the erect leaves extending above the ascending axis.

July 19.—Eighth pipette-ful of ammonia-solution added.

July 29.—Ninth pipette-ful of ammonia-solution added. The following details will show how dependent is the development upon the proximity to the mouth of the tube by which the ammonia-solution is applied.

No. 1 (furthest from the tube), one stem strong and vigorous at the base, and 13 inches high. No. 2 (third from the tube), three stems; one 12 to 13 inches high, the others smaller. No. 3 (second from the tube), five stems; three 12 to 13 inches high, and two new ones 1 to 2 inches high. No. 4 (nearest the tube), seven stems; four 8 to 13 inches high, and three new ones 1 to 3 inches high.

All upper leaves green and vigorous, most of the lower yellow and dead; some of the shoots entirely green.

August 10.—Tenth pipette-ful of ammonia-solution added.

August 17.—Eleventh pipette-ful of ammonia-solution added (a new solution, the pipette-ful =0·00359 gramme N.).

Recently, much more disposition to form stem; several plants from 18 to 30 inches high; nodes clear of the sheaths. Comparing with the Wheat without ammonia, which also tends more to stem of late, it appears that the amount of growth is due to the supply of combined nitrogen, and its character (stemmy) more to the season.

August 26.—Twelfth pipette-ful of ammonia-solution added.

September 7.—Thirteenth pipette-ful of ammonia-solution (the last application; in all, 0·0508 gramme N.) added. Growth from each seed as under:—

No. 1 (furthest from the ammonia-solution tube), one stem 23 inches high; twelve leaves; two nodes clear; slight indication of heading.

No. 2 (third from the tube), one main stem growing, and two shoots dead; main stem 23 inches high; three upper leaves green, six lower ones yellowish; not heading yet.

No. 3 (second from the tube), three stems; two like "No. 2"; the third 12 to 14 inches high; little tendency to form stem, but long leaves from the axis.

No. 4 (nearest the tube, some of the roots washed when the solution, or water, is

added), two stems and five small shoots:—(*a*) highest leaf touches the top of the shade, and 3 inches of it lie against the wet glass, by which it is injured; ten leaves; three bare nodes; (*b*) 23 inches high; nine leaves, three upper green, others yellowish; nodes not clear of the sheath; not heading yet; (*c*) three small stems from the base, 6 to 8 inches high, ceased to grow, and apparently dying; (*d*) two small rudimentary shoots; ceased to grow, and dying.

Main stems—lower leaves yellow or dead; those starting a few inches from the soil numerous, and 12 to 16 inches long; those higher up, 4 to 6 inches long, green, and healthy; apparently incapable of supporting all the shoots started.

October 5.—Plants principally increasing in height of stem; three touch the top of the shade; upper leaves green. Shaded from the direct sun to prevent injury from the little aqueous lenses formed on the interior of the shade; yet sun apparently wanted for ripening.

October 24.—Shade entirely full of vegetable matter, some stems touching the top, and leaves touching on all sides. Season for growth about over; plants seemed stationary during some recent cold weather, but, the last few days being warmer, they have revived again. [This remark applies to all the Cereals.]

October 26.—*Plants taken up*; produce from each seed as under:—

No. 1 (furthest from the ammonia-solution tube), a mass of tufted leaves at the base; five leaves higher up below any visible node, formed before the plants began to run up stem (it was the same with the other plants, and with the leafy growth last year); higher up five more leaves, four visible nodes, and a head; total height 30 inches; rachis 1½ inch; three barren joints, and six with unripe seeds.

No. 2 (third from the tube), two dead shoots at the base, with several leaves each; twelve leaves higher up on the main stem, lower ones 6 to 9 inches long, upper ones shorter; four nodes visible; total height 28 inches; rachis 1½ inch long, three barren joints, and six with glumes and pales, but still green.

No. 3 (second from the tube), three stems: (*a*) 6 inches high; ten long narrow leaves; stem dead. (*b*) 24 inches high; fourteen leaves below the first node, and three higher up; two nodes visible; plant still growing and vigorous. (*c*) Height 31 inches; twelve leaves below the first node, and three above it; swelled at the top with a head not yet out.

No. 4 (nearest the tube), seven plants—five being small shoots 2 to 8 inches high, and two main stems. As mentioned in respect to No. 1, and applicable pretty generally to the Cereals with ammonia, a dense matted mass of leaves below the first node near the base, 8, 12, and 15 inches long, with thick sheaths forming a dense coat at the base of the stem. These plants are individually as follow:—(*a*) 28 inches high; three nodes; rachis two inches long, with five barren joints, and seven with glumes and pales, and seeds forming, green. (*b*) 35 inches high; four visible nodes; rachis 2½ inches long, with five barren joints and seven with glumes and pales, and shrivelled seeds turning yellow.

The soil wet, soft, and loose, and not filling up the interstices among the flints.

The roots tolerably distributed throughout the whole pot, though not extending much to the sides, but passing through all the holes at the bottom, and some at the sides, of the pot, and forming a dense matted mass underneath it in the pan. Those of the plants nearest to the watering and manuring tube have sent by far the most roots downwards into the pan,—those furthest from it having their roots proportionally much more confined in the soil near the base of the plant, where they are much matted, with divaricate branches and short branchlets—these characters being more nearly those of the Cereals grown without nitrogenous manure.

Preparation and analysis as described at pp. 543, 544.

No. 10.—*Barley* (1858); *four seeds; prepared soil; with nitrogenous manure.*
(See Plate XV. fig. 11.)

April 27.—Seeds set, and the pot placed over sulphuric acid, and covered with a glass shade.

May 7.—The pot removed to its shade on the stand.

May 20.—Two plants up.

May 22.—A pipette-ful of the sulphate-of-ammonia solution (=0·004 gramme N.) added to the soil.

May 29.—Two plants; 4 to 6 inches high; five leaves on each, the lowest dead, the others green; improved since the addition of ammonia, more active growth and colour deeper green. A pipette-ful of the phosphate-solution added.

June 7.—Second pipette-ful of the sulphate-of-ammonia solution added; also a pipette-ful of the phosphate-solution.

June 19.—Two plants; 10 to 12 inches high, forming stem well; each plant seven leaves; one stem has a small shoot forming at its base.

June 21.—Third pipette-ful of the sulphate-of-ammonia solution added.

June 26.—Fourth pipette-ful of the sulphate-of-ammonia solution added. Two plants; 16 to 18 inches high, healthy and vigorous; eight leaves each; also each a vigorously growing shoot 4 to 5 inches long.

July 3.—Fifth pipette-ful of the sulphate-of-ammonia solution added; also a pipette-ful of the phosphate-solution.

July 12.—Sixth pipette-ful of the sulphate-of-ammonia solution added.

July 14.—Seventh pipette-ful of the sulphate-of-ammonia solution added. Two plants, namely,—

No. 1. (*a*) Main stem, 26 inches high; lower leaves ripe or dead, and yellow, upper ones green and growing; beard just appearing at the head. (*b*) Shoot, 18 inches high; three ripe or dead leaves, and three green.

No. 2. (*a*) Main stem, 28 inches high; lower leaves ripe or dead, four upper ones green; air-roots developing. (*b*) Shoot, 12 to 14 inches high; one dead, and four green leaves. (*c*) Shoot, 4 to 6 inches high; one dead, and three green leaves.

July 19.—Eighth pipette-ful of the sulphate-of-ammonia solution added.

July 28.—Ninth pipette-ful of the sulphate-of-ammonia solution added.

July 29.—Only two of the seeds germinated; plants as follow:—

No. 1. (*a*) Main stem, out of the ground single, and then gives off (*b*) and (*c*); is 22 inches high; lower leaves dead, and lowest shows a fungous growth; six air-roots (two still growing) from first joint or point of separation; five nodes, each giving a leaf, only the top one green; head bursting forth. (*b*) 28 inches high; five nodes, with leaves, upper two only growing; stem slim below and thicker higher up; awns of head appearing. (*c*) 5 inches high; two leaves; fresh and green.

No. 2. More vigorous than No. 1, and livelier colour; leaves the ground single; half an inch up divides, and also gives off roots which go into the soil; three-quarters of an inch higher a second shoot, and more roots given off; one reaches the soil, two growing downwards, and several withered; lowest leaves dead and show fungous growth, particularly where they lie on the soil.

August 17.—Tenth pipette-ful of the ammonia-solution added (new solution =0·00359 N). Plants growing vigorously under the influence of the ammonia; nearly at the top of the shade; several heads appearing.

August 24.—Eleventh pipette-ful of the ammonia-solution added.

September 7.—Twelfth pipette-ful of the ammonia-solution added.

October 5.—Plants ripening; seven stems with heads; lower leaves of each dead or ripe, and two or three upper ones of each green. Some new shoots appearing at the base.

October 24.—Three heads green, the others ripe.

October 26.—*Plants taken up*:—

Only two seeds grew, and gave plants as follow:—

No. 1. Stem divided a little above the surface of the soil, giving plant (*a*), and an inch higher up divides again, giving (*b*) and (*c*); below the first point of separation the stem scarcely thicker than a pin, hard and solid. (*a*) 34 inches high; six visible nodes; stem below the lowest very thin and hard, but larger, soft and succulent higher up; head 3 inches long, having sixteen joints, with glumes and pales, and awns 4 to 6 inches long; two ripe plump seeds, and others shrivelled up. (*b*) 28 inches high; five nodes; below the lowest stem hard, firm, dry, and almost solid, and but little thicker than a pin; stem higher up larger, but still quite delicate; head 1½ inch long, with three joints barren, and seven with glumes, pales, and long awns, but no seed. (*c*) 30 inches high; five nodes; lower part of stem not quite so thick as (*a*) and (*b*); head ripe; rachis 2½ inches long, with two joints barren, and thirteen with glumes, pales, and awns; also some shrivelled seeds.

No. 2. Stem to 1½ inch above the soil little thicker than a pin, quite solid, and firm; then a thick and bunchy node and six stems. Stem (*a*) 18 inches high to rachis; four nodes; four leaves; crooked rachis 1½ inch long, with three joints barren, and seven with glumes, pales, and awns. (*b*) 21 inches high to head; rachis 2½ inches long, with three joints barren, and thirteen with glumes, pales, and long awns; four nodes; five

leaves. (*c*) green; 2½ inches high, and still growing. (*d*), (*e*), and (*f*) about 25 inches high to rachis; each with four or five nodes; rachis 1½ inch long, with three or four joints barren, and nine or ten with glumes, pales, and some green seeds.

The Root-development of these Barley plants was very extraordinary. Plant No. 1 has about ten roots coming from the first node or point of separation, extending deep into the soil, and some even through the bottom of the pot; roots also come out from the next joint above, on each of the separate stems, but these do not reach the soil. Plant No. 2, from the first joint, whence spring the six stems, throws out more than a dozen small roots, five of which reach the soil and ramify in it, some of the ramifications going down into the pan. The roots starting within the soil not so much matted near the base of the stem as those of the Wheat; a good many go down and ramify amongst the flints, or go through into the pan; they are finer, and not so much blunted and divaricated as the Wheat roots. For illustration of the curious root-development of the barley plant, see Plates XV. fig. 16.

Soil dry, loose, and porous.

Preparation and analysis as described at pp. 543, 544.

No. 11.—*Oats* (1858); *four seeds; prepared soil; with nitrogenous manure.*
(See Plate XV. fig. 12.)

April 27.—Seeds set, and pot placed over sulphuric acid, and covered with a glass shade.

May 7.—The pot removed to its place on the stand.

May 20.—Three plants up; 4 to 4½ inches high; two leaves each; green and healthy.

May 22.—A pipette-ful of the sulphate-of-ammonia solution added (=0·004 grm. N.).

May 29.—Three plants; 5 to 7 inches high; four or five leaves each; two lowest dead, three uppermost green and growing. These plants have increased the most of any since the addition of ammonia. A pipette-ful of the phosphate-solution added.

June 7.—Second pipette-ful of the sulphate-of-ammonia solution added; also a pipette-ful of the phosphate-solution.

June 19.—Three plants; 10 to 14 inches high; six leaves each; apparently somewhat injured by hot sun the last few days.

June 21.—Third pipette-ful of the sulphate-of-ammonia solution added.

June 26.—Fourth pipette-ful of the sulphate-of-ammonia solution added. Three plants, 12 to 14 inches high. No. 1, head developed and obviously seeds forming; one shoot 1 inch, and one 4 inches long from the base. No. 2, head forming; two shoots from the base 4 to 5 inches long. No. 3, much like No. 2. Growth of main stems apparently checked and that of shoots increased by some recent hot weather; in all, six shoots; green, healthy, and promising further growth.

July 3.—Fifth pipette-ful of the sulphate-of-ammonia solution, also a pipette-ful of the phosphate-solution added.

July 12.—Sixth pipette-ful of the sulphate-of-ammonia solution added.

July 14.—Seventh pipette-ful of the sulphate-of-ammonia solution added. Plants as follow:—

No. 1. Two stems; one ripe, with two or three seeds; six leaves, lowest dead ripe, and five green. The other stem 12 to 14 inches high; green; forming head.

No. 2. Three stems, from 12 to 15 inches high. One stem swelled at the top with a head, but not maturing; checked early, apparently from want of available nitrogen, after which the ammonia added developed the two shoots, whose stems are green, with one dead and four green leaves, and heads forming.

No. 3. Two stems; one 8 to 10 inches high, dead or ripe; the other 14 to 15 inches high, green and healthy.

July 30.—*The plants taken up*:—

The experiment stopped rather prematurely, the shade getting slightly cracked. The plants had manifested three orders of growth, as follow:—

(1) The first growth, which was forming head when a fresh supply of ammonia gave rise to shoots at the base. (2) The above shoots gave three headed stems: one with three full and two imperfect seeds; one with two full and one imperfect seed; and one with one full and two imperfect seeds. (3) New shoots from the base on the further addition of ammonia; green and healthy at the close, but promising to go to seed.

The roots were very little more distributed than those of the Oats without nitrogenous manure in No. 3 shade, and only extended about half the depth of the pot. The soil was wet and soft.

Preparation and analysis as described at pp. 543, 544.

No. 12.—*Beans* (1858); *three seeds; prepared soil; with nitrogenous manure.*

April 27.—Seeds set, and the pot placed over sulphuric acid, and covered with a glass shade.

May 20.—Pot removed to its shade on the stand. Three plants up; 2½ inches high; three leaves on each, colour dark green; healthy.

May 22.—A pipette-ful of the sulphuric-acid solution added.

May 29.—Three plants; one dead, one much specked, the third improving. A pipette-ful of the phosphate-solution added.

June 7.—A pipette-ful of the sulphate-of-ammonia solution added; also a pipette-ful of the phosphate-solution.

June 19.—All apparently dying; leaves much specked with black.

June 21.—Plants all obviously past recovery; removed but not analysed.

No. 13.—*Peas* (1858); *three seeds; prepared soil; with nitrogenous manure.*

April 27.—Seeds set, and the pot placed over sulphuric acid, and covered with a glass shade.

May 20.—Pot removed to its shade on the stand. Three plants up, growing exceedingly well; 3, 2½, and 1 inch high; three, two, and two leaves, respectively.

May 22.—A pipette-ful of the sulphuric acid solution added.

May 29.—One plant dead; another sickly; the third has given off shoots 3 to 4 inches high, which appear healthy. A pipette-ful of the phosphate-solution added.

June 7.—A pipette-ful of the sulphate-of-ammonia solution (=0·004 gramme N.) added; also a pipette-ful of the phosphate-solution.

June 19.—Two plants dead; the third has two green shoots, 3 and 5 inches high, but delicate.

June 26.—The two shoots growing languidly; seven leaves each; also some leaflets.

July 3.—A pipette-ful of the phosphate-solution added.

July 14.—Main shoot 12 to 13 inches high; four lower leaves dead, four upper green; two shoots at the base dead, and one green and thriving.

July 29.—The two dead plants entirely prostrate, and near one a pale-green moss formed on the surface of the soil. The third plant has three dead branches, and one about 6 inches high with green leaves at the top.

August 17.—All the plants dead.

August 24.—Plants taken up, and submitted to analysis, although the growth so unsatisfactory. Of course, of themselves, the results can have little weight in reference to the question at issue.

No. 14.—*Clover* (1858); 226 *seeds*; *prepared soil*; *with nitrogenous manure.*

Two pots of Clover had been sown at the same time as the other plants, one to be without, and the other with nitrogenous manure. They came up well, but very soon died; and on June 6 two more pots were sown in the same manner as before, excepting that the ash was neutralized with sulphuric acid, and re-ignited before being mixed with the soil; both also had a pipette-ful of the phosphate-solution, and the one that was to have nitrogenous manure a pipette-ful of the ammonia-solution, at the time of sowing the seed. The one sown without the ammonia-solution failed so soon that the experiment was abandoned early; the other (with the ammonia) forms the subject of the following record.

June 6.—Seeds set as above, in soil with neutralized ash, and both phosphate and sulphate-of-ammonia solution added*.

June 19.—A good deal up; actively and healthily growing.

June 21.—Second pipette-ful of the sulphate-of-ammonia solution added*.

June 26.—Third pipette-ful of the sulphate-of-ammonia solution added. Plants quite green and healthy.

July 3.—Fourth pipette-ful of the sulphate-of-ammonia solution added; also a pipette-ful of the phosphate-solution.

July 12.—Fifth pipette-ful of the sulphate-of-ammonia solution added.

July 14.—Sixth pipette-ful of the sulphate-of-ammonia solution added. Surface of

* It is not quite certain that the ammonia-solution was added at this date; and in the Table of the results (XIV. p. 581) it is assumed that it was not.

the soil covered with plants, growing well; 3 to 4 inches high, with two or three trifoliate leaves each.

July 19.—Seventh pipette-ful of the sulphate-of-ammonia solution added.

July 28.—Eighth pipette-ful of the sulphate-of-ammonia solution added.

July 29.—The plants growing, but not vigorously; some black and dead, and some withering leaves; some petioles more than 4 inches long.

August 10.—Ninth pipette-ful of the sulphate-of-ammonia solution added.

August 17.—Tenth pipette-ful of the sulphate-of-ammonia solution added (new solution = 0·00359 gramme N.). Green and growing well; twenty to twenty-five stems 4 to 6 inches high; a good many roots above ground.

September 7.—Eleventh pipette-ful of the sulphate-of-ammonia solution added. Large strong roots visible; numerous leaves from the base; not much development of stem; length of petioles 3 to 5 inches.

October 5.—Twelfth pipette-ful of the sulphate-of-ammonia solution added. Not growing much, yet not dying; stems becoming bushy.

October 24.—Many of the lower petioles and leaves dead, and some a little mouldy; from the base of the dead petioles generally spring a number of green leaves, and petioles 2 to 4 inches long bearing green leaves, which look healthy, but almost stationary from day to day.

October 26.—*Plants taken up*:—

Twenty-one plants; much as at last date.

Roots somewhat matted at the base, and throughout the surface of the soil; extending 2 to 3 inches through the soil, and some to the inner sides of the pot; none among the flints, or in the pan under the pot.

Soil rather wet, loose, and porous.

Preparation and analysis as described at pp. 543, 544.

No. 15 (1858).—*Wheat, Barley, Oats, Beans, Peas, and Clover, in a pot of Garden soil.*

May 20.—Some of the plants just coming up.

May 29.—Wheat, Barley, and Oats, 6 to 8 inches high; Beans 4 to 6 inches high, with some specks on the leaves; Clover up and growing. The condensed water yellowish, and gives a yellowish-green deposit.

June 19.—The pot full of green herbage. Wheat, Barley, Oats, and Beans, all healthy and growing well; Peas 3 to 4 inches high; Clover 2 to 3 inches, with three leaflets.

June 26.—Wheat and Barley touch the top of the shade, and the plants crowded; Clover 3 to 4 inches high.

July 14.—Shade full of growing matter. Wheat and Barley just heading.

August 2.—The Oat heading, with two grains; Wheat growing, but the ends of some of the leaves dying. The two Beans quite black and dead. Several kinds of weed come up; two in bloom.

August 17.—Shade full of vegetable matter. Wheat, Barley, Oats, and weeds growing; but Leguminous plants dead.

September 7.—A good deal of growth yet.

October 5.—A good deal of grass and weeds growing. Cereals ripening.

October 24.—Shade full; experiment stopped. Wheat not quite ripe. Barley and Oats dead ripe; Beans and Peas all dead; a little Clover still living; some grass, and other weeds, green, and some seeded. The whole soil filled with roots, many distributed through the flints, and a large quantity growing in the pan under the pot.

No. 16.—*Buckwheat* (1858); *forty-two seeds* (1 *gramme*); *prepared soil; with nitrogenous manure.*

August 20.—Seed sown, and the pot placed over sulphuric acid, and covered with a glass shade.

August 24.—Pot removed to its shade on the stand. Several plants up.

September 7.—A pipette-ful of the sulphate-of-ammonia solution added (=0·00359 gramme N.). Plants growing well.

October 5.—Second pipette-ful of the sulphate-of-ammonia solution added. Much more vigorous than the Buckwheat without ammonia (No. 7); about twenty plants; 5 to 7 inches high; four to six leaves on each.

October 24.—Third pipette-ful of the sulphate-of-ammonia solution added. Growing well; 5 to 7 inches high; six plants in bloom. Comparing with No. 7, the influence of ammonia here is very marked, as shown in size, vigour, and maturation.

November 22.—*Plants taken up*:—

Twenty-four plants; 4 to 7 inches high; five to seven leaves on each; six stems have flowered; bloom gone off and rudimentary seed formed; would probably have grown more but for frost.

Roots less in proportion to upward growth than with Buckwheat without ammonia; none deeper in the soil than 1½ to 2 inches; slim, delicate, and but little distributed.

Soil loose and porous.

Preparation and analysis as described at pp. 543, 544.

No. 17 (1858).—Plants grown with Nitrogenous Manure in M. G. Ville's Case.

The same descriptions of pot, pan, soil, ash, &c., were used for these experiments as for the others, as explained at page 565. Plants as under:—

Wheat, four seeds; Barley, four seeds; Oats, four seeds; Beans, three seeds.

June 29.—Seeds set, and the pots placed over sulphuric acid, and covered with a glass shade.

July 5.—Pots removed to M. Ville's Case.

July 14.—A pipette-ful of the sulphate-of-ammonia solution given to each pot (=0·004 gramme N.). Wheat, four plants just appearing; Barley, three plants 1½ inch

high; Oats, two plants just appearing; Beans, one plant 4 inches high, two others just appearing.

July 19.—Second pipette-ful of the sulphate-of-ammonia solution to each pot.

July 28.—Third pipette-ful of the sulphate-of-ammonia solution to each pot of Cereals.

August 2.—Wheat, four plants; 7 inches high; extremities of some of the leaves dying. Barley, three plants; two 9 inches, and one 7 inches high; two lower leaves of each yellow, remainder green. Oats, two plants; 10 inches high; the lower leaf of each dead, and the tips of some of the others turning yellow. Beans, two plants up; one 8, and the other 6 inches high.

August 17.—Fourth pipette-ful of the sulphate-of-ammonia solution added (new solution, =0·00359 gramme N.). All the plants growing luxuriantly. Wheat, about 14 inches high; Barley and Oats, 8 to 12 inches high; Beans, 9 and 12 inches high.

September 7.—Fifth pipette-ful of the sulphate-of-ammonia solution added.

Wheat, four plants; (*a*) 14 inches high with ten leaves; (*b*) 13 inches high with nine leaves; (*c*) 15 inches high with ten leaves; (*d*) 11 inches high with ten leaves.

Barley, three plants; (*a*) 23 inches high with eleven leaves; (*b*) 14 inches high with ten leaves; (*c*) 25 inches high with eleven leaves.

Oats, two plants; (*a*) 26 inches high with seven leaves; (*b*) 29 inches high, nine leaves.

Beans, two plants; main stems dead, but new shoots growing.

October 5.—Wheat, four healthy and vigorous plants; (*a*) 14 inches high; two green and eight dead leaves; (*b*) 14 inches high; two green and seven dead leaves; (*c*) 15 inches high; two green and eight dead leaves; (*d*) 14 inches high; three green and eight dead leaves.

Barley, three plants; two healthy and vigorous, and one less so; (*a*) 24 inches high; three green and eight dead leaves; (*b*) 18 inches high; three green and seven dead leaves; (*c*) 28 inches high; three green and eight dead leaves.

Oats, two healthy and vigorous plants, in ear; (*a*) 30 inches high; two green and five dead leaves; (*b*) 30 inches high; two green and seven dead leaves.

Beans, two plants; (*a*) 9 inches high with three branches; (*b*) 14 inches high with three branches.

October 24.—Wheat, four plants, erect and firm; much as on October 5, but larger; no nodes visible; but little tendency to form stem. Barley, much as on October 5. Oats, much as on October 5. Beans, apparently ceased to grow.

Sixth pipette-ful of the sulphate-of-ammonia solution added. Wheat, four plants, 14 to 18 inches high; stems stout and erect, but without visible nodes; very leafy. Barley, four plants, just coming to ear. Oats, two plants, main stems touching the top of the Case; (*a*) has a shoot 22 inches long springing from the first node 2 inches from the soil, and coming into head; (*b*) has three small shoots, one $\frac{1}{2}$ inch and one $1\frac{1}{2}$ inch long, springing from the base, and one 3 inches long from the first node. Beans, slightly revived since the last report.

Notes on taking up the Plants.

Wheat taken up December 9.—The four plants all about the same size; strong, healthy, and vigorous; ten to twelve long dead leaves each, 8, 10, or 12 inches long, closely compacted one above another at their base, and two or three green flourishing leaves each at the top. Roots very thick and matted at the base, extending immediately around in all directions, with short rootlets covered with divaricated branches, filling the upper layers of soil; not many extend more than 2½ inches down, very few go through the bottom of the pot into the pan, and none ramify there. Soil loose, porous, and moist; and some water in the pan.

Barley taken up December 9.—Three plants; (*a*) 29 inches high, with a small shoot (second growth) 1 inch long, from the base; fifteen leaves, two top ones green and growing; swelled at the top with a head forming; (*b*) 20 inches high; thirteen leaves, upper ones green and growing; and indications of head forming; (*c*) much like (*a*) in height, leaves, and shoot; a short air-root from the first node. Roots the same character in all; branched, considerably matted at the base, finely divided with numerous little divaricate branchlets; only a few extend down into the pan, and they do not ramify in it. Soil loose, open, porous, and moist; some water in the pan.

Oats taken up December 9.—Two plants:—(*a*) with two stems; one 32 inches high, eleven imperfect seeds, and seven leaves; the other 26 inches high, coming from the second node 2 inches up the main stem, with twelve seeds; six or eight air-roots 1 to 2 inches long, coming from the base node; a shoot an inch long from the lowest node, and another lower down at the base. (*b*) A main stem with two long branched ones; main stem 32 inches long, with five leaves and ten undeveloped seeds without solid nucleus; one branched stem, from the third node, 3 inches up the main stem, 14 inches long, with three leaves, still green and growing; the other branch from the base of the main stem 10 inches long, green and growing; some air-roots 1 to 2 inches long come from the base of the largest branch, also several from the next node below, one or two of which extend to the soil and ramify in it. There is also another small branch 2 to 3 inches long coming from the base of the plant and still green and growing. Roots much alike in both; considerably matted at the base, little distributed, extending 3 or 4 inches in the upper parts of the soil, with many small divaricate branches; no roots in the pan. Soil loose, porous, and moist; and some water in the pan.

Beans taken up December 9.—Two plants, with eight or ten leaves each; one 9 inches, the other 14 inches high; both dead. The roots of both very much branched at the base of the stem, but not extending deep into the pot, none going down to the flints.

The Wheat, Barley, Oats, and Beans, prepared and analysed as described at pp. 543, 544.

XXIV. *On the Indian Arc of Meridian. By the Venerable* J. H. PRATT, *M.A., Archdeacon of Calcutta. Communicated by Professor* STOKES, *Sec. R.S.*

Received September 3,—Read November 22, 1860.

IT is with pleasure that I request the attention of the Royal Society to the present communication, in continuation and completion of my former papers, because I think that the anomalies which the Indian Arc has appeared to present are here traced to the true causes.

1. I will explain what those anomalies were. On completing a laborious and well-executed survey of the two northern portions of the Indian Arc of Meridian, between Kaliana (29° 30′ 48″) and Kalianpur (24° 7′ 11″), and Kalianpur and Damargida (18° 3′ 15″), Colonel EVEREST found that their astronomical and geodetical amplitudes differed considerably; in the higher arc the geodetic amplitude he found to be in excess by 5″·236, in the lower of the two arcs in defect by 3″·791*. The three stations had been selected with great care, and were finally chosen as being apparently free from all disturbing causes. Indeed, a fourth station which had been at one time adopted, Takal Khera in Central India, was rejected by Colonel EVEREST because a neighbouring hill-range was discovered on calculation to produce a deflection of about 5″. Kaliana had been chosen nearly sixty miles from the lower hills at the foot of the Himmalaya Mountains, in the full conviction that it would be free from mountain influence. The surprise was therefore great when, on the completion of the survey of the two arcs in question, these two errors were brought to light. The first was attributed to the influence of the Himmalayas, but without any calculation; but the second, with its negative sign, received no interpretation. At this stage I devised a method of calculating the effect of the Himmalayas by a direct process; and found that the deflections produced are far greater than the errors which had to be explained, and the negative sign was left altogether unaccounted for. Thus the perplexity was increased. It next occurred to me that the vast Ocean to the south of India might have some influence on the plumb-line. On making the necessary calculations the effect of this cause was found, as the mountain attraction had been, to be far greater than had been anticipated; the negative sign was still unexplained, and the difficulties were not cleared up. No other cause of disturbance was apparent at the surface. But I showed by calculation that in the crust below one *might* exist sufficient to reduce the large deflections occasioned by the Mountains and the Ocean, and make them accord with the results deduced by Colonel EVEREST

* Colonel EVEREST used a mean figure of the earth somewhat different to that used in this paper, which is taken from the volume of the British Survey lately published, and makes the errors somewhat less than those above given. See EVEREST 'On the Indian Arc,' 1847, p. clxxvi.

from the arcs themselves. But, being hidden from our sight, neither the magnitude nor indeed the existence of this cause could be *à priori* ascertained, much less reduced to calculation. Whether, moreover, the errors brought to light by Colonel EVEREST arose solely from local attraction, or from local attraction combined with some local peculiarity in the curvature of the Indian Arc, was not apparent; so that the subject of local attraction, and its influence on geodetic operations in this country, was still involved in obscurity, and the anomalies of the Indian Arc remained unexplained in the papers which I have hitherto forwarded to the Society. In the present communication I think ambiguity is removed. It is demonstrated that no peculiarity in the curvature of the arc can produce any part of the errors brought to light by Colonel EVEREST; that those errors arise solely from local attraction; that they are in fact the exact measure of the difference of the resultant local attraction at the two extremities of each arc, from whatever causes the attraction may arise—mountains, ocean, or crust; lastly, it is proved that there *are* hidden causes in the crust below the Indian Arc, and the differences of their resultant effect upon the stations of the arc are computed. An inference from these results is, that the relative position of places in a Map, laid down from geodetic operations, is accurate, being altogether unaffected by local attraction; though the position of the Map itself on the terrestrial spheroid will be dependent upon the observed latitude of some one station in it, and that observed latitude will be affected by the local attraction at that place. To determine the absolute latitude in some one station connected with the geodetic operations is still a desideratum.

§ 1. *Summary of the Results of former Papers.*

2. The results of my former papers I may briefly sum up as follows:—

(1) In the first of them * I calculated the effect of the Mountain-Region north of India upon the plumb-line at the three principal stations of the northern portion of the Indian Arc; viz. Kaliana (29° 30′ 48″), Kalianpur (24° 7′ 11″), and Damargida (18° 3′ 15″). The deflections towards the north were found to be 27″·98, 12″·05, 6″·79; and in consequence of these, the observed astronomical amplitudes would be 15″·93 and 5″·26 less than the true amplitudes determined by normals to the meridian line in the meridian plane.

These quantities, as I showed †, are not materially affected by new information regarding the mountain mass communicated to me by Lieut.-Colonel STRACHEY.

(2) In my second paper ‡ I calculated the effect on the plumb-line of a slight but wide-spread deviation of density in the crust of the earth, in excess or defect, from

* Philosophical Transactions, 1855; also 1859, p. 770.

† Ibid. 1859, p. 774. The reader is requested to make the following corrections in that paper:—

Page 761, *line* 4, *for* multiply *read* modify.

— 767, — 2 *ab imo*, *for* $\frac{r}{r-d}$ *read* $\frac{2r}{r-d}$

— 781, — 26, *for* require *read* requires.

Page 782, *line* 16 *ab imo*, *after* by *insert* 0·185 and by

— 794, — 17, *for* 15·88 *read* 62·60

— 795, — 4 *ab imo*, *for* 15·88 *read* 62·60

— — — — *for* 514·57 *read* 561·29

‡ Philosophical Transactions, 1859, p. 745.

that required by the fluid-law, and showed that the effect might be very sensible and important. The results of the calculation were embodied in the following Table:—

TABLE OF DEFLECTIONS, caused by an excess or defect of matter throughout a semi-cubic space of 4 millions of cubic miles,—the mean density of the excess or defect being $\frac{1}{100}$th part of the density of the earth at the depth of the centre of the cubic space.

Depth of the centre of the semi-cubic Space.	Distance, measured along the chord, from the station to the point where the radius of the earth drawn through the middle point of the semi-cubic space meets the earth's surface.				
	379 miles.	581 miles.	781 miles.	980 miles.	1173 miles.
50 miles	1″·940	0″·835	0″·457	0″·248	0″·118
150 miles	1·621	0·803	0·456	0·252	0·120
250 miles	1·383	0·782	0·483	0·272	0·131
350 miles	1·067	0·749	0·490	0·286	0·142
450 miles	0·663	0·713	0·425	0·277	0·145

(3) In my third paper * I calculated the effect on the plumb-line of the deficiency of attracting matter in the Ocean. I assumed the following law, as giving an average representation of the mass of water; viz. that the depths at the middle of the Bay of Bengal and of the Arabian Sea in the latitude of Cape Comorin, and at the mid-point between Madagascar and Australia, are severally $\frac{3}{4}$, 1 and 3 miles, and that the bottom slopes from the shores to these points, or to lines joining the first two with the third, or to other lines drawn northwards from those two points.

The meridian deflections toward the north at the three stations were made to be 6″·18, 9″·00, and 10″·44, causing an increase in the amplitudes equal to 2″·81 and 1″·44.

On combining these with the effect of the mountains, the deflections are 34″·16, 21″·05, and 17″·23, and therefore the true amplitudes 13″·11 and 3″·82 greater than the observed or astronomical amplitudes.

These are the main results of my calculations.

3. In the Great Trigonometrical Survey of India, which has been conducted with so much care and ability, the amplitudes of the two arcs in question, calculated geodetically on the supposition of the Indian Arc being curved like the mean arc, came out, the first 5″·236 in excess, and the second 3″·791 in defect, of the amplitudes observed astronomically. Neither the attraction of the mountains, nor that of the ocean combined with it, as appears from the last paragraph, would account for these, and especially for the negative sign. The other cause treated of (a variation in the density of the crust) being purely hypothetical and, if existent, yet altogether unknown in position and extent, it seemed hopeless to look for any precise explanation of the deviations of the plumb-line from that quarter, although the sufficiency of the cause to produce a sensible deflection was demonstrated.

I therefore attempted in each paper to explain the difference by attributing it to the

* Philosophical Transactions, 1859, p. 779.

Indian Arc being curved differently to the mean meridian of the earth. As each new disturbing cause—first the mountains, secondly the possible variation in density below, thirdly the ocean—was thought of and the effect calculated, the resulting curvature, of course, came out differently.

In the present communication, however, I shall demonstrate that no change in the curvature of the arc, within reasonable and indeed wide limits, can possibly have any appreciable effect on the calculated amplitude. It is this fact which leads to the result I have announced in the first paragraph. I will explain how this result did not flow from my former calculations. The length of the arc s between two stations is

$$a(1-\tfrac{1}{2}\epsilon)\lambda-\tfrac{3}{2}a\epsilon\sin\lambda\cos 2m,$$

λ and m being the amplitude and middle latitude, and a, ϵ the semi-major-axis and ellipticity. In order to find the effect produced on the dimensions of the ellipse passing through the two stations by increasing or decreasing the amplitude, this was differentiated, s and m being considered constant. This gives an equation connecting da and $d\epsilon$ with $d\lambda$, the change of the amplitude. A relation was then assumed (in the absence of a better method) between da and db, viz. that the mean value of a and b is the same in the two ellipses*. The calculation which I now give rests upon the fact, that the length of the chord of the arc must be the same in both the ellipses, the local and the mean, drawn through the stations at the extremities of the arc. There was a difficulty in following this course before, which I have now overcome. I find the length of the arc in terms of the unknown chord and semi-axes, and then differentiate with respect to the semi-axes, remembering that the chord is constant. All the terms now being small, an approximate value may be used in them for the chord in terms of either semi-axis and the observed latitudes of the extremities of the arc.

§ 2. *Statement of the several calculations which have been made of the form of the northern portion of the Indian Arc.*

4. I will bring together the results of the various measures and calculations which have been made of the arc between Kaliana and Damargida, divided near the middle by Kalianpur.

I. By a comparison of the two portions of the arc, Colonel Everest, taking the observed amplitudes, got the following results (a and b being the semi-axes of the ellipse):—

$$a=20985260 \text{ feet}, \quad b=20875737, \quad \epsilon=\frac{1}{192};$$

and he found that the amplitudes, calculated on the supposition of the arc being part of the mean ellipse, are $5''{\cdot}236$ in excess and $3''{\cdot}791$ in defect of the observed amplitudes†.

II. Captain Clarke, in his Chapter on the Figure of the Earth, in the volume of the British Ordnance Survey lately published, gives formulæ which enable us to calculate

* Philosophical Transactions, 1859, p. 762, Note.

† See Colonel Everest's Volume, 1847, p. 428; also p. clxxvii of the Preface.

the form of the Indian Arc, making use of the observed data. He shows* that the following quantities must be added to the observed latitudes of the three stations to make the arcs measured by the Survey fit an ellipse of which the axes are given by

$$a-b=\frac{1}{500}\left(1-\frac{u}{10000}+\frac{v}{50}\right)20890000,\ a+b=2\left(1-\frac{u}{10000}\right)20890000 \text{ feet},$$

viz. for Kaliana . . . $0{\cdot}403+4{\cdot}1251u+2{\cdot}7756v+x$

Kalianpur . . $-4{\cdot}085+2{\cdot}1831u+1{\cdot}6203v+x$

Damargida x

He finds for the mean ellipse of the whole earth $u=-0''{\cdot}3856$, $v=1''{\cdot}0620$, $x=0''{\cdot}050$, and therefore the corrections of the observed latitudes are, by the above formulæ, $1''{\cdot}810$, $-3''{\cdot}156$, $0''{\cdot}050$. Hence the amplitudes thus determined are $4''{\cdot}966$ in excess, and $3''{\cdot}206$ in defect of the observed amplitudes.

The form of the local ellipse can be determined from the data by putting the corrections for the latitudes equal to zero. This gives

$$4{\cdot}1251u+2{\cdot}7756v=-0{\cdot}403,$$

$$2{\cdot}1831u+1{\cdot}6203v=\ 4{\cdot}085,$$

which give

$$u=-19{\cdot}2015,\ v=28{\cdot}3920,$$

and thence

$$a=20984066 \text{ feet},\ b=20876151,\ \epsilon=\frac{1}{193}.$$

These results agree well with those of Colonel Everest noticed above.

III. The third measure is that determined by a comparison of the two arcs, the amplitudes being corrected for mountain and ocean attraction. Let s, s', λ, λ', m, m' be the lengths, amplitudes, and middle-latitudes of two arcs, of which the amplitudes are not large—as in this instance. Then

$$\frac{s}{\lambda}=\frac{a+b}{2}-3\frac{a-b}{2}\cos 2m,\ \frac{s'}{\lambda'}=\frac{a+b}{2}-3\frac{a-b}{2}\cos 2m',$$

$$\frac{a-b}{2}=\frac{1}{3}\frac{\frac{s}{\lambda}-\frac{s'}{\lambda'}}{\cos 2m'-\cos 2m},\quad \frac{a+b}{2}=\frac{\frac{s}{\lambda}\cos 2m'-\frac{s'}{\lambda'}\cos 2m}{\cos 2m'-\cos 2m}.$$

By what I have stated in paragraph 2, the increase to be made to the amplitudes to correct for mountain and ocean attraction is $13''{\cdot}11$ and $3''{\cdot}82$. The values of λ and λ' are therefore

$$\lambda=5^\circ\ 23'\ 37''+13''=5^\circ\ 23'\ 50'',\ \lambda'=6^\circ\ 3'\ 56''+4''=6^\circ\ 4'\ 0'',$$

also

$$s=1961157 \text{ feet},\ s'=2202926\dagger.$$

These lead to

$$a=20906792,\ b=20843795,\ \epsilon=\frac{1}{332}.$$

* See Ordnance Survey, pp. 737, 741, 767.

† See Colonel Everest's Volume, 1847, p. 427.

IV. The fourth measure of the arc is one proposed by Captain CLARKE in the volume of the Ordnance Survey. He suggests that by the principle of least squares the ellipse should be found which departs least from the mean ellipse in form, and at the same time gives deflections of the normal from the normal of the mean ellipse most in accordance with the calculated deflections. He finds this ellipse, taking account of mountain attraction only; the amount of ocean-attraction not having then been ascertained. The following recalculation, according to Captain CLARKE's method, takes account of both.

Let l_1, l_2, l_3 be the latitudes of the three stations referred to the mean ellipse. Then $l_1-1''{\cdot}81$, $l_2+3''{\cdot}16$, $l_3-0''{\cdot}05$ are the observed latitudes (see the calculation under II.). Let l_1+e_1, l_2+e_2, l_3+e_3 be the latitudes of the three places referred to any other ellipse, the axes being given by the formulæ in u and v under II. Then $e_1+1''{\cdot}81$, $e_2-3''{\cdot}16$, $e_3+0''{\cdot}05$ are the corrections which must be added to the observed latitudes to make them accord with this new ellipse. The dimensions, then, of this ellipse are determined by solving these equations:—

$$\begin{aligned} e_1+1{\cdot}81 &= \ \ 0{\cdot}403+4{\cdot}1251u+2{\cdot}7756v+x,\\ e_2-3{\cdot}16 &= -4{\cdot}085+2{\cdot}1831u+1{\cdot}6203v+x,\\ e_3+0{\cdot}05 &= x. \end{aligned}$$

These equations give

$$\begin{aligned} u &= -0{\cdot}3856+2{\cdot}5946e_1-4{\cdot}4446e_2+1{\cdot}8500e_3,\\ v &= \ \ 1{\cdot}0620-3{\cdot}4958e_1+6{\cdot}6056e_2-3{\cdot}1098e_3. \end{aligned}$$

Suppose d_1, d_2, d_3 are the angles of deflection caused by the mountains and ocean. Then the most probable ellipse to measure the curvature of the Indian Arc (supposing there are no other causes of deflection of the vertical) is that which makes

$$\begin{aligned} &(e_1-d_1)^2+(e_2-d_2)^2+(e_3-d_3)^2\\ &+(2{\cdot}5946e_1-4{\cdot}4446e_2+1{\cdot}8500e_3)^2+(3{\cdot}4958e_1-6{\cdot}6056e_2+3{\cdot}1098e_3)^2 \end{aligned}$$

a minimum. By differentiation with respect to e_1, e_2, and e_3 we obtain three equations, which after transformation become

$$\begin{aligned} e_1 &= \ \ 0{\cdot}76873d_1+0{\cdot}35715d_2-0{\cdot}12584d_3,\\ e_2 &= \ \ 0{\cdot}35716d_1+0{\cdot}34199d_2+0{\cdot}30086d_3,\\ e_3 &= -0{\cdot}12583d_1+0{\cdot}30087d_2+0{\cdot}82493d_3. \end{aligned}$$

These give

$$\begin{aligned} u &= -0{\cdot}3856+0{\cdot}17432d_1-0{\cdot}03671d_2-0{\cdot}13760d_3,\\ v &= \ \ 1{\cdot}0620+0{\cdot}06325d_1+0{\cdot}07484d_2-0{\cdot}13808d_3. \end{aligned}$$

The values of the calculated deflections d_1, d_2, d_3 are $34''{\cdot}16$, $21''{\cdot}05$, $17''{\cdot}23$. When these are substituted in the above formulæ, we have

$$e_1=31''{\cdot}61,\ e_2=24''{\cdot}58,\ e_3=16''{\cdot}25,\ u=2{\cdot}4255,\ v=2{\cdot}4189.$$

Hence the errors in the observed latitudes as affected by deflection (or $e_1+1{\cdot}81$, $e_2-3{\cdot}16$,

$e_2+0{\cdot}05$) are $33''{\cdot}42$, $21''{\cdot}42$, and $16''{\cdot}30$, which are very nearly equal to the calculated deflection. Also the values of u and v give the following results for the semi-axes and ellipticity:—

$$a=20919988,\quad b=20846981,\quad \epsilon=\frac{1}{287}.$$

5. The mean ellipse, as determined in the British Ordnance Survey Volume, gives

$$a=20926500,\quad b=20855400,\quad \epsilon=\frac{1}{294}.$$

If δa, δb be the excess (or, in case of a negative sign, the defect) of the semi-axes of any of the four ellipses described above, compared with the mean ellipse, then the following are true:—

	Arc I.	Arc II.	Arc III.	Arc IV.
$\delta a =$	11·13 miles,	10·90 miles,	−3·73 miles,	−1·23 miles.
$\delta b =$	3·85 miles,	3·93 miles,	−2·20 miles,	−1·60 miles.

§ 3. *The deviation of the local elliptic arc from the form of the mean ellipse.*

6. The four several ellipses enumerated in the last section, representing the form of the arc between Kaliana and Damargida under different data and methods of calculation, are not necessarily concentric with the mean ellipse; but they must have their axes parallel to those of the mean ellipse, because the latitudes are measured from the same or parallel lines.

Suppose one of these four ellipses drawn through the extremities of the arc, Kaliana —Damargida, and an ellipse equal to the mean ellipse also drawn through those two fixed points, with the axes of the ellipses parallel to each other. Let a, b, ϵ be the semi-axes and ellipticity of the first of these, α and β the coordinates to its centre measured from some fixed point near that centre, and therefore near the centre of the earth. The squares and products of $a-b$, ϵ, α and β may be neglected. Let s be the length of the arc, R the distance of the point of the arc in mid-latitude from the origin of coordinates, l and l' the observed latitudes of the extremities (viz. 29° 30′ 48″ and 18° 3′ 15″), λ and m the amplitude and middle-latitude of the arc. I proceed to find the difference in length, and the distance at the mid-latitude of the local and mean arcs lying between the two stations, and also the distance of the centre of the local ellipse from that of the ellipse equal in dimensions to the mean ellipse, but drawn through the two stations at the extremities of the arc, as described above.

7. *First.* The difference in length of the arcs.

$$s=\tfrac{1}{2}(a+b)\lambda-\tfrac{3}{2}(a-b)\sin\lambda\cos 2m.$$

Let c be the chord, r and θ, r' and θ' the polar coordinates from the centre of the ellipse to the extremities of the arc;

$$\therefore\ c^2=r^2+r'^2-2rr'\cos(\theta-\theta')=2rr'\{1-\cos(\theta-\theta')\}+(r-r')^2,$$
$$r=a(1-\epsilon\sin^2 l),\quad r'=a(1-\epsilon\sin^2 l').$$

Also $$\tan\theta=(1-2\epsilon)\tan l,$$

$$\therefore\ \theta=l-\epsilon\sin 2l,\quad \theta-\theta'=\lambda-2\epsilon\sin\lambda\cos 2m;$$

$$\therefore\ 1-\cos(\theta-\theta')=1-\cos\lambda-2\epsilon\sin^2\lambda\cos 2m=2\sin^2\tfrac{1}{2}\lambda\left\{1-2\epsilon(1+\cos\lambda)\cos 2m\right\};$$

$$\therefore\ c^2=4a^2\sin^2\tfrac{1}{2}\lambda\left\{1-2\epsilon(1+\cos\lambda)\cos 2m-\epsilon(\sin^2 l+\sin^2 l')\right\}$$

$$=4a^2\sin^2\tfrac{1}{2}\lambda\left\{1-\epsilon\{1+(2+\cos\lambda)\cos 2m\}\right\};$$

$$\therefore\ \sin\tfrac{1}{2}\lambda=\frac{c}{2a}\left\{1+\tfrac{1}{2}\epsilon\{1+(2+\cos\lambda)\cos 2m\}\right\};$$

$$\therefore\ \frac{\lambda}{2}=\sin^{-1}\frac{c}{2a}+\frac{\epsilon}{2}\left\{1+(2+\cos\lambda)\cos 2m\right\}\frac{c}{\sqrt{4a^2-c^2}}$$

$$=\sin^{-1}\frac{c}{2a}+\frac{\epsilon}{2}\left\{1+(2+\cos\lambda)\cos 2m\right\}\tan\frac{\lambda}{2}.$$

Hence $$s=a\left(-\tfrac{1}{2}\epsilon\right)\lambda-\frac{3}{2}a\epsilon\sin\lambda\cos 2m$$

$$=a(2-\epsilon)\sin^{-1}\frac{c}{2a}+a\epsilon\left\{1+(2+\cos\lambda)\cos 2m\right\}\tan\frac{\lambda}{2}-\frac{3}{2}a\epsilon\sin\lambda\cos 2m$$

$$=(a+b)\sin^{-1}\frac{c}{2a}+(a-b)\left\{1+\tfrac{1}{2}(1-\cos\lambda)\cos 2m\right\}\tan\tfrac{1}{2}\lambda.$$

Taking the variation with respect to the axes,

$$\delta s=(\delta a+\delta b)\sin^{-1}\frac{c}{2a}-\frac{a+b}{a}\frac{c\delta a}{\sqrt{4a^2-c^2}}+(\delta a-\delta b)\left\{1+\tfrac{1}{2}(1-\cos\lambda)\cos 2m\right\}\tan\frac{\lambda}{2}.$$

Since the terms are small, we may use approximate values;

$$\therefore\ \delta s=(\delta a+\delta b)\tfrac{1}{2}\lambda-2\tan\tfrac{1}{2}\lambda.\delta a+(\delta a-\delta b)\left\{1+\tfrac{1}{2}(1-\cos\lambda)\cos 2m\right\}\tan\tfrac{1}{2}\lambda$$

$$=(\delta a+\delta b)\left(\tfrac{1}{2}\lambda-\tan\tfrac{1}{2}\lambda\right)+(\delta a-\delta b)\tfrac{1}{2}\tan\tfrac{1}{2}\lambda(1-\cos 2\lambda)\cos 2m.$$

Applying this to the case in hand, we have $\lambda=11^\circ\ 27'\ 33''$ and $2m=47^\circ\ 34'\ 3''$. These lead to

$$\delta s=-0{\cdot}0003350(\delta a+\delta b)+0{\cdot}0006747(\delta a-\delta b)$$
$$=\ 0{\cdot}0003397\delta a-0{\cdot}0010097\delta b.$$

8. *Secondly.* The distance between the arcs at the mid-latitude.

The equation to the local ellipse is

$$\frac{(x-\alpha)^2}{a^2}+\frac{(y-\beta)^2}{b^2}=1.$$

Neglecting small quantities of the second order,

$$x^2+y^2=a^2+2\alpha x+2\beta y-2\epsilon(a^2-x^2),$$

$$\therefore\ r^2=a^2+2a\alpha\cos\theta+2a\beta\sin\theta-2a^2\epsilon\sin^2\theta,$$

$$\therefore\ r=a+\alpha\cos\theta+\beta\sin\theta-a\epsilon\sin^2\theta.$$

Let R, C, C′ be the values of r at the mid-latitude and at the extremities of the arc;

$$\therefore\ \mathrm{R}=a+\alpha\cos m+\beta\sin m-(a-b)\sin^2 m,$$
$$\mathrm{C}=a+\alpha\cos l\ +\beta\sin l\ -(a-b)\sin^2 l,$$
$$\mathrm{C}'=a+\alpha\cos l'+\beta\sin l'-(a-b)\sin^2 l'.$$

Multiply by 1, M, N, add and make the coefficients of α and β vanish;

$$\therefore\ \cos m+\mathrm{M}\cos l+\mathrm{N}\cos l'=0,\ \ \sin m+\mathrm{M}\sin l+\mathrm{N}\sin l'=0;$$
$$\therefore\ \mathrm{M}=-\frac{\sin(m-l)}{\sin(l'-l)}=-\frac{1}{2}\sec\tfrac{1}{2}\lambda=\mathrm{N};$$
$$\mathrm{R}+\mathrm{MC}+\mathrm{NC}'=a(1+\mathrm{M}+\mathrm{N})-(a-b)(\sin^2 m+\mathrm{M}\sin^2 l+\mathrm{N}\sin^2 l')$$
$$=a(1+2\mathrm{M})-\tfrac{1}{2}(a-b)\{1-\cos 2m+2\mathrm{M}(1-\cos\lambda\cos 2m)\}$$
$$=\tfrac{1}{2}(a+b)(1+2\mathrm{M})+\tfrac{1}{2}(a-b)(1+2\mathrm{M}\cos\lambda)\cos 2m$$
$$=\tfrac{1}{2}(a+b)\left(1-\sec\tfrac{1}{2}\lambda\right)+\tfrac{1}{2}(a-b)\left(1-\cos\lambda\sec\tfrac{1}{2}\lambda\right)\cos 2m.$$

Taking the variation with respect to the axes,

$$\delta\mathrm{R}=\tfrac{1}{2}(\delta a+\delta b)\left(1-\sec\tfrac{1}{2}\lambda\right)+\tfrac{1}{2}(\delta a-\delta b)\left(1-\cos\lambda\sec\tfrac{1}{2}\lambda\right)\cos 2m.$$

Put $\lambda=11^\circ\ 27'\ 11''$, $2m=47^\circ\ 34'\ 25''$,

$$\delta\mathrm{R}=-0{\cdot}0025078(\delta a+\delta b)+0{\cdot}0050586(\delta a-\delta b)$$
$$=\ 0{\cdot}0025508\delta a-0{\cdot}0075664\delta b.$$

9. *Third.* The coordinates to the centre of the local ellipse from the centre of the ellipse equal to the mean ellipse drawn through the extremities of the arc.

By eliminating β from the two equations which give C and C′, we have

$$\alpha=\frac{(\mathrm{C}'-a)\sin l-(\mathrm{C}-a)\sin l'-(a-b)\sin l\sin l'(\sin l'-\sin l)}{\sin(l-l')}$$
$$=\frac{\mathrm{C}'\sin l-\mathrm{C}\sin l'}{\sin(l-l')}-\frac{\sin l-\sin l'}{\sin(l-l')}\{a+(a-b)\sin l\sin l'\}$$
$$=\frac{\mathrm{C}'\sin l-\mathrm{C}\sin l'}{\sin(l-l')}-\frac{\cos m}{\cos\frac{1}{2}\lambda}\left\{a+\tfrac{1}{2}(a-b)(\cos\lambda-\cos 2m)\right\}$$

Also

$$\beta=\frac{(\mathrm{C}-a)\cos l'-(\mathrm{C}'-a)\cos l+(a-b)(\sin^2 l\cos l'-\sin^2 l'\cos l}{\sin(l-l')}$$
$$=\frac{\mathrm{C}\cos l'-\mathrm{C}'\cos l}{\sin(l-l')}-\frac{\cos l'-\cos l}{\sin(l-l')}\left\{a-(a-b)(1+\cos l\cos l')\right\}$$
$$=\frac{\mathrm{C}\cos l'-\mathrm{C}'\cos l}{\sin(l-l')}-\frac{\sin m}{\sin\frac{1}{2}\lambda}\left\{b-\tfrac{1}{2}(a-b)(\cos\lambda+\cos 2m)\right\}.$$

Taking the variations with respect to a and b,

$$\delta\alpha = -\frac{\cos m}{\cos\frac{1}{2}\lambda}\left\{\delta a + \frac{1}{2}(\delta a - \delta b)(\cos\lambda - \cos 2m)\right\}$$

$$\delta\beta = -\frac{\sin m}{\cos\frac{1}{2}\lambda}\left\{\delta b - \frac{1}{2}(\delta a - \delta b)(\cos\lambda + \cos 2m)\right\}.$$

Put $\lambda = 11^\circ\ 27'\ 11''$, and $2m = 47^\circ\ 34'\ 25''$,

$$\begin{aligned}\delta\alpha &= -0{\cdot}9196678\delta a - 0{\cdot}1404082(\delta a - \delta b)\\ &= -1{\cdot}0600760\delta a + 0{\cdot}1404082\delta b,\\ \delta\beta &= -0{\cdot}4053130\delta b + 0{\cdot}3353533\ (\delta a - \delta b)\\ &= \ \ 0{\cdot}3353533\delta a - 0{\cdot}7406663\delta b.\end{aligned}$$

10. The formulæ I have thus obtained are as follows:—

$$\begin{aligned}\delta s &= \ \ 0{\cdot}0003397\delta a - 0{\cdot}0010097\delta b,\\ \delta R &= \ \ 0{\cdot}0025536\delta a - 0{\cdot}0075756\delta b,\\ \delta\alpha &= -1{\cdot}0600760\delta a + 0{\cdot}1404082\delta b,\\ \delta\beta &= \ \ 0{\cdot}3353533\delta a - 0{\cdot}7406663\delta b.\end{aligned}$$

The values of δa, δb have been found in paragraph 5 for the Four Ellipses. By substituting them in these formulæ we are able to compare the ellipses with the mean ellipse. The results of this substitution are gathered together in the following Table, which contains also the semi-axes and ellipticities:—

	Mean Arc.	Arc I.	Arc II.	Arc III.	Arc IV.
a =	feet. 20926500	feet. 20985260	feet. 20984066	feet. 20906792	feet. 20919988
b =	20855400	20875737	20876151	20843795	20846981
ε =	$\frac{1}{294}$	$\frac{1}{193}$	$\frac{1}{193}$	$\frac{1}{332}$	$\frac{1}{287}$
δa =	.....	miles. 11·13	miles. 10·90	miles. −3·73	miles. −1·23
δb =	.....	3·85	3·93	−2·20	−1·60
δs =	.....	− 0·0000761	− 0·0002654	0·0009542	0·0011977
δR =	.....	− 0·0007445	− 0·0019379	0·0071414	0·0089801
$\delta\alpha$ =	.....	−11·26	−11·00	3·65	1·08
$\delta\beta$ =	.....	0·88	0·74	0·38	0·77

§ 4. *The difference between the geodetic and astronomical amplitudes, in the Indian Arc between Kaliana and Damargida, arises solely from local attraction affecting the plumb-line, and in no degree whatever from any deviation of the curvature of the arc from that of the mean arc.*

11. The differences of the length of the four arcs, and of that of the mean arc between Kaliana and Damargida, are, by the Table in the last paragraph,

−0·0000761 mile, −0·0002654, 0·0009542, 0·0011977.

These, converted into seconds, at the estimate of 69·5 miles to one degree, or 1 mile to 51″·8, are

$$-0''{\cdot}00394,\ -0''{\cdot}01375,\ 0''{\cdot}04943,\ 0''{\cdot}06204,$$

which are absolutely insensible.

From this it follows, that the length of the arc lying between its two extremities will be sensibly the same, whether it coincide with the mean ellipse in curvature, or with any of the other ellipses enumerated in § 2. Of course, on determining the geodetic amplitude from the formula

$$s=\tfrac{1}{2}(a+b)\lambda-\tfrac{3}{2}(a-b)\sin\lambda\cos 2m,$$

the amplitude will come out differently for these different ellipses, although s is the same. But the fact, that the length of the arc is the same whatever the curvature of the arc (within the recognized limits), leads to this result—that the geodetic amplitude calculated from the length measured by the Survey, by means of the above formula applied to the mean ellipse, will be the amplitude corresponding to the mean ellipse, however much the actual arc differs from the mean ellipse owing to geological or other causes. Hence the deflection of the plumb-line in India from the normal to the mean ellipse can in no degree be attributed to the possible or probable circumstance of the curvature of the arc differing from the mean ellipse, as it may differ materially from it without producing this effect.

I may illustrate this still further by finding what amount of deviation in the curvature there may be without producing even 1″ in the calculated length. The formulæ of paragraph 10 give

$$\delta s=0{\cdot}0003397\delta a-0{\cdot}0010097\delta b,$$
$$\delta R=0{\cdot}0025536\delta a-0{\cdot}0075756\delta b.$$

Eliminate δa, and these give

$$\delta R=7{\cdot}517\delta s+0{\cdot}000015\delta b.$$

Putting $\delta s=1''=\frac{1}{51{\cdot}8}$ mile, and neglecting the second term,

$$\delta R=0{\cdot}1451 \text{ mile}=\tfrac{1}{7}\text{th of a mile.}$$

The surface of the earth may, therefore, be elevated or depressed through one-seventh part of a mile at the middle parts of the arc (about 800 miles long) without producing more than 1″ difference in the length of the arc.

The Table in paragraph 10 shows that the length of the Indian Arc, according to no one of the four different measures which have been made of it, differs by even $\frac{1}{800}$th of a mile, and in three cases even by much less than this, from the ellipse equal to the mean ellipse.

12. The deviations brought out by the Indian Survey must arise, therefore, altogether from local attraction. The effect of the two visible causes—the Mountain-Mass and the

Ocean—have been calculated approximately, and are found to produce the deviations 13″·11 and 3″·82, making the astronomical amplitudes so much less than those calculated geodetically. But the Survey makes these deviations 5″·24 and −3″·79. There must, therefore, be some other source of attraction which increases the amplitudes by the differences of these, viz. by 7″·87 and 7″·61. We must attribute this to those hidden and unknown causes which lie below in the crust of the earth, where, as I have shown, causes, sufficient to produce a sensible deflection in the plumb-line, calculation proves may easily be supposed to reside. The following appears to me to be the simplest hypothesis regarding the variation below to account for these quantities, 7″·87 and 7″·61, which, it will be observed, are nearly equal to each other, that appertaining to the northern portion of the arc being somewhat larger than the other.

If the density of the crust deviates by $\frac{1}{100}$th part from the density given by the fluid-law through a cubic space, measuring 200 miles parallel and at right angles to the meridian and 200 miles deep, and situated with the centre of its upper surface at Kalianpur, then the Table in par. 2 shows that the deflections caused by the attraction of the upper and lower halves of this mass at a distance 379 miles from the centre of the upper surface on a point on the surface of the earth, along the chord of the arc, are 1″·94+1″·62=3″·56.

If the deviation in density be twice this, viz. $\frac{1}{50}$th part of the fluid-density, this deflection becomes 7″·12, very nearly the quantities to be accounted for. Now Kaliana is 371 miles, and Damargida is 430 miles from Kalianpur: these do not differ much from 379.

It is very conceivable, therefore, that the deflections 7″·87 and 7″·61, which have to be accounted for, arise from a slight excess of density of about $\frac{1}{50}$th part prevailing through a circuit of about 100 or 120 miles around Kalianpur, and to a depth of about 200 miles. Of course an indefinite number of other similar hypotheses might be framed to account for the deflections, but hardly one so simple as this. If we adopt the hypothesis of deficiency of matter beneath the Mountain Mass, we must suppose a similar deficiency to exist south of Damargida towards Cape Comorin and the Ocean; but as two independent hypotheses are here necessary, this solution is not so simple as the one I have adopted above.

13. Had I foreseen the result of the demonstration given in this communication, that a deviation in the curvature is altogether inadequate to account for any part even of the errors in the amplitudes, it would have been at once perceived, as it is now, that the errors 5″·24, −3″·79 brought out by the Survey, must arise solely from local attraction affecting the plumb-line; and these errors would have been taken, as they now must be, to be the accurate measures of the differences of total local attraction at the three stations.

The calculations of Himmalayan and Ocean Attraction are nevertheless of considerable importance. Without them we should most probably have remained ignorant of the large amount of deflection due to that cause.

14. The numerical values of δR at the end of § 3 show that the Indian Arc, as represented by the first and second of the four measures I have enumerated, is slightly flatter than the mean ellipse in the corresponding parts; but as represented by the third and fourth, is somewhat more curved*.

15. The calculations of the Survey bring to light, with considerable exactness, the errors in the amplitudes, or *differences* of latitude of the stations, but do not at all help us to discover what the total deflections at the stations are. These can be found only by a direct calculation of the effect of the causes, such as I have given in my former papers. If these are not determined and allowed for, the latitude of a place determined by an observation of the sun or other heavenly body must always be erroneous to the extent of the deflection of the plumb-line at the place of observation. Thus, if the estimate of the hidden cause of disturbance in the instance of the Indian Arc, as above given, be accepted, the deflections at the three stations are—

Arising from the Mountains . .	27″·98	12″·05	6″·79
Arising from the Ocean	6·18	9·00	10·44
Arising from the Hidden-cause .	−7·87	0·00	7·61
Totals	26·29	21·05	24·84

* In this Paper only the two northern portions of the Arc are considered. In this note I will give the results of a comparison of the three great divisions of the whole arc from Kaliana to Punnœ near Cape Comorin. The Curve of the Sea-level—for this is, of course, what we mean by the Arc, as it is this which the Survey calculates—is defined by the angles between the normals (that is, the plumb-lines) at the extremities of its three divisions, the lengths of the intervening arcs in feet, and the middle latitudes. The data are as follows: they are taken from p. 757 of the Volume of the British Survey lately published. They differ slightly from those given at p. 427 of Colonel Everest's Volume of 1847: the results of the first comparison below therefore differ slightly from those in the text of this Paper, in the first of the four measures of the Indian Arc enumerated in § 2.

	Astronomical amplitude.	Length in feet.	Twice the middle latitude.
Arc I. Kaliana to Kalianpur	(λ) 5° 23′ 37″·060,	(s) 1961138,	$(2m)$ 53° 38′ 0″
Arc II. Kalianpur to Damargida ...	(λ') 6 3 55·970,	(s') 2202905,	$(2m')$ 42 10 27
Arc III. Damargida to Punnœ	(λ'') 9 53 44·160,	(s'') 3591784,	$(2m'')$ 26 12 46

Putting these in the formulæ

$$\frac{a-b}{2}=\frac{1}{3}\frac{\frac{s}{\lambda}-\frac{s'}{\lambda'}}{\cos 2m'-\cos 2m'}, \qquad \frac{a+b}{2}=\frac{\frac{s}{\lambda}\cos 2m'-\frac{s'}{\lambda'}\cos 2m}{\cos 2m'-\cos 2m}$$

and

$$\delta R=\frac{1}{2}(\delta a+\delta b)\left(1-\sec\frac{1}{2}\lambda\right)+\frac{1}{2}(\delta a-\delta b)\left(1-\cos\lambda\sec\frac{1}{2}\lambda\right)\cos 2m,$$

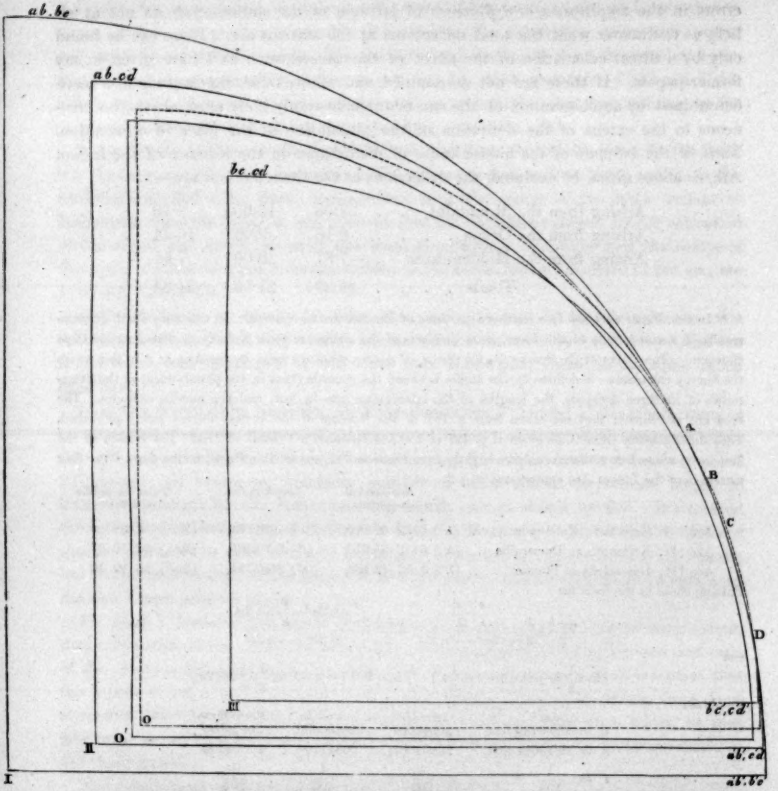
we obtain

	Arc I.	Arc II.	Arc III.	Mean ellipse.
a	=20983139 feet,	20943697,	20893936,	20926500
b	=20875011 feet,	20863963,	20841164,	20855400
s =	$\frac{1}{194}$	$\frac{1}{262}$	$\frac{1}{396}$	$\frac{1}{294}$
δa =	56639 feet,	17197,	−32564	
δb =	19611 feet,	8563,	−14236	
δR =	−5·8 feet,	−4·6 feet,	−4·4 feet.	

These results are exhibited in the following diagram. ABCD is the actual arc or sea-level curve connecting

By these quantities will the latitudes of the three places, as determined by observations of the heavenly bodies, be wrong. The existence of such large discrepancies would not

Kaliana, Kalianpur, Damargida, and Punnœ; ab, bcABC$a'b'$, $b'c'$ is the ellipse I drawn through A, B, C, and having its semi-axes parallel to the semi-mean-axes, and 58639 and 19611 feet respectively longer than



those semi-axes. In the same way ellipses II. and III., as determined above, are drawn. (The parts of these three ellipses between A and D are not drawn in the diagram. The differences in the lengths of the semi-axes are made about eighty times out of proportion, that the differences between the ellipses may be visible.) The dotted ellipse about the centre O is an ellipse equal to the mean ellipse, and so drawn as to

have been known, or perhaps suspected, if the actual calculation of the attractions had not been made.

16. It will be observed that the four several elliptic arcs which I have examined, as representing the Indian Arc, have been compared, not with the mean ellipse itself, but with an ellipse equal to the mean ellipse, and supposed to be drawn through the extremities of the arc. By the calculations of one part only of a meridian line it is impossible to determine the position of the centre of the mean ellipse, and therefore to ascertain how much the arc may have been upheaved or depressed with reference to the original centre of the earth when in a fluid state. It would require the survey of the whole meridian from pole to pole to determine this.

§ 5. *Conclusions from the whole investigation regarding the Indian Arc.*

17. The results finally arrived at may be stated as follows:—

(1) Colonel EVEREST discovered that the astronomical amplitudes of the two portions of the Indian Arc between Kaliana and Kalianpur, and between Kalianpur and Damargida, are, the first less by 5″·24, and the second greater by 3″·79, than the geodetic amplitude calculated with the mean semi-axes and ellipticity of the earth.

(2) The geodetic amplitudes of these two portions of the arc, calculated from the measured lengths and with the mean axes, will come out sensibly the same, even should the curvature of the arc differ from that of the mean meridian within reasonable but wide limits—a thing which geology teaches us to be very likely the case.

(3) Hence the geodetic measurements of the Survey being without sensible error, as is known by the tests applied, the discrepancy mentioned in (1) can arise only from local attraction affecting the vertical line, and so changing the astronomical amplitudes.

(4) Two great visible causes of disturbance of the vertical by attraction are, the Mountain Mass on the north of India, and the Ocean on the south. The influence of both of these is felt all over India; the first producing a northerly deflection varying from 27″·98 at Kaliana to a sensible angle (probably about 3″, but this I have not calculated) at Cape Comorin; the second producing also a northerly deflection, varying from about 19″·71 at Cape Comorin to 6″·18 at Kaliana.

(5) The combined effect of these two visible causes is to make the astronomical amplitudes of the upper arc 13″·11 too small, and of the lower 3″·82 also too small. They are therefore insufficient to account for the discrepancies pointed out by Colonel

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pass through A and D, the extremities of the whole arc. The ellipse about the centre O′ is the mean ellipse itself, the position of which with respect to our starting line (viz. the arc ABCD) is not known, except that its axes are parallel to those of the other ellipses. The values of δR above deduced show how very little the Indian Arc differs in curvature from the curvature of the mean ellipse in the same latitudes: and that it is very slightly flatter. How much other parts of the meridian may differ in curvature from the mean ellipse it is impossible to say, and therefore how much the Indian continent may be bodily upheaved or depressed below the mean ellipse. To determine this would require, as stated in the text, the survey of a whole meridian line.

EVEREST. Some other cause must exist tending to increase the upper astronomical amplitude by $13''{\cdot}11-5''{\cdot}24=7''{\cdot}87$, and also to increase the lower amplitude by $3''{\cdot}82+3''{\cdot}79=7''{\cdot}61$.

(6) It has been demonstrated that a slight but wide-spread variation in the density of the crust from that deduced from the fluid-theory, either in excess or defect, such as there is no difficulty in conceiving to exist, is sufficient to account for deflections such as these. For example, an excess of density amounting only to $\frac{1}{10}$th part, extending through a circuit of about 120 miles around the mid-point of the whole arc between Kaliana and Damargida (and therefore not far from Kalianpur), and to a depth of about 200 miles, will produce this effect, and make the calculated deflections from the three causes—the Mountains, the Ocean, and this Hidden Cause below—exactly accord with the observed errors in the astronomical amplitudes.

(7) The resulting total deflections at Kaliana, Kalianpur, and Damargida, arising from the three causes, are $26''{\cdot}29$, $21''{\cdot}05$, and $24''{\cdot}84$: these make the two astronomical amplitudes, the one $5''{\cdot}24$ smaller, and the other $3''{\cdot}79$ larger than the geodetic amplitudes, the errors brought to light by Colonel EVEREST.

(8) No sensible error can arise in the relative situation of places determined by geodetic measurements, and arranged in a map. But the position assigned to the map itself on the surface of the mean spheroid will be affected by local attraction; viz. by the error at the station the latitude of which is observed in order to fix the map. This error may amount to as much as half a mile. Any station afterwards inserted in the map, from an observation of the sun, will be out of its place on the map, by the difference of the errors arising from local attraction at that station and at the principal station which fixes the position of the map on the spheroid. The calculation shows that this error may amount in some places to as much as one-tenth of a mile.

XXV. *Notes on the Generative Organs, and on the Formation of the Egg in the Annulosa.*

By JOHN LUBBOCK, *Esq.*, *F.R.S.*

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MYRIAPODS.

THROUGH the labours of BRANDT, FABRE, NEWPORT, STEIN, TREVIRANUS, and other eminent naturalists, we are tolerably well acquainted with the anatomy of the generative organs in the Myriapods; but these observers have occupied themselves principally with the arrangement and forms of the organs, and have not paid much attention to the different stages of egg-development, nor to the relation in which the young egg stands with reference to the surrounding tissues. This relation is indeed very curious, and seems to have been generally misunderstood. It is well known that the Myriapods have not long egg-tubes, as is the case with most insects, but that each egg arises in a separate follicle. It was, however, natural to suppose that this follicle held the same position with reference to the ovary as the very similar egg-follicles of certain insects, as, for instance, of *Coccus*. This, however, is by no means the case. In Plate XVII. fig. B, I have given a diagrammatic section of the ovary of *Coccus*, with a single egg-follicle (*a*), the vitelligenous cells being represented at *b*, and the Purkinjean vesicle at *c*.

If, now, we compare with this a similar diagram of the ovary in *Glomeris* (Plate XVII. fig. A), also with a single egg-follicle, we shall see that this latter is very much alike in both cases—the shape of the egg-follicle (*a*), the Purkinjean vesicle (*c*), and the vitelligenous bodies (*v*) being very similar; but whereas in *Coccus* and in all insects the egg-follicle projects *from* the ovary, in *Glomeris* and the other Myriapods, so far as my observations go, the follicle projects *into* the ovary. If, therefore, we consider the ovary as consisting of an outer membrane (*d*) and an inner epithelial layer (*e*), it would appear that while the egg in the Myriapods arises between these two layers, in the insects it originates on the inner side of both.

This difference appears to me to be very important, and, as will be mentioned under the head of *Iulus*, escaped the attention of our great anatomist NEWPORT, and led him to give an erroneous description of the ovary in that genus. I have chosen to compare *Glomeris* with *Coccus* in the above-mentioned diagrams, because the vitelligenous bodies make the resemblance of, and at the same time the difference between, these two genera more striking. If, however, we compare with them a similar diagram of the ovary of *Phalangium* (Plate XVII. fig. C), we shall see, not only that the vitelligenous bodies are absent, but that the egg-follicle differs equally from that of the insect on the one hand, and that of the Myriapod on the other. The egg-follicle projects *from* the

ovary as in *Coccus*, &c.; but, on the other hand, the Purkinjean vesicle lies on the outer side of the epithelial layer (*d*), as in *Glomeris*, and in consequence the egg-follicle, which in *Coccus* consists of both the ovarian membranes (so far as the epithelial layer can be called a membrane), and in *Glomeris* of the epithelial layer only, is in the Phalangidæ, and perhaps all the Arachnida, formed only by the outer membrane. Consequently, while in the insects the mature ovum passes into the ovary through the neck of the follicle, in Myriapods and the Arachnida it bursts through the epithelial layer, in the former at its free, and in the latter at its attached end.

If these characteristics are found eventually to hold good throughout the Myriapods and Arachnida, the differences thus shown to exist between these groups will be of great interest; but it is too early to generalize on the subject with much confidence. Moreover, it often happens that one or a few epithelial cells *are* attached in Arachnida to the inner side of the follicle-wall. This happens, however (so far as my observations go), without any regularity, and the cells thus present fulfil perhaps no important function in the formation of the egg. The Crustacea* appear to differ from the three preceding groups in the fact that their eggs do not possess separate follicles. The nuclei of the epithelial cells in the ovary of *Oniscus* are about $\frac{3}{1000}$ths of an inch in diameter, with a bright nucleolus; but along the inner edge of the ovary one or two may generally be found as much as $\frac{6}{1000}$ths of an inch in diameter, but not otherwise differing from the small ones. Between these extremes all intermediate stages may with patience be discovered.

When the epithelial nucleus has attained the above size, or even a little earlier, a deposit of dark, structureless matter appears round it. This is the commencement of the yelk, and has at first no bounding membrane, as the membrane of the cell itself seems to perish at a very early stage. It soon, however, acquires a clearly marked outline, though it is not until it is full-grown that any true membrane is formed round it. The outline is, however, so clear and sharp that it is often difficult to believe that there is in reality no membrane. Pressure, however, clearly shows that there is none.

In *Ligia oceanica* the process is almost exactly similar; and although the animal is so much larger, the epithelial cells, their nuclei, and the young Purkinjean vesicles are as nearly as possible of the same size as in *Oniscus*.

* To M. Schöbl (Zeitschrift für Wiss. Zool. 1860, p. 465) is, I believe, due the credit of being the first to describe the position of the female orifice and the presence of a spermatheca in the Oniscidæ. I had myself independently made the same observations in *Oniscus* as he has in his new genus *Haplophthalmus*, and can therefore confirm what he says on the subject, though the size of the eggs is so great, compared with that of the orifice, that I am still at a loss to understand how they make their exit. M. Schöbl found the spermatheca full of spermatozoa in May only, but in *Oniscus* I found them in January, February, June, and August, the only months in which I looked for them. They were always motionless, and I never found in the female any of the large round cellular bodies which occur in the generative organs of the male. The spermatozoa were not confined to the spermatheca, but I met with them also in the oviduct, and even frequently (if not always) in the ovary itself.

Leuckart* has already noticed a remarkable vesicle which was observed by him in the yelk of *Geophilus*. I have not convinced myself as to the presence of any such body in that genus, but have observed a vesicle constantly present in the yelk of *Arthronomalus*, a genus which is closely allied to *Geophilus*, and which was probably the subject of Leuckart's examination. A smaller vesicle, probably however of a similar nature, also occurs in *Lithobius*, and a still more rudimentary structure also appears to exist in *Iulus*. In *Glomeris*, as already mentioned, vitelligenous cells, not unlike those of *Cocçus*, are present.

Chilognaths.

Glomeris marginata, Plate XVI. figs. 1, 2, 3.—In this species, and probably in all the Chilognaths, there are two vulvæ, which are situated in the third segment, and open into two short egg-canals. These soon unite into a common oviduct, which again in its turn quickly expands into a large, simple, cylindrical ovary. This ovary extends in a straight line back nearly to the posterior pair of legs, and lies immediately below the intestinal canal. It is extremely delicate, and so transparent that it might easily be overlooked if it was not rendered conspicuous by the opake whiteness of the eggs contained in it. These, however, are not produced indiscriminately over the whole surface of the ovary, but only along two ribbon-shaped parts which run along the lower lateral portions of the ovary, almost from one end to the other, and thus leave the whole of the upper wall of the ovary, the sides, and a narrow strip along the middle of the ventral surface quite free from eggs. When the egg-germs are still quite small, these two "stromas" have the appearance of two separate ovaries; and it is probable that, as M. Fabre has suggested in his excellent memoir "Sur l'anatomie des organes reproducteurs et sur le développement des Myriapodes†," it is from having examined the animal at this stage, and from having overlooked the ovarian walls, that the earlier observers were led to describe the organ as being double‡.

The follicles in which the eggs are developed also probably contributed in no small degree to this error, since they do not project, as one would naturally expect they would do, from the outer surface of the ovary, but, on the contrary, protrude into its central cavity; so that even if the delicate walls of the ovary are perceived, they may easily be mistaken for a band of some tissue intended to keep the two supposed ovaries in their place. The walls of the follicle consist of nucleated epithelial cells (Plate XVI. fig. 1), and each follicle contains only a single egg. The follicle is at first small, but grows in size as the egg-matter contained in it increases in volume, and takes an elongated

* Art. Zeugung.

† Annal. des Sci. Nat. 1855.

‡ See Treviranus, Verm. Schr. p. 45, who, however, has correctly seen the two strings of eggs; Brandt, Müller's Archiv, p. 837; and Stein, Müller's Archiv, 1842, p. 248. M. Fabre, though he has correctly explained the relation of the different parts, still describes the ovary as double, since he denies the name of ovary to the central sac, and applies it only to the two stromas, in which, according to him, the eggs are formed. It is not, however, quite correct to say that the eggs are formed in the stromas, since in reality they arise in follicles which are only attached to the stroma by their base.

oval form. At the free end of each follicle, opposite its place of attachment to the stroma, is a place deficient in epithelial cells, the space being apparently occupied by a thick layer of some amorphous substance.

The specimens which I examined in July and August contained some eggs $\frac{1}{80}$th of an inch in diameter, and of an opake white appearance, with others in all earlier stages of development.

I was not able to ascertain with certainty the origin of the Purkinjean vesicle; but in the youngest egg-follicle it only differed in size and in the solid appearance of the nucleus from the ordinary epithelial cell. I have therefore little doubt that the Purkinjean vesicle is a modified epithelial cell, and that the "macula" of WAGNER is homologous with its nucleus. The smallest Purkinjean vesicle I observed was $\frac{1}{1600}$th of an inch in diameter, the follicle itself being only about double the size. The macula was distinctly visible; and neither in this one, nor in others rather more advanced, could I see the vitelligenous bodies.

The young egg-follicles, at a somewhat later stage, strikingly resemble those of *Coccus* before the constriction has commenced and when the epithelial cells are not very conspicuous; and this similarity is the more striking because we find in *Glomeris* vitelligenous bodies (Plate XVI. figs. 1 & 2, *v*) like those of insects, and specially like those of *Coccus*. The size and number is certainly more variable, but not in any great degree, five or six being the largest, and three or four the commonest number, and the usual relative size being also that which is generally found in *Coccus*. The yelk-substance is at this period more or less clear and transparent, and no oil-globules have been formed in it, so that the Purkinjean vesicle and spot are easily seen (Plate XVI. fig. 1, *p*); the vitelligenous bodies, on the contrary, so nearly resemble the yelk-substance in colour and consistence, that only in a few instances their outlines can be perceived, and then only in part and with difficulty.

This apparent similarity, however, does not prove any real identity of composition, as may readily be shown by the addition of a little acetic acid. No sooner is this done, than the yelk-substance becomes considerably darkened; and as the vitelligenous bodies remain unaltered, the contrast makes them very clearly visible; so that if a number of egg-follicles are in the field of view, the difference which is thus effected in their appearance is very striking (Plate XVI. fig. 2). As usual, the acetic acid renders the cell-wall of the Purkinjean vesicle almost invisible, and wipes out, as it were, the macula; but the contrast of colour leaves the vesicle itself very plain.

Under this treatment the Purkinjean vesicle very much resembles a vitelligenous cell, though it is generally rounder; and if the acid has been very weak, the ghost, so to say, of a macula may still be perceived, and sometimes a very similar nucleus may be seen in some of the vitelligenous bodies. This very seldom happened; but I have figured one of the few cases observed, in Plate XVI. fig. 2. Perhaps, therefore, in *Glomeris* the vitelligenous body is homologous with the Purkinjean vesicle, and the nucleus of the one with the macula of the other. According, however, to MEYER's observations, it

would appear that in insects, as the Purkinjean vesicle surrounds itself with a well-defined portion of yelk, the original vitelligenous nucleus also forms a pseudo-cell; so that the vitelligenous body of *Glomeris*, which corresponds with the Purkinjean vesicle, is also homologous with the nucleus of the vitelligenous pseudo-cell of insects.

The action of tartaric acid is much like that of acetic acid. Ammonia renders the macula invisible, and the Purkinjean vesicle almost invisible, but it does not make the yelk-matter dark. Water does not act energetically on these tissues.

When the egg-germ has attained a length of about $\frac{1}{100}$th of an inch, the vitelligenous bodies have gradually disappeared, and the yelk-substance begins to present, under transmitted light, a dark colour, like that which at an earlier stage it assumes under the action of acetic acid. The Purkinjean vesicle can still be seen indistinctly; but when the follicle has attained to a length of $\frac{1}{70}$th of an inch it has become quite opake, and the Purkinjean vesicle can only be seen after the application of pressure.

Up to this time, however, the yelk is surrounded by no vitelline membrane, and indeed the majority of the oil-globules seem to be produced in the stroma, and not in the egg-follicle itself. The yelk consists of a clear fluid, containing an immense quantity of small oil-globules, varying up to $\frac{1}{800}$th of an inch, of which size there are a great number.

The largest follicles were about $\frac{8}{300}$ths of an inch in length, and contained eggs which were already surrounded by a firm chorion. On carefully tearing this open, the Purkinjean vesicle could generally be perceived. It was from $\frac{1}{300}$ths to $\frac{1}{400}$ths of an inch in diameter, spherical and transparent, but the wall was often, as it were, stained in places. When seen sideways this gave the effect of a patch, as in fig. 3; but when looked at from in front it could only be seen faintly, and had, under a high power, no definite outline.

The macula at this stage consisted generally, if not always, of two vesicles, one of them more than twice as large as the other. The latter, which is no doubt derived from the large one, first appears when the Purkinjean vesicle is about $\frac{8}{300}$ths of an inch in diameter. Under the action of acetic acid it disappears, as does the large macula; and its occurrence is very constant, since I found it in every Purkinjean vesicle which was more than $\frac{10}{300}$ths of an inch in diameter.

I did not find any mature eggs until the beginning of September; and even at this time many females did not contain any; and in all there were only a few eggs which had escaped from their capsules. These lay free in the cavity of the ovary or oviduct; they had a leathery chorion, and, like the eggs of insects, seemed to possess no second envelope. They are about $\frac{1}{15}$th of an inch in size, and of a broad elliptic shape.

I could find in them no trace of the Purkinjean vesicle, nor of the maculæ. The yelk consisted, 1st, of an apparently viscid substance; 2ndly, of minute molecules; and 3rdly, of oil-globules from $\frac{1}{10000}$th to $\frac{8}{10000}$ths of an inch in diameter. If they were

placed between two slips of glass, and these latter were rubbed together, the oil-globules ran into one another and took the form of rods, thus showing that they possessed no true membrane.

Iulus.—The most detailed account of the generative organs, and formation of the egg, in *Iulus*, is that given by Mr. Newport, in his celebrated memoir "On the Organs of Reproduction and the Development of the Myriapoda*." Mr. Newport correctly describes the ovary as consisting of an elongated bag, which "in the pregnant female is smooth and distended with ova that have passed into it from the ovisacs;" but he figures and describes these ovisacs as projecting freely from the outer side of the ovary, whereas, in fact, in *Iulus* as in *Glomeris*, the egg-follicles project *into* the free cavity of the ovary, and do not project from its outer surface. Mr. Newport was perhaps led into this error partly by what he expected to see, but also partly, no doubt, by a misinterpretation of what he actually did see. The wall of the ovary is extremely delicate, and at the same time so firmly united to innumerable tracheæ, and to parts of the fatty tissue, that it is almost impossible to detach the organ without injury; but if the wall is pierced, the egg-follicles immediately make their way out of the orifice, and the organ takes on an appearance much like that represented in his fig. 5, plate 3. The egg-follicles do not, however, clothe the whole inner surface of the ovary, but in this genus, as in *Glomeris* and *Polydesmus*, are confined to two long ribbon-shaped parts which run along almost the whole length of the organ.

In examining the early stages of the formation of the egg in the follicle, Mr. Newport unfortunately used specimens which had remained for twenty-four hours in rectified spirits of wine. In consequence, he describes the youngest egg-follicles, which were about $\frac{1}{1000}$th of an inch in diameter, as being "filled with very minute graniform cells of a uniform size (about $\frac{1}{10000}$th of an inch), slightly opake, and of a yellow colour." If, however, he had examined in water a freshly killed specimen, he would have found these follicles perfectly clear, transparent, and colourless, and the Purkinjean vesicle and macula much more conspicuous. In such follicles the yelk always seemed to me to be a clear fluid, without any cellular contents at all resembling the minute graniform cells described by Mr. Newport. The smallest egg-follicles I have observed were about the same size as those mentioned by Mr. Newport. It is only when they have attained the size of about $\frac{1}{300}$th of an inch in length, that they begin to grow dark from the deposition in their interior of a finely granular yelk.

When they are $\frac{1}{95}$th of an inch they have become quite opake; but the yelk-globules are still very small, not generally more than $\frac{1}{8000}$th of an inch in diameter. At this stage it is not easy to get a good view of the Purkinjean vesicle by crushing the egg, as, even if the vesicle is not itself destroyed, the yelk is so sticky that it is difficult to separate it sufficiently. This can, however, generally be effected by tearing the egg open carefully with two needles.

In this manner it can be ascertained that the macula is still single; but it generally

* Philosophical Transactions, 1841.

looks as if it contained one or more nuclei imbedded in a darker substance. Moreover, in addition to the large macula, which is generally more or less vesicular, there is a smaller one, as in *Glomeris*. This latter is perhaps to be compared with the small vesicular macula of *Acheta*.

In August the *Iuli* which I examined contained no mature eggs; so that they do not appear to be so forward as those of *Polydesmus*.

The cells forming the follicles, or at least their nuclei, could be well seen; and the cells themselves are indicated by small projections along the edges of the follicles, but their walls are so delicate that they can seldom be seen in a full view. From the mode of examination adopted by Mr. Newport, the histological structure of the follicle-wall quite escaped his observation.

In the mature eggs the yelk contained, 1st, globules of all sizes up to $\frac{1}{1000}$th of an inch; 2ndly, greenish spherules $\frac{1}{8000}$th to $\frac{1}{3000}$th of an inch; and 3rdly, the usual intermediate substance. The large yelk-globules are quite round, but they are not liquid, and on applying pressure they split at the edges. Sometimes a globule has a single fissure; but very often the splits radiate from the centre, or rather proceed inwards from the edges towards the centre.

In mature eggs Mr. Newport saw a "transparent globular vesicle," which he assumed to be the "proper germ-vesicle considerably enlarged;" this, however, is certainly a mistake, since the true Purkinjean vesicle has always disappeared by the time the egg is full-grown; and the vesicle in question is therefore probably the embryo-cell. As regards the final lot of the Purkinjean vesicle, I have as little in *Iulus* as in the other Myriapods been able to come to any satisfactory conclusion. The largest eggs in which I could satisfactorily see the Purkinjean vesicle were about $\frac{1}{100}$ths of an inch in diameter; and the largest vesicles were about $\frac{1}{100}$th of an inch in diameter, clear, transparent, and with a single nucleus. The macula in *Iulus* does not, therefore, break up into numerous smaller maculæ, as is the case in *Lithobius* and, according to Wittich, also in Spiders; or if this does take place, it belongs to a later period.

The yelk does not contain any vitelligenous bodies; but in a great many cases, where the yelk was beginning to darken, I observed in it an irregular, yellowish, granular patch. This patch was too irregular and amorphous to be of much functional importance, and at the same time so generally present, that it could not, I think, be altogether without significance. The patch is of a more or less oval form, and smaller than the Purkinjean vesicle, though somewhat larger than the macula; it is not present in very small eggs, nor in those which have become quite opake. I am inclined to look upon it as corresponding with the concentrically laminated body found in similar stages of the egg in some spiders; at the same time, I desire rather to throw this out as a hint than to express it as an opinion.

Acetic acid slightly darkened the yelk, and made the patch almost invisible.

Polydesmus (Plate XVI. figs. 4, 5, 6, 7).—In form and arrangement the generative organs, both male and female, in this genus resemble those of *Iulus*. The development

of the young ovules, however, is by no means the same. They do indeed form two distinct series, as is the case in all Chilognaths; but instead of eggs in all stages lying indiscriminately next to one another, and without the least arrangement, we here find all the youngest eggs occupying the inner border of the series; and as we pass across the series from the median line of the body to the sides, we pass from the youngest to the oldest eggs. Moreover each line of eggs is in approximately the same stage of growth; so that all the eggs in the outer row and all the inner row are respectively of nearly equal size, and those constituting the intermediate rows are of intermediate dimensions. The smallest eggs which I could see well were rather less than $\frac{1}{1000}$th of an inch in diameter, and quite transparent. They did not seem to be enclosed in any distinct follicle, although in later stages, when they had increased to $\frac{1}{300}$th of an inch in diameter, an epithelial membrane with distinct nuclei could be seen as distinctly as in *Iulus*. In the very young eggs, however, I could never see a trace of it; yet their margin was so clear and well marked, that it was difficult not to believe that each egg was formed by a true cell. Moreover, under the action of dilute spirits of wine the yelk darkened, and often contracted irregularly, in which case the margin of the egg remained as before, and was perfectly distinct.

We have here almost the same difficulty as in the question about the skin of certain Infusoria; but the point is perhaps of more importance, since if we regard each egg in *Polydesmus* as a specially modified cell, we could no longer consider it as homologous with the egg of certain other Myriapods, as, particularly, of *Glomeris*. I have, however, never met with any stage intermediate between the youngest eggs and the true epithelial cells, and cannot, therefore, at present solve the question.

In very young eggs the Purkinjean vesicle scarcely seems to possess any definite membrane; and sometimes, even in eggs as much as $\frac{1}{300}$th of an inch in diameter, on the addition of pure water its outline became flocculent, and finally disappeared. Different eggs, however, behave very differently in this respect. In Plate XVI. fig. 8, I have represented three eggs as they appeared at the same time in the field of view. They were dissected out in sugar and water, and then put into pure water. One (*a*) has remained unaltered, except that the macula has become, as usual, darker; in the second (*b*) the Purkinjean vesicle has disappeared and the macula remains; in the third (*c*) the macula has disappeared and the Purkinjean vesicle remains.

I did not see in *Polydesmus* anything equivalent to the "patch" which occurs in the young eggs of *Iulus*. In the smallest eggs the macula was already very distinct. In a few cases it seemed to contain one or more nucleoli, but generally it gave me the impression of being a more or less solid body attached to the wall of the Purkinjean vesicle. At first the Purkinjean vesicle was $\frac{1}{1000}$th or $\frac{1}{1250}$th of an inch in diameter, or about half as large as the egg; when, however, the egg has increased to $\frac{1}{150}$th, the Purkinjean vesicle has only increased to about $\frac{1}{1000}$th. At this stage the yelk-granules commence to be formed in the egg; they are at first very small, and give it a brownish appearance.

The macula increases in size nearly in proportion to the growth of the Purkinjean vesicle, and is single from the earliest stage observed up to the latest, but appears often to contain vacuola.

In eggs $\frac{1}{180}$th of an inch in diameter the Purkinjean vesicle is almost hidden by the dark opake yelk, whose globules are about $\frac{1}{8000}$th of an inch in diameter. At this stage it scarcely differs, except in size, from its appearance when it first became visible.

After this the egg increases but little in diameter; the yelk-globules, however, grow until they become about $\frac{3}{8000}$ths or $\frac{4}{8000}$ths of an inch in diameter, and the egg becomes quite opake. At this stage I could, on applying pressure, see a clear space, which was probably the Purkinjean vesicle; but it was so delicate, and the yelk-substance was so viscid, that I was never able to get a good view of it. Acetic acid darkened the yelk but slightly, and did not bring any vitelligenous bodies into view. It dissolves the Purkinjean vesicle and macula, as usual. Ammonia makes the whole ovary very pale, the macula invisible. It also causes the Purkinjean vesicle to disappear, but not so quickly. The epithelial nuclei are, if the ammonia is very dilute, rendered plainer than they were before. In mature eggs the yelk consists of at least three parts.

1st. Yelk-globules from $\frac{5}{8000}$ths to $\frac{8}{8000}$ths of an inch in diameter, quite clear and transparent.

2ndly. Yelk-spherules from $\frac{1}{8000}$th to $\frac{3}{8000}$ths of an inch in diameter, and of a greenish hue. These are not so regularly round as the preceding, and have a look of solidity, while under pressure the globules quite lose their shape and run into one another.

3rdly. A viscid substance in which the two first are imbedded.

The spherules and globules are generally of the size indicated, but a good many depart considerably from the average. No trace of a Purkinjean vesicle was ever seen in the mature eggs. These are nearly spherical; and when a number of them are contained in the body, it is difficult to make an incision without some of them coming immediately through it. This was the case with several females which I examined in the middle of August, and in one of which I found 400 ripe eggs, besides which there were a few which I did not count, and a great number in the ovary in all the previous stages. I presume therefore that at this period the walls of the ovary are exceedingly distended, and give way directly they are touched by any sharp instrument.

The female makes a little hollow ball of earth in which she deposits a number of eggs together. I found several of these in the glasses in which I kept these animals, while on the contrary the specimens kept in confinement by M. Fabre*, did not lay any. I unfortunately omitted to isolate these eggs, or to watch for the moment of their hatching, and cannot, therefore, state in what form the young one leaves the egg. A few days after the eggs had been laid, however, I found a number of young Polydesmi

* *Loc. cit.* p. 274.

in my glass, and they consisted of eight segments, including the head; they had three pairs of legs, which were attached to the second, fourth, and fifth segments, leaving the first, third, sixth, seventh, and eighth without feet.

By the month of October most of the females had laid their eggs. The ovary then contains another set, the largest of which were, in four specimens examined by me, just beginning to become dark. These do not appear to be laid until the following spring; at least this was the case with some specimens which I kept in confinement.

The spermatozoa much resemble those of *Phalangium*. They are small, elliptical bodies about $\frac{1}{1000}$th of an inch in length, and containing a bright, rod-like nucleus. Stein appears to be the only naturalist who has hitherto described them from personal observation. He, however, figures them as round cells, with a tendency to arrange themselves in straight lines. I, on the contrary, generally found large quantities heaped together in the testicular sacs, and in the vasa deferentia masses of spermatozoa not unlike those found in *Chelifer*. Neither in *Polydesmus* nor in *Glomeris* does Stein either mention or figure the nucleus in the elliptic or fusiform seminal cells. M. Fabre says, "Chez le *Polydesmus complanatus* je n'ai trouvé que de menus corpuscules anguleux, sans forme déterminée, réunis plusieurs ensemble en petites pelotes mamelonnées"; I also have constantly found these small masses of angular bodies in the tubular parts of the male generative organs, but not in the lateral sacs. They do not, however, appear to have any relation to the spermatozoa, and seem rather like a product of excretion.

Chilopods.

Lithobius (Plate XVI. figs. 8–13).—In the genus *Lithobius* I have examined three species—*L. variegatus*, *L. pilicornis*, and *L. Sloanei*.

The vulva is, as probably in all the Chilopods, situated at the posterior part of the body; and the ovary is in the form of an elongated sac, which extends for a longer or shorter distance towards the head. Besides having the orifice at the posterior instead of the anterior end of the body, the female generative organs of the Chilopods differ from those of the Chilognaths in the important fact, that the ovary lies *above* instead of *below* the intestine.

The eggs arise singly, each in a separate follicle, and, as I have already mentioned to be the case in Chilognaths, project *into* the general cavity of the ovary, instead of from its external surface. They occur in the same individual in all stages, and without any regularity, the largest and smallest lying side by side in the most complete confusion. The ovarian wall consists, as usual, of an outer structureless membrane, while the small epithelial cells are confined to the single stroma and the ovarian follicles.

The smallest Purkinjean vesicles which I met with were rather less than $\frac{1}{1500}$th of an inch in diameter. I did not see any cells intermediate in character between the ordinary epithelial cells and the smallest Purkinjean vesicles, but I am inclined to think that the latter are modifications of the former. The macula was distinct, and apparently consisted of several small rounded bodies more or less closely attached to one another.

The cells constituting the wall of the follicles are transparent, and vary in size from $\frac{1}{1000}$ths to $\frac{5}{1000}$ths of an inch in diameter. Their nuclei are distinct, and from $\frac{3}{1000}$ths to $\frac{3}{1000}$ths of an inch in size. Sometimes the cells are almost invisible, and the nuclei only can be seen.

In eggs rather more advanced, a certain quantity of clear yelk may be seen round the Purkinjean vesicle, and the macula consists of about seven bright, solid-looking bodies, each measuring about $\frac{1}{350}$th of an inch in diameter. The diameter of the Purkinjean vesicle is at first about half as large as that of the follicle in which it is contained, but increases in size much more slowly than the latter. When the follicle is about $\frac{1}{100}$th of an inch in diameter, the yelk begins to become dark from the deposition of granules, which are very minute, none exceeding $\frac{1}{1000}$th of an inch in size. In the mean time the maculæ have become smaller and more numerous, until in the largest Purkinjean vesicles there are a great many of them, and they are very minute.

This gradual increase in the number and diminution in the size of the maculæ occurred in all but one of the specimens examined by me. In this case many eggs, even among those which had begun to darken, contained a large macula, generally surrounded by smaller ones.

A great many of the eggs contained a vesicle (Plate XVI. figs. 11, 12, &c.) like that of *Arthronomalus*, but much smaller and with no distinct nuclei. This small vesicle could by no means always be seen; and even in those ovaries in which it was most distinctly visible, it appeared to be present only for a short time. It seemed to be succeeded by a small patch, like that already described as occurring in *Iulus*. It is not seen in eggs less than $\frac{1}{100}$th of an inch in diameter, and it can no longer be distinguished when the yelk has become quite opake. In one specimen out of twenty-six egg-follicles, larger than the above size and yet not opake, I saw it in eighteen, but in other specimens it was less constant, or even perhaps altogether absent. I have several times been inclined to look upon it as a mere accidental agglomeration of yelk; but it is, I think, too regular and too constantly present. All that I have said about the "patch" in *Iulus*, applies equally well to that of *Lithobius*.

When the egg has attained a size of $\frac{1}{100}$th to $\frac{1}{63}$rd of an inch, it has become quite opake, and without compression its contents cannot be distinguished. The yelk consists of oil-globules, varying from $\frac{1}{1000}$th of an inch down to a very minute size, and imbedded in a clear, sticky substance. On applying pressure the germinal vesicle at once comes into sight. It is $\frac{1}{1000}$ths of an inch in diameter, and shows no trace of any nucleus, but appears to contain a clear fluid, with minute granules.

At this stage the yelk seems to be surrounded by a delicate membrane, which, when the egg is mature, has become a strong shell. In eggs of this size it often happened that on applying pressure a second clear space appeared, always detached from, and three or four times as large as, the Purkinjean vesicle. This appeared to be a portion of the yelk free from the globules and granules; it had no definite outline, and gene-

rally, after the egg was crushed, could no longer be perceived; once, however, I succeeded in isolating it from the yelk, when it took the form of several clear masses apparently of a glairy substance, without any membrane, though with a clearly defined border. In this case the Purkinjean vesicle contained a number of small cellular bodies, about $\frac{1}{1000}$th of an inch in diameter, and exceedingly like the embryo-cells of *Coccus**, having the same greenish tinge and somewhat angular outline. The yelk-masses, which were at first transparent, turned an opake yellow under the action of water; but the process was much slower in some than in others. Acetic acid only slightly darkened the eggs, and did not bring any vitelligenous cells into view. It completely wiped out the macula. Under ammonia the young follicles became very faint, and the Purkinjean vesicle disappeared.

The largest eggs in which I found Purkinjean vesicles were from $\frac{1}{1000}$ths to $\frac{33}{1000}$ths of an inch in diameter, the vesicle being about one-third of that size.

The mature eggs from the cavity of the ovary were about $\frac{7}{100}$ths in diameter, and nearly spherical in shape. They contained the usual yelk-masses, of which the larger ones were $\frac{5}{1000}$ths of an inch in diameter, and under pressure split at the sides like those of *Iulus*. They contained no Purkinjean vesicle.

I was for some time much puzzled by finding among the ordinary eggs certain spindle-shaped bodies, some of which were very narrow, while others were so broad as scarcely to differ from the spherical eggs, except by having one side flattened. They were surrounded by a layer of epithelial cells, contained ordinary yelk, with generally a Purkinjean vesicle, and in fact possessed all the characteristics of true eggs. For some time I believed that, besides the spherical eggs, other more or less spindle-shaped ones were produced in the ovary of *Lithobius*, so that, as in *Daphnia*, the same ovary gave rise to two sorts of reproductive bodies. At length, however, it occurred to me to cut off part of a young egg; and I then at once found that the larger portion of the wounded egg was one of my spindle-shaped bodies, which therefore were nothing more nor less than the ova which had been accidentally cut through in the dissection of the ovary. If, however, the eggs are too large, they simply burst when cut, and the contents escape. The most suitable ones are those which contain granules and have not yet developed any large oil-globules. This observation would be scarcely worth mentioning, were it not that the cut surface of the yelk presents an outline which is as well defined as the other parts, and shows no trace of the wound. It is of course evident that this part cannot be surrounded by any membrane, and the similar appearance presented by the rest of the yelk is therefore deceptive. So definite or clear, however, is the outline, that it is difficult not to believe in the presence of a vitelline membrane, and it seems probable that the membrane described as surrounding the egg-germs of some other animals may have its origin in a similar deception.

The numbers of the two sexes seem to be nearly the same: thus out of thirteen speci-

* Philosophical Transactions, *loc. cit.* p. 364.

mens of *Lithobius pilicornis* seven were males; in *L. forficatus* the only two specimens examined were males; and out of three specimens of *L. variegatus* two were females, making altogether ten males to eight females.

The number of labial teeth is used by Newport as a specific character; but I found in this respect an astonishing want of symmetry: thus, in the few specimens examined by me, a male *L. variegatus** had seven labial teeth on one side and nine on the other; three specimens of *L. pilicornis* had six teeth on one side and only five on the other, while two others had only four teeth on one side; making altogether, out of twenty-eight specimens, nine which were abnormal in this respect.

In addition to the Gregarinæ, which are very generally found in the Myriapods, and particularly in *Polydesmus* and *Glomeris*, I have met with a few other parasites. A specimen of *Lithobius pilicornis* contained, not in the intestinal canal, but in the general cavity of the body, two Nematoid worms 3 inches in length. Two other specimens of the same species contained respectively one or two dipterous larvæ.

Cryptops (Plate XVI. figs. 19, 20, 21).—In this genus the ovary is narrow, and its walls are very delicate. The eggs are not arranged with any regularity as to size. They are at first round, but soon become elongated, with their longer axis parallel to that of the ovary, and do not appear to be so numerous as in *Lithobius*. As in most other genera of Myriapods, the macula is single at first; but in this genus, as in *Glomeris*, a secondary smaller macula may in subsequent stages almost invariably be found in the Purkinjean vesicle. It is remarkable that this genus should differ so entirely from *Lithobius* in the history of its macula. The formation of the eggs in other respects is, however, very similar in these two genera. The epithelial nuclei on the egg-follicles could in some cases be very plainly seen; but the ovary did not contain any loose nuclei or cells, except those which had already become young eggs. These latter seem to come to maturity late in the autumn. A female examined in the middle of September contained no ripe eggs; indeed the largest ones were only $\frac{1}{100}$th of an inch in length, and the yelk had only just begun to darken.

Another female, examined on the 24th of October, was more advanced; and a few of the eggs were opake, though none were yet quite ready to be laid.

Acetic acid darkened the yelk but slightly, and brought no vitelligenous bodies into view. It dissolved the macula as usual.

In all the specimens examined by me, the spermathecæ were full, but contained filiform spermatozoa *only*, without any of the ovoid cells which accompany the spermatozoa in the spermatophores. These spermatophores were, I believe, first observed by M. Fabre, and I could add little to the excellent description of them given by him. He does not, however, mention in the text†, although he has represented in his figure, certain small elliptic bodies which are found in the spermatophores with the filiform

* Another specimen, of which the sex was not determined, had only six teeth on one side; and two others had eight on one side and seven on the other.

† Unless under the name of the "pulviscule blanche," which forms a layer round the spermatozoa.

spermatozoa. These bodies are probably analogous to the vesicles which in the spermatic sacs of some Entomostraca surround the filiform spermatozoa; and the function which is ascribed to the latter, namely, that of imbibing moisture and so expelling the filiform spermatozoa, may also perhaps belong to the former. Nevertheless one cannot but be struck with the fact, that they exactly resemble in shape the elliptic bodies which are found in the testis and spermatheca of *Chelifer*, and which cannot have any such function. In the present species, however, as above mentioned, the elliptic vesicles do not appear to find their way into the spermathecæ.

M. Fabre, to whose excellent paper I have already so often alluded, made, with reference to some Chilopods, the very curious observation that the male spins a sort of nest, or rather web of silk, and deposits in the middle a drop of semen. He has not actually observed that the genus *Cryptops* has this extraordinary habit, but he thinks it probable. From the fact that the two spermathecæ of the female *Cryptops* generally contain spermatozoa, it would seem that the females must have the scarcely less remarkable instinct to visit these nests, and in some manner absorb the contents into their vagina.

I find much difficulty in imagining how, under Mr. Darwin's theory, such habits as these can have originated. It is easy enough to understand how they can continue when they have once existed long enough to harden into an instinct; but I do not understand how they can have begun. It may be hoped, however, that we shall find among other Myriapods some species with less abnormal habits, or that in some other manner new light may be thrown on the matter.

Geophilidæ.

*Arthronomalus** (Plate XVI. figs. 16, 17, 18).—In this genus the form of the ovary resembles that of *Cryptops*; the eggs, however, are less elongated. The smallest egg-germs were about $\frac{1}{1000}$th of an inch in diameter, and consisted of a macula, which was often double, a Purkinjean vesicle, and *apparently* a vitelline membrane. In all probability, however, no such membrane is present.

It seemed to me also that in *Arthronomalus* the young egg-germs were not enclosed in any separate follicle, though on the larger eggs the usual epithelial layer could be seen. The macula in eggs a little more advanced was, as far as my observations went, never multiple, and resembled therefore in this point *Cryptops* rather than *Lithobius*.

In the eggs of *Arthronomalus* the vitelline vesicle is particularly distinct; it cannot, however, be perceived until about the period when the yelk begins to become opake; at this stage, however, it makes its appearance, and almost always contains a few globules. Indeed, when only a few yelk-globules are present, they generally all lie in the vitelline vesicle. Plate XVI. fig. 17 represents an egg with a Purkinjean vesicle and spot, and at *a* this curious body, which I may perhaps call the vitelline vesicle. I cannot term

* I examined some specimens of *A. longicornis*, and also some which had the antennæ nearer together, with about sixty-eight pairs of legs.

it a cell, first, because I could not satisfy myself that it had a distinct membrane; and secondly, because it is doubtful whether it is homologous with the true vitelligenous cells of insects,—these latter being coeval with the Purkinjean vesicle, while I never saw the vitelligenous vesicle of *Arthronomalus* except in eggs which were already of some size, and in which the yelk was beginning to become opake. We must, I think, seek for its analogue rather in the yelk-nucleus described first by WITTICH in the eggs of spiders.

*Geophilus** (Plate XVI. figs. 22–26).—In the form of the ovary and the first stages in the development of the eggs, this genus much resembles *Arthronomalus*. At the end of November, the long, narrow ovary contained a single series of about thirty or forty nearly mature eggs, which could even be perceived through the skin. Around them were others in earlier stages of development; but there was a considerable interval between the most advanced of these and the large ones, which latter were approximately equal in size, and are probably all laid together.

In the young eggs the Purkinjean vesicle resembled that of *Arthronomalus*, and the macula was single, sometimes apparently homogeneous, sometimes nucleated, or, rather, perhaps vacuolated. In the smallest eggs the Purkinjean vesicle was round, but in others a little more advanced it exhibited one or two prolongations. What, however, struck me as very interesting was that the Purkinjean vesicles in several specimens, in which the eggs were rather larger, were no longer homogeneous, but appeared to consist of two substances, one surrounding the other (Plate XVI. figs. 22–26). The inner portion was generally produced at one or two places; and as the boundary of the outer part was less affected or even quite circular, the inner part passed at these places almost or quite through the outer substance. In some cases the macula also appeared to have undergone subdivision. It appeared to me that portions of the Purkinjean vesicle in this manner gradually separated themselves from the rest. At any rate many of the eggs, at the stage when the yelk was beginning to become dark and granular, contained one, two, or three patches, which were apparently detached portions of the Purkinjean vesicle.

I naturally referred with much interest to the vitelline vesicle of *Arthronomalus*; but in this genus the Purkinjean vesicle is always circular and homogeneous. If, therefore, the homogeneous yelk-masses of *Geophilus*† do originate in the manner indicated, it would appear that they are not homologous with the vitelline vesicle of *Arthronomalus*, which does not seem to be derived from the Purkinjean vesicle. It is, however, very improbable that two genera in other respects so nearly allied, should differ in a point

* I examined *G. acuminatus*, and also some specimens which had about fifty-three pairs of legs, filiform antennæ, and the basilar segment smaller than the sub-basilar. I am inclined to think from these characters that they must belong to *G. brevipes*, of which NEWPORT had only seen a single specimen.

† A somewhat similar though apparently more regular subdivision of the Purkinjean vesicle has been figured by LEUCKART (Unter. z. Nat. des Mens. und d. Thiere, 1858) as occurring in *Aphis*. LEUCKART also mentions a vitelline vesicle as existing in *Geophilus*; but his species probably belonged to the nearly allied genus *Arthronomalus*.

apparently of so much importance; and in all probability, therefore, a true vitelline vesicle will be discovered in *Geophilus*, though it is certainly not so conspicuous as in *Arthronomalus*. Certainly no such vesicle has yet been noticed in *Cryptops*, but it may perhaps have been overlooked in that genus also.

In the largest eggs the yelk completely hid the Purkinjean vesicle, which, however, became evident on the application of pressure. It had become again homogeneous and round, and was about $\frac{1}{300}$th of an inch in diameter. The macula was single, but often vacuolated.

Acetic acid darkened the yelk and destroyed the macula, as usual. The Purkinjean vesicle remained visible by contrast of shade. The full-grown eggs were about $\frac{6}{300}$ths of an inch in diameter; and the yelk consisted principally of small oil-globules, not larger than $\frac{1}{1000}$th of an inch.

ARACHNIDA.

PHALANGIDÆ.

Nemastoma bimaculatum, FAB.—This pretty little species is common in Kent, under stones, logs of wood, &c. When found, it often feigns death; and its horny skin and dark shrivelled appearance may well deceive any unsuspicious observer. It is altogether black, excepting two white patches on the back; and as no other English species is at all like it, it has for the physiologist the great merit of being easily identified.

The general arrangement of the female generative organs is much like that of *Phalangium Opilio*, as described by TREVIRANUS* and TULK†. A female dissected in the middle of September already contained a few mature eggs, but these became much more numerous later in the autumn.

The external membrane of the ovary is, as usual, structureless; but on one or two of the follicles in an allied species, *Phalangium cornutum*, I saw bright spots, arranged at tolerably regular intervals, and raised a little from the general surface. These were probably the last remnants of nuclei, which had disappeared altogether everywhere else. The membrane is in places thrown into numerous follicles. Inside the outer membrane I found no regular layer of epithelial cells, but a number of nuclei apparently imbedded in a homogeneous substance. These nuclei (if I may call them so) are in this species unusually plain, round, or elliptical in shape, from $\frac{1}{300}$th of an inch to $\frac{1}{1000}$th of an inch in length, and with finely granular contents. One of the granules is generally larger than the rest, and thus represents a nucleolus.

It seemed to me that the origin of the egg was as follows. The nucleolus of one of the larger nuclei, lying next to the external membrane of the ovary, increased in size, while the other granules disappeared; and the nucleus having gradually become a Purkinjean vesicle, and passed into a follicle, we have thus all the elements of a young egg-germ. Whether, however, all the ovarian nuclei are thus in turn developed, or whether, as I have sometimes been inclined to think, some of them, after attaining to

* Vermischte Schriften, p. 34.

† Annals and Mag. of Nat. Hist. 1843.

the maximum size, become merely vitelligenous bodies, and not true Purkinjean vesicles, I could not satisfactorily ascertain. If even, however, this is the case, the yelk-forming nuclei do not themselves enter the follicle.

It results from the above description that the ovarian capsules consist of a simple membrane, and are not bounded, as in insects and Myriapods, by any inner layer of epithelial cells. The smallest Purkinjean vesicles observed were about $\frac{1}{1300}$th of an inch in diameter, and had a single solid-looking macula. At this period the Purkinjean vesicle and the yelk are quite clear and transparent; nor are they darkened by the action of dilute acetic acid, though the former is, as usual, destroyed by it.

When, however, the egg-follicle is about $\frac{1}{300}$th of an inch in diameter, the yelk begins to become granular, and in consequence darker in colour. The granules are at first quite small, but they rapidly increase in number; and when the egg is $\frac{3}{300}$ths of an inch in size it has become quite opake.

During all this period the Purkinjean vesicle remained unaltered, except in size. The macula also enlarges, and becomes vacuolated; but I never saw it break up, nor was I able to determine its ultimate destination. In an egg $\frac{3}{300}$ths of an inch in length, the Purkinjean vesicle was as before, the macula single and vacuolated.

No trace of either Purkinjean vesicle or macula could I ever detect in the full-grown egg from the matrix. These eggs were of an elliptic shape, $\frac{55}{300}$ths of an inch in length, by about $\frac{45}{300}$ths in breadth. The yelk consisted, 1st, of minute, round, greenish globules about $\frac{1}{1000}$th of an inch in diameter; 2ndly, of the usual transparent, somewhat viscid substance; 3rdly, of irregularly-shaped, solid-looking yelk-masses, about $\frac{1}{80}$th of an inch in diameter, and each generally appearing to contain a rounded globule in its interior.

The chorion is a single, simple, transparent, structureless membrane.

The other species of Phalangidæ which I have dissected are *Phalangium cornutum*, *Leiobunus rotundus*, and *Opilio agrestis*, all of which, in the arrangement of their generative organs, agreed in all essential particulars with the preceding species.

In *Leiobunus rotundus* the ovarian nuclei were as distinct as in *Nemastoma bimaculatum*, but rather smaller; in *P. cornutum* they are more delicate, and the contents are less granular. The formation of the egg, of the Purkinjean vesicle, and of the macula proceed almost exactly as in the preceding species, and need not therefore be again detailed. In *L. rotundus*, however, the macula has not the appearance of a vesicle, but rather of a cloud of small granules compacted together.

In some cases the ovary presents a rather unusual appearance, from the fact that the stalks of the ovarian follicles are not circular, but elliptic; this peculiarity struck me more particularly in *Leiobunus*.

The eggs of *P. cornutum* begin to darken when they are about $\frac{10}{300}$ths of an inch in diameter; and the yelk then consists of very fine particles, not more than $\frac{1}{3000}$th of an inch in diameter.

In *P. cornutum* the egg had become quite opake when it was about $\frac{35}{300}$ths of an

inch in diameter. At this stage some of the larger yelk-globules were $\frac{1}{1000}$th of an inch in size, but the majority were much smaller. The Purkinjean vesicle was $\frac{1}{1000}$ths of an inch in size, and the macula was, as usual, single; it did not, however, appear to be a vesicle, but rather, as in *Leiobunus*, a cloud of particles, without any true surrounding membrane, and more or less compactly arranged.

The chorion of the egg is no doubt, in all the Phalangidæ, formed by the consolidation of the outer layer of the viscid part of the yelk; and as this process is gradual, it is impossible to say exactly when it begins, or when it is finished. The yelk in the mature eggs of *Leiobunus* and *Phalangium* is constituted like that of *Nemastoma*.

Leuckart says that in *Phalangium*, besides the eggs with a simple, homogeneous macula, there are some in which it is granular, and others in all intermediate stages. I have myself observed the same thing not only in the Phalangidæ, but also in many other Articulata. The difference arises, however, I believe, from the action of the fluid in which the eggs are examined; and the homogeneous specimens represent the normal condition.

The internal male generative organs of *Phalangium* (Plate XVII. fig. 45) have been quite misunderstood, I think, by every one who has written on the subject. Treviranus* described the numerous short white tubes which fall into the vas deferens as "Saamengefässe;" Tulk also says†, "The testes are formed by a cluster of elongated, narrow and slightly tortuous cæcal tubes;" and this view has been generally adopted‡. Each of these tubes contains a narrow central tube, which gives off on each side numerous branchlets; and these branchlets terminate in cluster-like glands. If, therefore, they were simplified and shortened very much, they would resemble in structure the accessory glands of *Chelifer*, which are certainly not testes. It would of course be unsafe to rely much on this comparison; but I never found any trace of spermatozoa in these tubes, which, on the contrary, contained numerous delicate vesicles about $\frac{1}{800}$th of an inch in diameter, and with finely granular contents; the secretion of these tubes, too, is, I believe, a fluid. On the other hand, among these short tubes I always found one much longer than the rest (Plate XVII. fig. 45, *d*), and convoluted instead of nearly straight. Its internal structure also was entirely different, being without any such small branched duct and glands. This I regarded at first as the true testis, especially as at its lower end it contained immense numbers of minute spherical bodies, which are the spermatozoa.

There is, however, another large tube (*a*), which was already known to Treviranus§, and described by him as the Z-shaped tube. As it occurs only in the males, he presumed that it was connected with the secretion of the semen, but he was unable to trace its connexion with the other generative organs. The tube lies across the digestive organs, and

* Vermischte Schriften, p. 36.

† *Loc. cit.* p. 21.

‡ Leuckart, article "Zeugung;" Siebold and Stannius's 'Anatomy;' and Leydig, "Zum feineren Bau der Arthropoden," Müller's Archiv, 1855.

§ Vermischte Schriften, p. 37.

at each end contracts into a fine tubule (*b*), which TREVIRANUS traced a little way among the stomachal cæca and then lost. TULK was scarcely more fortunate. He says of it, "I examined the direction of these minute ducts with great care, and found that they pass forwards and curve round the tracheal trunks, near to their origin, from above downwards, and are lost at the inner extremity of the spiracular groove, where they may probably open externally. The function assigned to this part is thus rendered extremely problematical."

This description much excited my curiosity. A simple tube running transversely across the body, and opening at each side, would be an organ entirely without a parallel among the Articulata, or, so far as I know, in the whole animal kingdom. Moreover, after a little consideration I could not help thinking that this must be the testis, not only because it occurred only in the males, but also because its situation was much like that of the ovary, and because its contents much resembled immature spermatozoa.

The contents of the abdomen are, however, so intricate, and the fine continuations of the tube are so closely attached in places to the tracheæ, and so delicate in themselves, that only after several failures did I succeed, in *Ph. urnigerum* and *Leiobunus rotundus*, in tracing what I may call the vasa deferentia of the testis beyond the tracheal trunks (*c*). At length, however, I succeeded in doing so, and found that they turned round again and passed with many convolutions to the central line of the body, where they fell into the delicate end of the common ductus ejaculatorius (*d*). Thus, therefore, all doubt as to the function of the Z-shaped tube is removed, and we see that the male and female organs of *Phalangium* offer the same parts, and are formed on exactly the same type. In both of them the secretory part, the testis in one sex and the ovary in the female, is in the form of a ring the posterior part of which is much wider than the anterior, while from the centre of the anterior half proceeds a tube which is rather short and very wide in the gravid female, while it is narrower and longer in the male.

Mr. NEWPORT, in his paper on *Iulus*, expresses his admiration at the remarkable similarity existing between the generative organs of the two sexes in that genus. In this case, however, the simplicity is so great that the similarity is much less striking than in *Phalangium*, where both organs are of a most unusual form.

The spermatozoa, as they are found in the ductus ejaculatorius, are minute spherical bodies, about $\frac{1}{4000}$th of an inch in diameter, of a greenish hue, and containing a brightly refractive, rod-like body. Whether, however, this is their definite shape I am unable to say, not having noticed them in the female. LEUCKART, in his justly celebrated article "Zeugung," has given a nearly similar description of these bodies. LEYDIG*, however, has obtained them from the vas deferens, and concludes, rather from noticing that they possess a tremulous motion than from any actual observation, that they are provided with fine cilia. In *P. urnigerum* the accessory tubes were quite short, and much branched.

* *Loc. cit.* p. 469.

In *Opilio agrestis* and *Nemastoma bimaculatum* the male generative organs were formed nearly as in *Phalangium*, but I have not actually traced the connexion between the testis and the ductus ejaculatorius. In *O. agrestis*, however, I traced the latter up to its bifurcation, and the vasa deferentia as far as the great tracheal trunks; so that no doubt in all the Phalangidæ these parts are formed on the same type.

Scorpionidæ.

Chelifer (Plates XVI. and. XVII. figs. 27–36).—Although the testis of *Chelifer* closely resembles that of the Scorpion, the ovary of the two genera is very different. Instead of the net-like complex organ of the Scorpion, we find in the smaller Chelifers only a simple, tubular organ, extending backwards from the vulva and bearing from thirty to fifty eggs in one stage of development, enclosed in spherical follicles on short stalks, and arranged in a row on each side with more or less regularity.

Between these eggs lie others which are not so far advanced, but which, when the first have been laid, will in their turn attain to maturity. Besides these two series of eggs, I generally found other small follicles, with irregular yellowish contents; these are probably follicles from which the eggs have escaped, and their yellowish contents are "corpora lutea," homologous with the yellowish contents so often found in insects at the lower end of the egg-tubes, and which have been compared by Stein to the "corpora lutea" of the higher animals.

I am not able to throw much light on the early stages of egg-formation; but it seemed evident that in *Chelifer* the egg-follicle was formed by one single simple membrane, the epithelial cells occupying the stalk only. These were so delicate, that without the action of reagents they could scarcely be perceived; but if water, either pure or with a trace of ammonia, be used instead of syrup, they become more evident. Although, however, the follicle is not lined by any epithelium, occasionally one of the cells could be detected in it.

The epithelial cells varied from $\frac{3}{8000}$ths of an inch to $\frac{6}{8000}$ths of an inch in diameter, and had a distinct though pale nucleus. Among them I found other solid-looking bodies, about $\frac{3}{8000}$ths of an inch in diameter. I never succeeded in making out satisfactorily the origin of the Purkinjean vesicle; but, from analogy with what is stated to be the case in Spiders, and with what occurs in other animals, it seems probable that one of the epithelial nuclei increases in size, developes the yelk round itself, and causes the external membrane to bend outwards. The macula appears to be originally simple; it is always, however, a difficult subject for investigation.

In the early stages the yelk possesses a definite outline, but, as the action of a drop of ammonia shows, this does not arise from the presence of any true membrane. It is at first clear and transparent; but when it has attained a size of $\frac{1}{800}$th of an inch, yelk-globules begin to appear in it; and these become more and more numerous, until, when the egg has attained about $\frac{1}{300}$th of an inch, the greater part of it is occupied by them, and the Purkinjean vesicle completely hidden. They are generally somewhat collected

round the centre, so that the margin is still composed of the clear yelk-substance; and their refrangibility is great, so that they give the egg a peculiar and beautiful appearance.

They do not continue to enlarge as they increase in number; but even when only two or three are present, they are often as much as $\frac{1}{1000}$th of an inch in diameter—a size which is seldom exceeded even in full-grown eggs.

The females are provided with a sac, which I think myself justified in calling a spermatheca. It opens close to the vulva, is slightly narrowed at the outlet, cylindrical in form, and more or less bent at the end, which is double. I always found it full of the greenish ovoid spermatozoa. They exactly resembled those found in the testis and ductus ejaculatorius of the male, but sometimes seemed to have developed round themselves a cellular envelope. I never found in this organ any filiform spermatozoa.

Weak acetic acid dissolved the Purkinjean vesicle and macula as usual, but it had no effect on the yelk-globules, nor the intermediate clear yelk-substance. Very dilute *ammonia* also dissolved the macula, and, as above mentioned, at first made the epithelial cells more distinct. It did not affect the yelk-globules, nor darken the yelk-substance, but apparently it caused the latter to swell, since, although the wall of the follicle did not seem to shrink, the contents were more or less completely ejected from it. It had no effect on the yellowish "corpora lutea."

I began to examine *Chelifer* in the month of August; and of the first few specimens collected nearly one-half had eggs or young ones attached to them*. The eggs were seventeen or eighteen in number, and were enclosed in a sort of case, in shape somewhat resembling a D (Plate XVI. fig. 29). Each egg had a more or less separate compartment; and they were arranged in one plane, five being generally in the middle, and the remainder surrounding them in a single series, the straight border containing four or five. The case lay at the lower side of the abdomen, with the straight margin in front, and was attached to the body of the animal at or close to the vulva. The case itself consists of a transparent structureless substance.

I met with five specimens in this condition, but, being busy at the time, and expecting to find others in the autumn, I unfortunately made only a few rough notes. The eggs were about $\frac{1}{1000}$ths of an inch in diameter, and of various shapes. They possessed a firm chorion. They underwent a regular yelk-segmentation, but the yelk-spheres by no means filled the egg. The eggs in each case were in approximately the same stage of development. The most advanced contained eight yelk-spheres† (fig. 28), and also some large, clear, transparent vesicles.

The yelk-spheres occupied about two-thirds of the egg, and appeared to be composed of oil-globules, loosely connected together, and each about $\frac{3}{1000}$ths of an inch in dia-

* De Theis also found a female of *C. cancroides* with eggs attached to the under side of the abdomen. (Ann. Sc. Nat. 1st ser. vol. xxvii.)

† This was the case with all the eggs in three of my specimens, and it is remarkable that Grube never found more than eight spheres of segmentation in the eggs of *Clepsine*.

meter. I only met with one specimen carrying young. The latter were fully developed, and of the mature form, but still incapable of motion. They were much larger than the eggs.

On dissecting these five specimens I was astonished to find in them no trace of an ovary, nor of a testis, but instead, and in the same position, I found a large organ (Plate XVII. fig. 30) consisting of thirty short cylindrical cæca with parallel walls. In front the organ passes into two large bags; and from the anterior end of each of these, rises a short duct. I did not trace these ducts to their extremity; but they probably open near to one another. This seemed to happen close to, if not at the place, where the vulva occurs in ordinary specimens. The organ contains oil-like vesicles, which vary in size up to $\frac{1}{100}$ths of an inch; the usual size, however, is from $\frac{3}{800}$ths to $\frac{3}{800}$ths of an inch, and the larger ones contain daughter vesicles. It would appear that this remarkable organ must secrete nourishment for the embryos, in which way we may account for their great increase in size before leaving the mother. I have not, indeed, evidence sufficient to prove this; but it would not be altogether without analogy, since LEUCKART has discovered in the viviparous Diptera a branched organ which secretes a substance to serve as food for the young during its stay in the uterus.

Whereas, however, this branched organ is present in all the females of the viviparous Diptera, I have never found the sacculated organ of *Chelifer* except in the egg-bearing specimens, although in the months of September, October, and November I dissected a great many females, and particularly sought to find the structure in question.

All the specimens examined by me were found within a few feet of one another, under some planks which were lying on a hot-bed in our kitchen-garden, and I noticed no external difference between the egg-bearing specimens with the sacculated organ and the ordinary males and females. Unfortunately, however, my attention was not at first directed to this point; and when after only a fortnight's interval I returned to my Chelifers, no more case-bearing specimens were to be found, though in the last fortnight of September I looked over more than a hundred specimens in hopes of finding some. Moreover, the females examined during this time and up to the beginning of November, all contained eggs developing in the ovary, as above described. It is also worthy of notice that, whereas the ovary always contained from thirty-five to forty eggs in one stage of development, the number of eggs in each egg-case was only seventeen or eighteen, as before mentioned.

It may naturally be asked to what organ in the ordinary females this sacculated structure corresponds; and to this question I can give no satisfactory answer. It seems most probable, however, that it is a modification of the ovary. The form and position of these two organs is very similar; and there are two ducts, as in the testis; moreover, the egg-follicles of the one are somewhat similar in size and shape, and not very different in number, from the cæca of the sacculated organ. In one instance also I found the ovarian follicles with numerous small greenish oil-globules, while in the regular course of egg-development they contain large, dark, and very bright ones. In this state the ovary

much reminded me of the sacculated organ, and had all the appearance of being in an intermediate condition.

I am only too well aware that my observations have not yet gone far enough to justify any definite conclusions, but they seem to indicate that the eggs of *Chelifer** are in summer carried about by the animal, that the young ones in this position grow to some size, being nourished by a milky fluid secreted for that purpose by a special organ, but that, when winter comes on, the eggs are laid by the mother in some secure place, and perhaps do not hatch until the warm days of spring. Furthermore, the fact that each female *Chelifer* produces thirty-five to forty eggs in a brood, while the egg-bearing specimens have only seventeen or eighteen eggs attached to them, and that while these specimens, which I may perhaps be permitted to call "nurses" (though in a natural, and not a Steenstruppian sense), have the absent or at least rudimentary ovary† or testis, the ordinary specimens do not possess the peculiar sacculated "milk-gland," would seem to indicate (though I dare not do more than suggest the possibility) that in *Chelifer*, as in so many Hymenoptera, we have, besides the males and females, certain so-called neuter specimens which are (probably in this case also) females with imperfect generative organs, and whose function it is less to lay eggs themselves, than to feed and tend the young ones produced by the perfect females.

In September the males of *Chelifer* seem to be about as numerous as the females; out of the last sixteen specimens which I examined, eight were males, and eight were females.

The generative organs open at the anterior end of the abdomen. At the orifice is a peculiar chitinous body, broader at both ends than in the middle, and provided with strong muscles.

On one side of this body are two large vesicular organs (Plate XVII. fig. 36), each in the form of a sphere, with a deep transverse medial constriction. The free half is pale, delicate, and apparently empty; the other has a thicker wall; it contains about twenty-five straight, narrow canals of unequal length, and at the free end of each is a crown or flower of glandular bodies, which probably pour their secretion into the straight canal.

The testis (Plate XVII. fig. 31, *a*), which probably lies between the sternum and the digestive organs, is single, and consists of a median and two lateral tubes, united by three transverse branches. It represents, therefore, in miniature that of the Scorpion, differing from it, however, in that the two testes have coalesced along the middle line. This was, at least, the structure of the testis in six males which I dissected; in the first one, however, I thought I found four transverse branches; but as no second instance of this presented itself, the drawing I made at the time may be incorrect.

The vas deferens (*b*) is double, and about as long as the testis. I did not ascertain the exact relation which they bear to the orifice; but at the base of the above-mentioned chitinous body each of them forms a spherical swelling, and on leaving the testis one of them always formed a second elliptic sac. In most of the cases examined by me this

* The egg-bearing specimen noticed by De Theis was found in June.

† Or perhaps only a rudiment of one.

chamber occurred only in one of the tubes, and the other one was of nearly equal diameter from the testis to the anterior spherical chamber. In one, however, two sides were symmetrical, as I have represented in Plate XVII. fig. 31, *b*.

The spermatozoa are apparently produced indiscriminately in all parts of the testis. They are of an oval form, and are found in rounded masses (Plate XVII. fig. 32). I also found in some specimens cells containing filiform spermatozoa; but later in the autumn only the oval bodies were present. They are perhaps immature forms; but against this view must be set the fact that the spermatheca of the female contained these bodies only. It is, however, very unlikely that they should be altogether different from the spermatozoa of the so nearly allied genus *Obisium*.

The *tracheæ* open through orifices at the sides of the second and third abdominal segments; the spiracles lead into a short thick tube, from the end of which arise a great number of long thin tracheæ, which pass, without giving off many branches, to the different internal organs.

Obisium.—I found, under a piece of wood, in September and October last, five specimens of *Obisium muscorum*, only one of which was a female. It was caught on the 16th of October, and had no case of eggs, but the ovary contained eggs in course of development; and much resembled that of *Chelifer*. The egg-follicles contained sufficient oil-globules to hide the Purkinjean vesicle, though it became visible on the application of pressure. The oil-globules were rather even in size, and much smaller than in *Chelifer*, not exceeding $\frac{1}{1000}$th of an inch in diameter.

The testis resembles in form that of *Chelifer*; but the spermatozoa are very dissimilar. Scattered about in the testis were rounded masses of small cells, which gradually modified themselves into spermatozoa. These masses probably arose by the endogenous multiplication of small cells within a larger one. The rounded masses were of different shapes and sizes, and the development of the spermatozoa seemed to be quite independent of the size of the mass. The mature spermatozoa had a bright cylindrical head, about $\frac{1}{1000}$th of an inch in length and very narrow, and a very delicate, scarcely perceptible tail (Plate XVII. figs. 33 *a*, 35). They were quite motionless. The testis also contained some oval bodies, resembling in shape those found in *Chelifer*; their wall was not, however, so distinct, and I did not satisfy myself that they were not merely detached specimens of the usual small cells. In one specimen I found a number of vesicles of different sizes, and containing small rod-like bodies (Plate XVII. fig. 34). It is possible, however, that in this case I may have had before me some nearly allied species. The complemental glands are lobulated, and not spherical as in *Chelifer*. Each of the tubules, also, instead of ending in a crown of glands, terminates in a single, dark, club- or egg-shaped mass.

Of *O. orthodactylum* I found eleven specimens on the under side of the log of wood which supplied me also with *O. muscorum*. Six of them were males; the form of the testis was like that of the preceding species, but I am not able to speak positively as to the spermatozoa. The development of the eggs appeared to be much like that in

O. muscorum, and the oil-globules were small. None of the females bore egg-cases; but it was perhaps too late in the season.

THYSANOURA.

Petrobius maritimus.—In this interesting animal each of the ovaries consists of a tube running along the side of the abdomen, and giving off, on its inner side, seven short egg-tubes, which lie above the intestine. These latter, therefore, are fourteen in number; and in the beginning of September, when I examined them, each tube generally contained towards its lower end three egg-germs, in which a considerable deposition of yelk had taken place, and towards its free extremity from fifteen to twenty egg-germs in earlier stages of formation.

The egg-tube is lined with epithelial cells, generally from $\frac{1}{700}$th to $\frac{1}{1000}$th of an inch in diameter. Their nuclei are about $\frac{1}{1500}$th of an inch in diameter, and very faint. Often, indeed, they can scarcely be perceived; but, generally, when the tube had been lying some time in syrup they became tolerably plain. At the free end of the egg-tube are some solid-looking nuclei, about as large as the nuclei of the epithelial cells, and only differing from them in being more distinct, and possessing granular contents.

These nuclei are generally all about the same size; sometimes, however, one or two are larger than usual; and as this was the case in the first specimen I examined, I was inclined to believe that the nuclei increased in size, and thus became the Purkinjean vesicles. As I was not able in other specimens, however, to find any nuclei in the process of becoming Purkinjean vesicles, this view requires confirmation, though it is supported by the analogy of other animals.

Although in an unaltered condition the epithelial cells of the egg-tube are very faint, and often altogether invisible, yet if pure water be added and the syrup be removed, the cell-walls and the epithelial nuclei gradually become quite plain. Most of the cells are, from the apposition of their neighbours, irregular and somewhat angular in shape; here and there, however, we see one quite round, and these can scarcely be distinguished from the youngest Purkinjean vesicles. In the latter, however, the nucleus looks rather more solid. The smallest Purkinjean vesicle which I saw was $\frac{6}{1000}$ths of an inch in diameter.

The yelk of the young eggs appears to possess no vitelline membrane; nor, though the boundary of it is perfectly distinct, has it any definite shape, but, apparently in consequence of the pressure put upon it by its neighbours, the outline which it assumes is very variable. As, however, it continually increases in size, it gradually comes to occupy the whole width of the egg-tube, and then assumes generally a more or less wedge-like shape, the Purkinjean vesicle occupying the thicker end. There are usually three or four egg-germs in this stage (Plate XVII. fig. 37).

The two or three most advanced egg-germs approximated more or less to the form of the mature egg, and were darkened by the deposition of granules and small oil-globules.

Below the eggs was a yellow matter, corresponding apparently to the so-called corpora lutea found in the egg-tubes of insects.

The mature egg is elongated fusiform, about $\frac{8}{100}$ths of an inch in length, and enclosed in a tough, somewhat transparent chorion.

The Purkinjean vesicle, which in the smallest egg-germs was sometimes even less than $\frac{1}{1000}$th of an inch, increases to as much as $\frac{1}{10}$th of an inch in diameter. In the meanwhile the macula has undergone important changes.

I have already mentioned that on its first appearance it is a single, apparently solid body; but even in the smallest egg-germs the Purkinjean vesicle contains very often, besides the macula, a small vesicle, which increases in size with the macula, but otherwise undergoes no alteration (Plate XVII. fig. 40). In many cases, however, I could not see this secondary macula.

The macula itself soon appears to develope in its interior a clear space (Plate XVII. figs. 40, 41), which is apparently bounded by a membrane, since after a time it works its way to the surface of the macula, and forms a projection, and, indeed, sometimes appears to detach itself altogether from the macula. It is always quite clear and transparent, while the macula itself is turbid, though at this stage it again contains a clear space in its interior. I examined the Purkinjean vesicles of six full-grown eggs, but was unable to satisfy myself as to the normal state of their contents at this stage. All of them contained the large macula, which in some of them had the form of a hollow cap. Two of them had a second clear macula, about half as large as the first (Plate XVII. fig. 40); and one contained a number of small vesicles. These changes may be compared with what takes place in *Geophilus*.

The yelk consisted, as usual, of a viscid substance, containing fine granules and oil-globules, varying up to $\frac{1}{1000}$th of an inch in diameter. Acetic acid acted in the usual manner on these tissues, and dissolved all the granules contained in the free nuclei (which I supposed to be embryonic Purkinjean vesicles), just as it does the true macula.

Dilute ammonia also dissolves the macula and the granules of the free nuclei.

The spermatozoa have a minute pear-shaped head and a long tail. Taken from the testis of the male, they exhibit a wriggling movement.

General Remarks.

It appears that in the Annulosa, as in the other divisions of the animal kingdom, the Purkinjean vesicle is the first-formed part of the egg, and that the yelk and vitelline membrane are subsequently deposited round them. This holds good (according, at least, to the various naturalists who have written on the subject) in most insects, in Crustacea, Spiders, *Lacinularia* and other Rotatoria, in *Hermella*, in *Oxyuris*, *Ascaris*, and the Nematoidea generally. Dr. Allen Thomson, indeed*, extends this to the whole animal kingdom. "The germinal vesicle," he says, "is universally the first part of the ovum which makes its appearance; it does not appear to be nucleated or to possess its macula

* Article "Ovum," p. 133.

from the first in all instances, and this macula cannot, therefore, be regarded as the centre of its formation." Other naturalists, however, have given a very different account of the process of egg-development; and even Dr. THOMSON himself, in that part of the same article which refers to the Acalephæ, does not figure or describe the Purkinjean vesicle as appearing until the second stage. GEGENBAUR also gives a very similar account of what takes place in *Thaumantias*. He says, "One sees, moreover, often even in one and the same animal, that some of the cells filling the ovary increase in size, the membrane raises itself more considerably from the nucleus, and at this time molecules, generally arranging themselves round the nucleus, begin to differentiate themselves in the originally homogeneous cell-contents. Only two or three cells of the primitive ovarian parenchyme pass through these changes, and thus become egg-germs; their growth proceeds further and further, and the contents of the egg-cell now consist of a finely granular substance, in the centre of which a transparent nucleus (the germinal vesicle) lies imbedded." Among the Mollusca, according to a very accurate observer, M. LACAZE DUTHIERS, the egg of *Dentalium* arises from a modified epithelial cell, the nucleus of which becomes the Purkinjean vesicle. These instances, however, are foreign to our immediate subject; but even among the Annulosa similar observations are upon record. In describing the ovary of *Argulus*, LEYDIG* says, "The smallest eggs are clear round cells, whose vesicular nucleus contains many nucleoli. They alter themselves gradually into eggs, and pass slowly from a circular to an oval shape, &c." Again, in *Limulus*, GEGENBAUR† expressly describes and figures the egg as arising from the modification of a single epithelial cell, the nucleus of which becomes the germinal vesicle‡; and in *Cypris* the egg has been described as having a similar origin. In his paper on *Argas persicus*§, Dr. C. HELLER says, "In its original form the egg appears as a colourless cell, with a transparent vesicular nucleus and finely granular contents. In more advanced eggs the finely granular mass is in greater quantity, and of a yellowish colour; an evident germinal vesicle is now present, in which the round germinal spot is clearly visible." Finally, if MEISSNER is correct, the membrane of the original cell becomes the vitelline membrane also in *Mermis* and *Gordius*, and in some of the Nematoid worms.

If these observations, or any of them, are correct, it is evident that we have in the Annulosa, as even in the animal kingdom generally, two essentially distinct types of egg-development, since the original epithelial, or at least ovarian cell, which becomes the whole egg in *Argulus*, *Limulus*, *Argas*, *Mermis*, &c., forms in other cases only the Purkinjean vesicle; so that, taking the undifferentiated ovarian cells as our starting-point and standard of reference, the Purkinjean vesicle in certain animals corresponds to the whole egg in others. In other words, we can no longer regard the "eggs" of all Annulose animals as being homologous with one another, but we must consider that, as

* SIEBOLD and KÖLLIKER's Zeits. f. Wiss. Zool. 1850, p. iv.

† Anatomische Unters. eines *Limulus*, mit besonderer Berücksichtigung der Gewebe. Halle, 1858.

‡ *Loc. cit.* pl. 1. fig. 3.

§ Aus dem XXX. Bande des Jahr. 1858 der Sitz. der Math.-naturw. Cl. der Kais. Ak. Wien.

regards their origin and mode of development, the eggs of some (*Argulus*, *Limulus*, &c.) are homologous with the Purkinjean vesicles of others (*Oniscus*, *Ascaris*, &c.).

It is easy to convince oneself that the egg cannot be considered as a modified ovarian cell, at any rate in certain cases; and a single inspection of a female *Glomeris*, for instance, will leave no doubt upon this point. On the other hand, I was myself at first inclined to believe that the second process did occur in some animals. Thus in *Polydesmus complanatus* it seems at first sight evident that each of the minutest eggs is constituted by a cell whose nucleus forms the Purkinjean vesicle, and whose cell-wall gradually becomes the vitelline membrane. The same appearance is also represented by the young eggs of *Cryptops*, *Arthronomalus*, *Geophilus*, &c., and is sometimes seen also in those of *Iulus*; but in this latter genus it is easy to see, especially if pure water is used, that many, if not all, of the young eggs have in reality no vitelline membrane. In all the Chilopods I have, however, found scarcely a case in which the Purkinjean vesicle is not already surrounded by a spherical mass of clear yelk with a perfectly distinct border; in a few cases, indeed, I believe myself to have done so, and in others, in which a membrane is apparently present, it is easy to convince oneself that this is an illusion, since if a portion is cut off, the new surface equally appears to possess a distinct membrane.

On the whole, therefore, it seems probable that in *Argulus*, *Mermis*, &c., the youngest eggs yet observed may not have represented the earliest stage, but may have at an earlier period consisted only of the Purkinjean vesicle, and that the sharp edge of the yelk may have had the appearance of a true membrane. That no free Purkinjean vesicles were found with those which were already surrounded by yelk, may perhaps have depended on the state of the animals when they were discovered. Neither Wagner nor Stein found in the Neuroptera, Orthoptera, and Lepidoptera any Purkinjean vesicles which were not already surrounded by yelk; but Professor H. Meyer, who examined certain Lepidoptera in an earlier state, that is to say while still larvæ*, figures and describes certain cellular elements of the ovary, which subsequently become Purkinjean vesicles and surround themselves with yelk.

The same explanation cannot as yet be applied to *Limulus*; but the figure given† closely resembles the appearance presented by the eggs of Spiders, in which, however, according to Wittich, V. Carus, and Leuckart, the original cell forms the Purkinjean vesicle, round which the yelk is subsequently deposited. We may therefore fairly wait for a confirmation of Gegenbaur's observations before we unhesitatingly adopt the explanation which he offers of the mutual relations of the egg and the epithelial cells.

If, however, we may admit that no essential difference has as yet been proved to exist in the eggs of Annulosa, so far as regards the relations existing between the Purkinjean vesicle and the original ovarian cell, it would still seem that in the relations between the former and the yelk two very different types of development must be recognized.

* Zeits. f. Wiss. Zool. 1849, vol. i. p. 191.

† *Loc. cit.* fig. 9.

In describing the so-called "winter-ova" of *Lacinularia socialis*, Professor Huxley says*, "It will be observed that all these authors consider the winter-ova, or ephippial ova, and the ordinary ova to be essentially identical, only that the former have an outer case. The truth is that they are essentially different structures. The true ova are single cells which have undergone a special development. The ephippial ova are aggregations of cells (in fact, larger or smaller portions, sometimes the whole of the ovary) which become enveloped in a shell, and simulate true ova." This aggregation of several cells (one of them putting on the appearance and fulfilling the functions of a Purkinjean vesicle, and the whole becoming enveloped in a shell) is, however, the ordinary and only method of egg-development in many animals. In the Trematode and Cestoid worms, and the greater number of the *Turbellaria*, the yelk and the Purkinjean vesicle are formed in two separate organs. In *Piscicola*, according to Leydig, the mature egg contains, besides the Purkinjean vesicle and the ordinary yelk, a number of nucleated cells†.

In the Mites and Spiders, in *Chelifer*, *Obisium*, the Phalangidæ, and, so far as I know, all the Arachnida, the egg is the product of a single cell.

On the other hand, we find that complex eggs alone are present in vast numbers of insects, namely, in all the Lepidoptera, Diptera, Neuroptera (excluding the Libellulidæ and allied genera), Hymenoptera, Hemiptera, Homoptera, and Coleoptera. We are as yet ignorant of the mode of egg-development in the Thripsidæ and the Strepsiptera; nor does it seem quite clear whether the development of the pseudovum in *Aphis* can be referred to the complex type. It would appear, however, from the statements of Huxley, Leydig, and Leuckart, that in the opinion of these three eminent naturalists the pseudovum is a derivative of a single ovarian cell, and differs therefore in this respect from the ovum of the impregnated female.

We know little as yet about the early stages of egg-formation in the Crustacea; but it would appear that the simple mode prevails generally throughout this class, with the exception of the Daphnidæ.

As regards the Rotatoria, the so-called winter-eggs have been observed in *Hydatina*, *Brachionus*, and *Notommata*, as well as in *Lacinularia*; and we may probably conclude that in these and other allied genera the development of these eggs is on the same type, while "summer-eggs," again, are formed from one cell.

Among the Myriapoda the eggs of *Lithobius*, *Cryptops*, *Geophilus*, *Arthronomalus*, *Polydesmus*, and *Iulus* are probably simple. At least I am disposed to think that the vitelline vesicle is homologous with the yelk-nucleus of spiders; but I have not yet been able to ascertain this point satisfactorily.

Glomeris, however, offers apparently an exception to the rule so general among the Myriapods, as the large rounded bodies present in the egg-capsule (Plate XVI. figs. 1, 2) are probably homologous with the vitelligenous cells of insects.

* Microscopical Journal, vol. i. p. 14.

† Zeit. f. Wiss. Zool. 1849, pt. 1. pl. 10. fig. 56.

In excluding the ephippial ova from the category of true eggs, Professor HUXLEY was influenced to a certain extent by the supposition that they are fertile without impregnation, and are therefore "not ova at all in the proper sense, but peculiar buds." According to STEIN, however, the reverse is probably the case, and the summer-eggs are agamic, while the winter-eggs require to be fertilized*. However this may be, the development of the eggs of insects sufficiently proves that eggs composed of several ovarian cells, like those which are unicellular, generally are incapable of development without impregnation. But no one can deny the name of true eggs to the ova of Butterflies, &c.; and we cannot, therefore, class as "false eggs" those which arise from more than one cell. Perhaps it would be better to distinguish the two classes as "compound" and "simple" or "unicellular." The names we may adopt are, however, of less importance than the establishment of the fact that throughout the Annulosa there are two sorts of eggs, which are of an essentially different structure, and cannot, therefore, strictly speaking, be regarded as homologous with one another.

It is also worthy of notice that among the Articulata a few species possess two sorts of eggs. The cases are indeed few; but as they are also far between (*Lacinularia*, &c. among the Rotatoria, *Daphnia*, &c. among the Crustacea, and *Aphis*, *Coccus*, &c. among the Insects), we may perhaps see in them the last vestiges of a state of things which at a former period may have been general, or at least more common. It is true that the existence of two sorts of eggs is generally supposed to be connected with the presence of Agamogenesis; but this mode of generation may perhaps have had the effect of retaining a previously existing condition, rather than of originating a new and peculiar state of things. The cases of the Bee and of some Lepidoptera prove that a double method of egg-development is no necessary condition of Agamogenesis.

Passing on to the other sex, I am not competent to offer any opinion as to the relations of the male and female elements to one another, or the homologies existing between the product of the male, the semen, on the one hand, and the egg or any part of it on the other; but it is remarkable that, as we find (if I am correct in the view now advanced) in the Annulosa eggs of two different sorts, so also there are traces of a similar bimorphism of the semen. In *Notommata Sieboldii*, according to LEYDIG, the spermatozoa are of two sorts; ZENKER has made the same observation with reference to *Asellus aquaticus*†, in which animal I have also convinced myself of this curious fact; and among Mollusca there is the well-known case of *Paludina vivipara*.

STEIN‡ includes also in this category the Common Woodlouse, since while the three terminal tubules produce only hair-shaped spermatozoa, the matrix, or receptacle into

* This is also in accordance with the case of *Daphnia*. In this genus, as in Rotatoria, the "summer-eggs" are agamic, but it has not yet been conclusively proved that the "winter-eggs" of either require impregnation.

† It would appear that (see VAN BENEDEN, Recherches sur la Faune littorale de Belgique) the same is the case with the allied genus *Slabberina*.

‡ MÜLLER'S Archiv, 1842.

which the tubules open, contains also large nucleated cells. I am bound, however, to admit that I much doubt whether these nucleated cells do really perform any part in the act of fertilization, since, though I know them well by sight, and though I have over and over again found the hair-shaped spermatozoa in the generative organs of the female, I have never met with any of the round cellular bodies in this situation.

In *Lithobius*, also, and *Geophilus*, Stein* discovered, besides ordinary hair-formed spermatozoa, large cellular bodies, which he believes to be actively concerned in impregnation. But in the spermatic sacs of the female of *Lithobius pilicornis*, I found only the long hair-like sort; and it would appear, from what Fabre says†, that in the extraordinary silken nests discovered by him, and in which, reversing the usual order of things, the male lays its semen, that excellent observer found only the capillary spermatozoa.

The spermatophores of *Cryptops*, however, contain, besides the filiform spermatozoa, numerous oval bodies much resembling the spermatozoa of *Glomeris*; and though these may possibly have some function like that which has been attributed to the second sort of cells found in the spermatophores of the Calanoid Entomostraca, it has not yet been proved in either case that they are not homologous with spermatozoa. I may refer to what I have already said respecting *Chelifer* and *Obisium*‡, as tending to show that in these genera also a similar state of things exists.

I believe there are no other animals among the Annulosa in which two sorts of spermatozoa are at present known to occur; but it is evident that there may be many in which the difference, being slight, has not been observed; without, however, pushing this argument too far, it may fairly be doubted whether we are justified in assuming that the hair-shaped spermatozoa of Isopods, Insects, and Chilognaths are strictly homologous with the more or less spherical spermatozoa of most Crustacea and of the Chilopods; and whether it would not be more correct to correlate the hair-shaped forms with the similarly-shaped spermatozoa of *Lithobius*, *Asellus*, &c., and the cellular types with the less-elongated bodies found in the male generative organs of these interesting genera.

When two sets of spermatozoa are present, it is not unreasonable to suppose that their functions are different, and it would be of the highest interest to ascertain wherein this difference consists. The question has often occurred to me whether the two sorts of spermatozoa produce embryos of different sexes. It seems to be satisfactorily ascertained that the sex of the Hive-bee depends on the impregnation of the egg, though, on the other hand, no such connexion appears to exist in *Psyche*. In the Hive-bee the unimpregnated egg produces only a male embryo. But if in any species the reverse were the case, and the unimpregnated egg always produced females, and if, further, at any one time it so happened that no males nor any impregnated females were in existence, it is evident that the male sex would be extinct for ever. Such cases we have perhaps in the species of the genus *Cynips*, although in spite of the great amount of negative evidence, it is difficult not to believe that the males do really exist in this genus, and will sooner or later be discovered.

* *Loc. cit.* p. 252. † *Loc. cit.* p. 305.

‡ On a future occasion I shall attempt to show that the same is the case with the Sminthuridæ.

Explanation of the Plates.

PLATE XVI.

Fig. 1. Egg-follicle of *Glomeris marginata*. ×250.
Fig. 2. Another of *Glomeris marginata*. ×250, under the influence of dilute acetic acid.
Fig. 3. Purkinjean vesicle of *Glomeris marginata*. ×250.
Fig. 4. Young egg of *Polydesmus complanatus*. ×250.
Fig. 5. Young egg of *Polydesmus complanatus*, more advanced. ×250.
Fig. 6. Young egg of *Polydesmus complanatus*, more advanced. ×250.
Fig. 7. Young egg of *Polydesmus complanatus*, more advanced. ×250.
Fig. 8. Two very young Purkinjean vesicles of *Lithobius*. ×250.
Fig. 9. Young egg of Purkinjean vesicles of *Lithobius*. ×250.
Fig. 10. Young egg of Purkinjean vesicles of *Lithobius*, more advanced. ×250.
Fig. 11. Young egg of Purkinjean vesicles of *Lithobius*, more advanced. ×250.
Fig. 12. Young egg of Purkinjean vesicles of *Lithobius*. ×250.
Fig. 13. Young egg of Purkinjean vesicles of *Lithobius*. ×250.
Fig. 14. Epithelial nuclei on the general surface of the ovary. ×250.
Fig. 15. Epithelial cells on the egg-follicles. ×250.
Fig. 16. Young egg of *Arthronomalus*. ×250.
Fig. 17. Young egg of *Arthronomalus*, more advanced. ×60.
Fig. 18. So-called "yelk-nucleus" of *Arthronomalus*. ×250.
Fig. 19. Young egg of *Cryptops*. ×250.
Fig. 20. Young egg of *Cryptops*, more advanced. ×250.
Fig. 21. Young egg of *Cryptops*, with two Purkinjean vesicles. ×250.
Fig. 22. Young egg of *Geophilus*. ×250.
Fig. 23. Young egg of *Geophilus*. ×250.
Fig. 24. Young egg of *Geophilus*. ×250.
Fig. 25. Purkinjean vesicle of *Geophilus*. ×250.
Fig. 26. Purkinjean vesicle of *Geophilus*. ×250.
Fig. 27. Two egg-follicles of *Chelifer*. ×250.
Fig. 28. Egg of *Chelifer* undergoing segmentation. ×250.
Fig. 29. Egg-capsule of *Chelifer*. ×60.

PLATE XVII.

Fig. 30. Secretory organ of *Chelifer*. ×30.
Fig. 31. Testis of *Chelifer*. ×30.
Fig. 32. Mass of spermatozoa of *Chelifer*. ×250.
Fig. 33. Contents of testis. ×250.
Fig. 34. Contents of testis of *Obisium*. ×250.
Fig. 35. Contents of testis of *Chelifer*. ×250.
Fig. 36. Secretory organs of male *Chelifer*. ×60.
Fig. 37. Two egg-tubes of *Petrobius maritimus*. ×30.
Fig. 38. Nuclei from free end of *Petrobius maritimus*. ×250.
Fig. 39. Young egg of *Petrobius maritimus*. ×250.
Fig. 40. Purkinjean vesicle, more advanced. ×250.
Fig. 41. Purkinjean vesicle, more advanced. ×250.
Fig. 42. Purkinjean vesicle, more advanced. ×250.
Fig. 43. Purkinjean vesicle, more advanced. ×250.
Fig. 44. Youngest Purkinjean vesicles of *Oniscus*. ×250.
Fig. 45. Testis and vas deferens of *Phalangium cornutum*.
A. Diagram of ovary of *Glomeris*.
B. Diagram of ovary of *Coccus*.
C. Diagram of ovary of *Phalangium*.

XXVI. *On the Influence of Atmospheric Pressure upon some of the Phenomena of Combustion.* *By Dr.* E. Frankland, *F.R.S.*

Received June 20,—Read June 20, 1861.

In his classical researches upon flame, Davy mentions the influence which compression and rarefaction exert upon combustion in atmospheric air. Speaking of his experiments with compressed air, the performance of which presented considerable difficulties, he says*, "They show sufficiently that (for certain limits at least) as rarefaction does not diminish considerably the heat of flame in atmospherical air, so neither does condensation considerably increase it; a circumstance of great importance in the constitution of our atmosphere, which at all heights or depths at which man can exist, still preserves the same relations to combustion." His attention was also arrested by the light evolved under similar circumstances, although this phase of the subject does not seem to have attracted more than his cursory attention, and he does not appear to have made any exact quantitative determinations of the rate of increase or diminution of the light of combustion. In reference to this point he says†, "Both the heat and light of the flames of the taper, of sulphur, and of hydrogen were increased by acting on them by air condensed four times; but not more than they would have been by an addition of ¼ of oxygen." And again‡, "The intensity of the light of flames in the atmosphere is increased by condensation and diminished by rarefaction, apparently in a higher ratio than their heat, more particles capable of emitting light exist in the denser atmospheres, and yet most of these particles in becoming capable of emitting light, absorb heat; which could not be the case in the condensation of a pure supporting medium."

M. Triger, a French engineer §, records some observations on combustion in compressed air, which were made during some engineering operations of a peculiar kind, carried on in working a bed of coal lying beneath the alluvium on the banks of the river Loire. A stratum of quicksand from 59 to 65½ feet thick had to be penetrated; and it was consequently necessary to find some means of excluding the quicksand and water, which it was found impossible to keep out by the ordinary coffer-dams. To overcome this difficulty, M. Triger ingeniously employed stong wrought-iron cylinders about 3¼ feet in diameter, open below and closed at top. These were gradually sunk in the quicksand, whilst the air inside them was compressed to the necessary extent to exclude the outer semifluid matter. The workmen labouring within these cylinders were exposed to a pressure of about three atmospheres; and it was observed that the candles, by which they

* Philosophical Transactions for 1817, p. 65. † Ibid. p. 64.

‡ Ibid. p. 75 § Ann. de Chimie et de Physique, 3 sér. tome iii. p. 234, 1841

were lighted, burnt with much greater rapidity than at ordinary atmospheric pressure. Respecting this rapid combustion, M. Triger says, "A la pression de trois atmosphères, cette accélération devient telle que nous avons été obligés de renoncer aux chandelles à mêches de coton pour les remplacer par des chandelles à mêches de fil. Les premières brulaient avec une telle rapidité qu'elles duraient à peine un quart d'heure, et elles répandaient en outre une fumée intolérable."

An observant officer of artillery stationed in India, Quartermaster Mitchell, found that the time of burning of the fuses of shells was considerably increased from the diminution of atmospheric pressure at elevated stations. To the results of his experiments I shall presently have to refer in detail.

Finally, J. Le Conte*, in his interesting memoir on the influence of solar light on combustion, expresses the following opinion, with reference to the observations of Davy, Triger, and Mitchell: "Thus a variety of well-established facts concur in fortifying the conclusions to which we are led by *à priori* reasoning, namely, that the process of combustion is *retarded* by diminution of the density of the air, whilst it is accelerated by its condensation." M. Le Conte did not himself make any experiments on the influence of atmospheric pressure on the rate of combustion.

Such was the state of knowledge and opinion regarding the influence of atmospheric pressure upon the heat and light of combustion, when in the autumn of 1859, whilst accompanying Dr. Tyndall to the summit of Mont Blanc, I undertook some experiments on the effect of atmospheric pressure upon the rate of combustion of candles.

I. INFLUENCE OF ATMOSPHERIC PRESSURE ON THE RATE OF COMBUSTION.

a. Of Candles.

In the experiments just alluded to, six stearin candles were first burnt for one hour at Chamonix, the amount of stearin consumed being carefully determined for each candle: the same candles were afterwards burnt for one hour, carefully protected from currents of air, in a tent on the summit of Mont Blanc, the consumption of stearin being again ascertained. The following results were obtained:—

Number of Candle.	At Chamonix.	Summit of Mont Blanc.
	Barometer 26·4 inches, temp. 21°·5 C. Stearin consumed in one hour.	Temperature of air in tent 0°·5 C. Stearin consumed in one hour.
	grammes.	grammes.
1.	9·2	8·7
2.	9·9	9·5
3.	9·2	9·2
4.	10·4	8·8
5.	9·5	9·3
6.	9·2	9·0

* Silliman's American Journal of Science and Arts [2] xxiv. 317.

These numbers give the following average rates of combustion:—

At Chamonix ..... 9·6 grms. stearin per hour.
Summit of Mont Blanc ..... 9·1 grms. stearin per hour.

Or, omitting the fourth candle, which obviously gave abnormal results, the following would be the average rate of combustion:—

At Chamonix ..... 9·4 grms. stearin per hour.
Summit of Mont Blanc ..... 9·2 grms. stearin per hour.

This close approximation of the two results under such widely different atmospheric pressure, goes far to prove that *the rate of combustion of candles is entirely independent of the density of the air*, the slight discrepancy being probably attributable to the difference (21° C.) of atmospheric temperature in the two series of experiments. It is impossible to repeat these determinations in a satisfactory manner with artificially rarefied atmospheres, owing to the heating of the apparatus which surrounds the candle, and the consequent guttering and unequal combustion of the latter. But in an experiment with a sperm candle, which was burnt first in air under a pressure of 28·7 inches of mercury, and then in air at 9 inches pressure, other conditions being as similar as practicable in the two experiments, the consumption of sperm was found to be,—

At pressure of 28·7 inches . . . . 7·85 grammes of sperm per hour.
At pressure of 9·0 inches . . . . 9·10 grammes of sperm per hour.

This experiment, unsatisfactory as it was in several respects, tends to confirm, for higher degrees of rarefaction, the result previously obtained.

β. *Of Time-fuses.*

In a letter dated January 6th, 1855, an extract from which appeared in the 'Proceedings of the Royal Society*,' Quartermaster MITCHELL communicated the results of a series of carefully conducted experiments, proving that the rate of combustion of the fuses of shells was subject to considerable retardation, which he attributed to the diminution of atmospheric pressure at elevated stations, causing a more scanty supply of oxygen. The following is a short statement of the results of these experiments, in which three-inch fuses were burnt under different atmospheric pressures:—

	Height of barometer at 0° C.	Elevation above sea-level.	Time of burning.
	inches.	feet.	seconds.
1. Average of 6 experiments ..	29·61	....	14·25
2 Average of 6 experiments ..	26·75	3000	15·78
3 Average of 4 experiments ..	23·95	6500	17·10
4 Average of 2 experiments ..	22·98	7300	18·125

* Vol. vii. p. 316.

Comparing the amount of retardation with the corresponding reduction of pressure, we have the following results:—

Numbers of experiments between which comparison is made.	Diminution of pressure.	Retardation of combustion.
	inches.	seconds.
1 and 2	2·86	1·53
2 and 3	2·80	1·82
3 and 4	·97	1·025

Although these results, as I shall presently endeavour to show, are perfectly compatible with those obtained with candles under similar circumstances, yet the subject seemed to me of sufficient technical importance to warrant a repetition and extension of these experiments in artificially rarefied air. For this purpose a large iron cylinder was connected on the one hand with an air-pump, and on the other with a piece of gas-pipe 6 feet long and 4 inches internal diameter, the opposite end of the pipe being furnished with an arrangement by which the end of the fuse to be ignited could be introduced air-tight within the pipe, whilst the closed end of the fuse projected about 2 inches into the external air. The fuses were ignited at a given instant by a voltaic arrangement, consisting of ten cells of GROVE's battery, an instantaneous contact maker, and a piece of thin platinum wire which was inserted into the priming of the fuse. In order to ascertain with precision the moment when the deflagration was finished, the lateral hole at the posterior end of the fuse was bored through to the opposite side: a piece of thread was passed vertically through this aperture, and secured above to a convenient support, whilst an iron ball was affixed to its lower extremity at a distance of a few inches above an iron plate, upon which the ball fell when the fire reached the thread, thus indicating the moment when, under ordinary circumstances, the fire of the fuse would be communicated to the contents of the shell. The pressure was indicated by a mercurial gauge inserted into the gas-pipe.

The experiments were made with 6-inch fuses (for which I was indebted to the kindness of Mr. ABEL of the Woolwich Royal Arsenal), in the following manner. The fuse being inserted into the end of the gas-pipe, and the necessary degree of exhaustion in the iron cylinder and pipe having been obtained, the fuse was ignited at a given signal. During the continuance of its deflagration, an assistant worked the air-pump so as to prevent any great rise in pressure, whilst another observed the vacuum-gauge at the moment when the iron ball dropped. The mean between the pressure at the commencement of the deflagration, and that at the end, was assumed to be the mean pressure under which the fuse had been burnt; but it is obvious that this assumed mean pressure can only be approximative, although the gauge fell very regularly and gradually during the continuance of the deflagration.

The following results were obtained:—

I. At a barometrical pressure of 30·4 inches, fuse No. 1 burnt 31 seconds*.

* The first three fuses were burnt in the open air, but the arrangements for their ignition and for determining the cessation of combustion, were the same as in the other determinations.

II. At a pressure of 30·4 inches, fuse No. 2 burnt 30 seconds.
III. At a pressure of 30·4 inches, fuse No. 3 burnt 30 seconds.
IV. At the mean pressure of 28·4 inches, fuse No 4 burnt 32 seconds.
V. At the mean pressure of 28·1 inches, fuse No. 5 burnt 32·5 seconds.
VI. At the mean pressure of 25·55 inches, fuse No. 6 burnt 35 seconds.
VII. At the mean pressure of 25·85 inches, fuse No. 7 burnt 34·5 seconds.
VIII. At the mean pressure of 22·35 inches, fuse No. 8 burnt 38 seconds.
IX. At the mean pressure of 22·55 inches, fuse No. 9 burnt 37·5 seconds.
X. At the mean pressure of 19·9 inches, fuse No. 10 burnt 42 seconds.
XI. At the mean pressure of 19·4 inches, fuse No. 11 burnt 41 seconds.
XII. At the mean pressure of 16·15 inches, fuse No. 12 burnt 46 seconds.
XIII. At the mean pressure of 15·75 inches, fuse No. 13 burnt 45 seconds.

It will be seen, from an inspection of the above numbers, that, after the three first experiments at atmospheric pressure, an attempt was made to burn two fuses at the same pressure, but owing to the gauge sinking to the extent of about two inches during the deflagration, the mean pressures at which each pair of fuses were burnt never exactly coincided. For the purpose of comparison, however, it will be convenient to take the mean both of the pressures and times of burning of each pair, and to express the results as follow:—

Average pressure, in inches of mercury.	Average time of deflagration of 6-inch fuse.	Increase of time of burning over preceding observation.	Reduction of pressure corresponding with increase of time.	Increase of time for each diminution of 1 inch pressure.
	seconds.	seconds.	inches.	seconds.
30·40	30·33			
28·25	32·25	1·92	2·15	·893
25·70	34·75	2·50	2·55	·980
22·45	37·75	3·00	3·25	·925
19·65	41·50	3·75	2·80	1·339
15·95	45·50	4·00	3·70	1·081

There are here evident indications of the rate of retardation being somewhat greater at low than at comparatively high pressures; but, neglecting these indications, the above numbers give 1·043 second as the average retardation in a six-inch or thirty-seconds fuse for each inch of mercurial pressure removed. This result agrees closely with that obtained by Quartermaster MITCHELL, if we except those fuses which he burnt at the greatest altitude, and in reference to which some error must obviously have crept in, either as regards the altitude of the station where the fuses were burnt, or the duration of their combustion. The latter source of error is perhaps rendered less improbable, from the fact that only two experiments were made at the greatest altitude, whilst six were performed at two, and four at the third of the remaining stations. The following Table shows Mr. MITCHELL's results, uniformly with those in the last Table. The fuses which he employed being fifteen-seconds or three-inch ones, I have multiplied their

times of combustion by two, in order to bring them into comparison with the six-inch fuses which were used in my experiments:—

Pressure in inches of mercury.	Average time of combustion of 6-inch fuse.	Increase of time of combustion over last observation.	Reduction of pressure corresponding to increase of time.	Increase of time for each diminution of 1 inch pressure.
	seconds.	seconds.	inches.	seconds.
29·61	28·50			
26·75	31·56	3·06	2·86	1·070
23·95	34·20	2·64	2·80	·943
22·98	36·25	2·05	·97	2·113

Here, omitting the last determination as abnormal, we have the average retardation, in the combustion of a six-inch fuse, equal to 1·007 second for each diminution of one-inch mercurial pressure, which coincides almost exactly with the number (1·043) deduced from my own experiments.

The results of both series of observations may therefore be embodied in the following law:—*The increments in time are proportional to the decrements in pressure.*

For all practical purposes the following rule may be adopted:—*Each diminution of one inch of barometrical pressure causes a retardation of one second in a six-inch or thirty-second fuse.* Or, *each diminution of atmospheric pressure to the extent of one mercurial inch increases the time of burning by one-thirtieth.*

This retardation in the burning of time-fuses by the reduction of atmospheric pressure, will probably merit the attention of artillery officers. These fuses have hitherto been carefully prepared so as to burn, at Woolwich, a certain number of seconds, and the perfection with which this is attained is highly remarkable; but such time of combustion at the sea-level is no longer maintained when the fuses are used in more elevated localities. The ordinary fluctuations of the barometer in our latitude must render the rate of combustion of these fuses liable to a variation of about ten per cent. Thus a fuse driven to burn 30 seconds when the barometer stands at 31 inches, would burn 33 seconds if the barometer fell to 28 inches. The height to which a shell attains in its flight must exert an appreciable influence upon the burning of its time fuse; to a far greater extent, however, must the time of combustion be affected by the position of the fuse during the flight of a rifled shell: as in these projectiles the fuse always precedes the shell, the time of burning must obviously be very much shorter than when the shell and fuse are at rest. In an ordinary shell which rotates upon a horizontal axis, the alternate compression and rarefaction of the air at the mouth of the fuse, although tending to compensate each other, will still leave a considerable balance of compression, which must cause a marked retardation in the rate of burning.

The apparently opposite conclusions to which we are led as regards the influence of atmospheric pressure upon the *rate* of combustion, by the experiments upon candles on the one hand and upon time-fuses on the other, are by no means irreconcileable; in

fact, an examination into the conditions of combustion in the two cases scarcely leaves room for the expectation of any other result.

In the combustion of a candle, the radiant heat from the flame first melts the combustible matter in the capsule at the base of the exposed portion of the wick; the capillary action of the latter then elevates the liquefied wax, tallow, or spermaceti into the upper part of the wick, where it is exposed to a temperature which effects its volatilization and decomposition. It is thus evident that the rate of combustion, or at all events the rate of consumption of the combustible matter of the candle, is entirely dependent upon the capillarity of the wick, provided that the radiant heat from the flame is sufficient to keep up a supply of liquefied combustible matter at the base of the wick, and that the temperature of the flame is high enough to volatilize this matter on its arrival near the apex of the cotton. Now, as capillary attraction is not altered by variations of atmospheric pressure, and as the temperature of the flame is, as will be shown below, almost entirely independent of the same influence, a diminution in the consumption of combustible matter could only arise from the amount of radiant heat striking the capsule at the base of the wick being insufficient to keep up a supply of melted combustible matter equal to the capillary demands of the wick. There can scarcely be a doubt that the amount of heat radiated from a given area of the lower surface of the flame is diminished by rarefaction, owing to the decreasing luminosity of the flame: nevertheless this diminution is compensated by the increased flame surface, the radiant heat from which strikes the capsule when the flame becomes enlarged by rarefaction. Whether this compensation be complete or not is of little importance, since observation shows that, even at the highest degrees of rarefaction, a sufficient amount of heat reaches the capsule to keep up an abundant supply of liquefied combustible matter.

We have therefore no ground for any *à priori* assumption that the combustion of candles ought to go on at a decreased rate in rarefied air; in fact there is one consideration which might lead to the opposite opinion; it is this,—the rapidity of the consumption of a candle obviously depends upon the amount of liquefied wax, &c. which passes up its wick in a given time: this amount is determined, up to a certain maximum limit, by the rapidity with which it is got rid of at the upper portion of the capillary tubes where it is volatilized by the heat of the flame. Now, as the rapidity of volatilization is known to be increased by a reduction of pressure, it follows that a larger amount of the combustible would be thus removed from the upper portion of the wick in rarefied than in compressed air. Nevertheless this influence of reduced pressure must be very small in the case of bodies possessing such high boiling-points as tallow, wax, &c., and hence its influence upon the rate of combustion in rarefied air cannot be perceived.

Opposed to the above facts and considerations stands the observation of M. Triger, that candles burn much more rapidly in air compressed three times than in air at the ordinary pressure. This discrepancy, the cause of which can now only be conjectured, may perhaps reside in some of the circumstances, described above, under which the experiments were made. The constant supply of compressed air to a chamber such as that

in which the candles were burnt, must have occasioned a comparatively high temperature in the atmosphere of the chamber, necessarily causing the candles to gutter. Further, the very imperfect combustion which a candle undergoes at this high pressure would have a tendency to increase the size of that portion of the wick situated within the flame, and which constitutes the surface from which the evaporation of the combustible proceeds. Both these circumstances would, I conceive, practically tend greatly to shorten the time during which a candle would burn, which was precisely the circumstance that alone attracted M. Triger's attention, no quantitative determinations of the weight of combustible matter *actually consumed* having been made.

In the deflagration of time-fuses, the conditions are obviously very different. Here the combustible matter never comes into contact with atmospheric oxygen until it has left the fuse-case; unlike the candle, the composition contains within itself the oxygen necessary for combustion, and a certain degree of heat only is necessary to bring about chemical combination. If this heat were applied simultaneously to every part of the fuse-composition, the whole would burn almost instantaneously. This sometimes approximately occurs, when, by the expansion of the wooden case into which the composition is rammed, a slight space is formed between the case and its contents, thus allowing the deflagration to propagate itself between the case and the composition. Under such circumstances the fuse burns with explosive rapidity; and probably the occasional bursting of shells before, or immediately after leaving the guns, may be due in some cases to this cause. Under normal circumstances, however, the fuse burns only at a disc perpendicular to its axis; and the time occupied in its deflagration necessarily depends upon the rapidity with which each successive layer of composition is heated to the temperature at which chemical combination takes place. This heat, necessary to deflagration, is evidently derived from the products of the combustion of the immediately preceding layer of composition; and the amount of heat thus communicated to the next unburnt layer must depend, in great measure, upon the number of particles of these heated products which come into contact with that layer. Now, as a large proportion of these products are gaseous, it follows that, if the pressure of the surrounding medium be reduced, the number of ignited gaseous particles in contact at any one moment with the still unignited disc of composition will also be diminished. Hence the slower rate of deflagration in rarefied air.

II. INFLUENCE OF ATMOSPHERIC PRESSURE ON THE LIGHT OF COMBUSTION.

a. *Influence of Rarefaction.*

In burning candles upon the summit of Mont Blanc, I was much struck by the comparatively small amount of light which they emitted. The lower and blue portion of the flame, which, under ordinary circumstances, scarcely rises to within a quarter of an inch of the apex of the wick, now extended to the height of one-eighth of an inch above the cotton, thus greatly reducing the size of the luminous portion of the flame.

On returning to England, I repeated the experiment under circumstances which

enabled me to ascertain, by photometrical measurements, the extent of this loss of luminosity in rarefied air. The result proved that a great reduction in illuminating effect ensues when a candle is transferred from air at the ordinary atmospheric pressure to rarefied air. At the same time remarkable changes in appearance occur in the flame itself, especially at high degrees of rarefaction. During the diminution of pressure down to half an atmosphere, the chief alteration is the gradual invasion of the upper and luminous portion of the flame, by the lower blue and non-luminous part. As the pressure sinks towards 10 inches of mercury, the retreat of the luminous portion of the flame towards the apex goes on uninterruptedly, but the shape and colour of the flame also begin to undergo very remarkable alterations; the summit becomes more and more rounded, until at 10 inches pressure the flame assumes nearly the form of an ellipse, whilst the blue portion, which now comprises nearly the whole flame, acquires a peculiar greenish tint. Finally, at 6 inches pressure the last trace of yellow disappears from the summit of the flame, leaving the latter an almost perfect globe of the peculiar greenish-blue tint above mentioned. Just before the disappearance of the yellow portion of the flame, there comes into view a splendid halo of pinkish light, forming a shell half an inch thick around the blue-green nucleus, and thus greatly enlarging the dimensions of the flame. The colour of this luminous shell closely resembles that first noticed by Gassiot in the stratified electrical discharge passing through a nearly vacuous tube containing a minute trace of nitrogen. The colour thus imparted to the electrical discharge undoubtedly constitutes the most delicate test of the presence of nitrogen. In both cases I believe the coloured light to be due to incandescent nitrogen. Under a pressure of 4·6 inches of mercury, a small gas-flame burning in the chimney *b*, Plate XVIII. fig. 1, nearly fills the latter with the pinkish glow just mentioned, which extends to a height of nearly 3 inches above the true flame, forcibly reminding the observer of the electrical discharge through a nearly vacuous tube. The gas-flame did not manifest any tendency to extinction at this low pressure.

In attempting photometrical determinations with candles, it was found that, owing to the irregularities of combustion already noticed, no satisfactory quantitative experiments could be made in artificially rarefied air. Oil-lamps proved also equally unsuitable, owing to the gradual ascent of the base of the flame towards the apex of the wick, by which the size of the flame and the hourly consumption of oil were greatly diminished. Recourse was therefore had to coal-gas, which, although liable to certain minor disturbing influences, yet yielded results, during an extensive series of experiments, exhibiting sufficient uniformity to render them worthy of confidence.

Fig. 1, Plate XVIII., represents the arrangement of the apparatus employed. A is a governor into which the gas was first led, and whence it issued through a T-piece, one branch of which led to the jet B, which I will call *the standard flame*, whilst the other communicated with the test-meter C supplying a jet D, which may be conveniently termed *the experimental flame*: thus the delivery of gas of uniform pressure, at the two stopcocks regulating the supply to the two flames, was secured. The standard flame was shielded

from currents of air by a cylindrical glass shade. The vessel or receiver, in which the experimental flame was made to burn under different atmospheric pressures, consisted of a glass cylinder 12 inches high and $4\frac{1}{2}$ inches in diameter, welted and ground at both ends, which could be closed air-tight by the ground cast-iron plates *a a*, between which and the ends of the cylinder collars of leather were introduced, so as to distribute more evenly the pressure exerted by the nuts and screws of the three steel rods binding together the upper and lower plates. *b* is a glass chimney contracted at top and cemented with plaster of Paris into the stopcock *c*, which opens the communication for the exit of the products of combustion; *d* is a similar stopcock inserted into the under plate, for the admission of air into the cylinder. The gas-delivery tube *e* passes air-tight through a stuffing-box *f* in the cover of the cylinder, and was most carefully united with the exit-tube of the meter so as to exclude the admission of any trace of air during the experiments, and especially whilst the gas was being consumed under reduced pressure.

The atmosphere within the glass cylinder D could be uniformly maintained at any pressure, from that of the atmosphere downwards, by means of the air-pump E and the reservoir F. The latter was constructed of wrought iron, and, having a capacity of $2\frac{3}{4}$ cubic feet, served to maintain a very constant pressure unaffected by the intermittent action of the air-pump. The pressure in the reservoir, and consequently in the glass cylinder D, was indicated by the gauge G, which was usually worked with mercury, but occasionally, as described below, with water. H H is a Bunsen's photometer, by means of which, the relative intensity of the light of the standard flame and of the experimental flame was determined. The moveable paper disc *g* was protected from diffused light by the cylinder *h h*, so placed that the line joining the two flames passed through its axis. This cylinder was pierced with two apertures at opposite sides,—the one in front (shown in the figures) allowing the observer simultaneously to see the reflected images of both sides of the disc in two mirrors (not shown in the figure) placed at a proper angle behind the opposite aperture.

Such is a general outline of the arrangement of the apparatus used; but the following additional particulars may serve to illustrate more fully the mode of working, and also to explain some of the details of the figure not yet alluded to. The test-meter C was constructed in the usual manner, so as to show by observations of one minute's duration, the rate of consumption per hour; but, in order to ensure greater accuracy, these observations were always extended over a space of at least five minutes, and were repeated at intervals during the course of the determinations at each particular pressure. In order to have the rate of admission of gas to the experimental flame perfectly under control, a micrometer stopcock (*i*) was inserted in the exit-tube of the meter. Just above the internal orifice of the stopcock *d*, was placed a circular disc, so as to prevent the current of air from impinging upon the experimental flame; by this arrangement the latter always burnt with perfect steadiness. It is well known that the illuminating effect of a gas-flame depends very considerably upon the velocity of the current of air in which it burns, and that the maximum illuminating effect is always produced when

the velocity of the current of air is only just sufficient to prevent the escape of unconsumed fuliginous matter; in other words, a maximum of light is obtained from a gas-flame, other things equal, when that flame is maintained just below the *smoking-point*. This condition of maximum luminosity was carefully secured in all the following determinations, by regulating, by means of the stopcock *d*, the admission of external air, and thereby the velocity of the current in the chimney *b*, the cock *c* being wide open. When, however, the experimental flame was burning at atmospheric pressure, the cock *d* was removed, leaving a large aperture for the admission of air, whilst the current through the chimney was regulated by the cock *c*. The extremity of the gas-delivery tube *e* was narrowed to a circular aperture 1·5 millimetre in diameter (about $\frac{1}{17}$th inch), thus forming a burner of such magnitude as not only to prevent the gas from being discharged from *e* with more than the smallest possible pressure, but also to render the difference of pressure between the gas in *e* and the air in the glass cylinder practically the same in all the observations. In the early stages of the experiments considerable annoyance was experienced by the water produced in the combustion condensing in the tubes leading from the chimney *b* to the reservoir F, whilst the cock *c* and the caoutchouc connector between *c* and *k* became inconveniently heated. These difficulties were removed by enclosing the caoutchouc joint with a tin jacket (*l*), which was kept filled with hot water, and by providing two double-necked bottles (only one of which, *m*, is shown in the figure), immersed in vessels of cold water, for the completion of the refrigeration and the collection of the condensed water. *m* was conveniently connected with F, and the latter with the air-pump, by means of vulcanized india-rubber tubing of sufficient substance to resist compression, when rendered nearly vacuous internally.

Each series of experiments was made in the following manner:—The standard flame B was lighted, and its rate of consumption regulated to about ·6 or ·7 cubic foot per hour. The absolute amount of gas consumed by this flame was obviously not material, provided that its rate of consumption and conditions of combustion did not vary during the continuance of any one series of experiments. This constancy in the rate and conditions of combustion was secured by the governor A. The cock *c* being closed, and the pressure in F reduced to about 6 inches of mercury, the cock *d* was removed, *c* slightly opened, and the experimental flame ignited by the introduction of a small taper through the aperture from which *d* had been removed: the latter cock was then replaced, and gradually turned so as to cut off all but the necessary supply of air, whilst *c* was at the same time gradually opened so as to equalize the pressure in the glass cylinder and in F. With this diminution of pressure in the glass cylinder it was of course necessary simultaneously to reduce the size of the aperture through which the gas passed from the meter to the burner; and this was effected by the micrometer cock *i*. The pressure in F was now allowed to rise until it reached the lowest point at which a series of observations was to be made: at this point it was then maintained constant by the steady working of the air-pump. The consumption of gas in the experimental flame having been now accurately adjusted to ·65 cubic foot per hour, and all extraneous light excluded

from the room, a preliminary observation was made of the illuminating power of the experimental flame as compared with the standard. Owing to the gradual heating of the apparatus surrounding the experimental flame, the temperature, and consequently the luminosity of the latter, underwent a gradual and not unimportant increase, which continued for about an hour, when the illuminating power became perfectly constant. As soon as this constancy of light had been obtained, twenty observations of the illuminating power were made. The pressure in F was then suffered to rise to the point at which the next observations were to be made. The consumption of gas was again carefully adjusted to ·65 cubic foot per hour, when twenty photometrical observations were again made. Similar sets of observations at the remaining higher pressures up to the full atmospheric pressure completed the series.

In the following tabulated results the illuminating power of the standard flame is assumed to be 100, whilst the numbers given in the several columns represent the luminosity of the experimental flame compared with this standard. In all the series of observations, the consumption of gas by the experimental flame was ·65 cubic foot per hour, measured at the atmospheric pressure.

First Series.

No. of Observation.	Illuminating power of Experimental Flame compared with Standard Flame at 100. Pressure of air in receiver =					
	6·6 in. mercury.	9·6 in. mercury.	14·6 in. mercury.	19·9 in. mercury.	24·9 in. mercury.	29·9 in. mercury.
1	1·0	6·4	24·2	63·4	90·2	119·9
2	1·0	6·4	24·4	63·4	90·1	119·6
3	1·1	6·5	24·1	63·1	90·0	119·3
4	1·0	6·5	24·1	63·1	89·8	119·2
5	1·1	6·5	24·1	63·3	90·4	119·5
6	1·2	6·6	24·2	63·1	90·4	119·6
7	1·2	6·5	24·2	63·2	90·2	119·4
8	1·1	6·5	24·4	63·2	90·1	119·6
9	1·2	6·4	24·4	63·4	90·1	119·5
10	1·1	6·5	24·4	63·3	90·0	119·5
11	1·1	6·5	24·1	63·5	89·8	119·7
12	1·2	6·5	24·2	63·5	89·8	119·9
13	1·1	6·6	24·2	63·6	89·7	120·2
14	1·2	6·5	24·4	63·6	89·9	120·5
15	1·1	6·5	24·3	63·8	90·0	120·6
16	1·0	6·5	24·2	64·0	90·0	120·6
17	1·1	6·4	24·1	64·1	89·8	120·7
18	1·1	6·5	24·1	64·0	89·7	120·7
19	1·0	6·5	24·2	63·8	89·9	120·8
20	1·1	6·5	24·1	63·8	90·0	120·6
Mean	1·1	6·5	24·2	63·5	90·0	119·97

Second Series.

No. of Observation.	Illuminating power of Experimental Flame compared with Standard Flame at 100. Pressure of air in receiver =										
	10·2 in. mercury	12·2 in. mercury	14·2 in. mercury	16·2 in. mercury	18·2 in. mercury	20·2 in. mercury	22·2 in. mercury	24·2 in. mercury	26·2 in. mercury	28·2 in. mercury.	30·2 in. mercury.
1	4·3	14·6	23·6	35·1	44·0	56·8	72·9	86·3	95·6	108·1	117·5
2	4·2	14·6	23·7	35·1	44·2	56·8	72·9	86·3	95·5	108·1	117·5
3	4·3	14·6	23·7	35·1	44·2	56·5	72·9	86·3	95·5	108·6	118·4
4	4·3	14·6	23·6	34·9	44·2	56·8	72·9	86·8	95·4	108·6	118·4
5	4·2	15·0	23·6	35·1	44·2	56·5	72·6	86·8	95·1	108·1	118·4
6	4·3	15·0	23·6	34·9	44·2	56·5	72·9	87·1	95·5	109·0	118·4
7	4·2	15·0	23·7	35·1	44·0	56·8	72·6	87·1	95·7	109·0	118·4
8	4·3	15·0	23·7	34·9	44·0	56·8	72·9	87·1	95·9	109·5	118·8
9	4·2	15·0	23·7	34·9	44·2	56·8	73·2	86·8	95·8	109·0	118·8
10	4·3	15·0	23·5	34·7	44·2	57·0	73·2	86·8	95·8	109·5	119·4
11	4·2	14·9	23·5	34·7	44·2	57·0	72·9	86·8	95·9	108·6	119·4
12	4·2	14·9	23·6	34·7	44·4	56·8	72·9	86·8	95·9	108·6	119·4
13	4·3	15·0	23·7	34·9	44·4	56·8	72·9	87·1	95·9	108·1	119·9
14	4·3	15·0	23·6	34·7	44·6	57·0	73·2	87·1	96·0	108·1	119·9
15	4·2	15·0	23·7	34·9	44·6	56·8	72·9	87·1	95·9	108·6	119·4
16	4·2	15·0	23·6	35·1	44·6	57·0	72·2	86·8	95·7	109·0	119·4
17	4·3	15·0	23·5	35·1	44·6	56·8	72·6	87·1	95·7	108·6	118·8
18	4·3	15·0	23·5	34·9	44·4	56·8	72·9	86·8	95·9	108·6	118·4
19	4·3	14·9	23·6	34·9	44·6	57·0	73·2	86·8	95·6	108·1	118·4
20	4·3	15·0	23·5	34·9	44·6	56·8	73·2	87·1	95·9	108·6	117·5
Mean	4·3	14·9	23·6	34·9	44·4	56·8	72·9	86·8	95·7	108·6	118·8

In order to bring the two series of observations into more strict comparison with each other, and with following determinations, it will be convenient to reduce the mean experimental numbers to a standard of illuminating power, in which the light at the maximum pressure, that is the full atmospheric pressure, is assumed to be 100. We then get the following numbers:—

First Series.

Pressure of air in receiver in ins. of mercury.	Mean illuminating power. Experimental.	Mean illuminating power. Reduced.
29·9	119·97	100·0
24·9	90·0	75·0
19·9	63·5	52·9
14·6	24·2	20·2
9·6	6·5	5·4
6·6	1·1	·9

Second Series.

Pressure of air in receiver in ins. of mercury.	Mean illuminating power. Experimental.	Mean illuminating power. Reduced.
30·2	118·8	100·0
28·2	108·6	91·4
26·2	95·7	80·6
24·2	86·8	73·0
22·2	72·9	61·4
20·2	56·8	47·8
18·2	44·4	37·4
16·2	34·9	29·4
14·2	23·6	19·8
12·2	14·9	12·5
10·2	4·3	3·6

These numbers show that even the natural oscillations of atmospheric pressure produce a considerable variation in the amount of light emitted by gas-flames; and as such a variation is of interest from a technical point of view, it appeared to me of sufficient importance to warrant a special series of observations within, or nearly within, the usual fluctuations of the barometrical column. In order to attain greater delicacy in the pressure-readings in these experiments, a water-gauge was substituted for a mercurial one, but its indications are translated into inches of mercury in the following tabulated results:—

Third Series.

No. of Observation.	Illuminating power of Experimental Flame compared with Standard Flame at 100.			
	Pressure of air in receiver =			
	27·2 in. of mercury.	28·2 in. of mercury.	29·2 in. of mercury.	30·2 in. of mercury.
1	70·1	75·5	77·8	83·7
2	70·1	74·5	77·8	84·1
3	70·4	73·8	78·8	84·1
4	70·1	73·8	79·9	83·7
5	70·1	73·8	77·8	83·7
6	70·4	74·2	77·8	83·7
7	70·1	73·8	77·8	83·4
8	70·1	74·2	77·8	83·4
9	70·4	74·5	80·2	83·4
10	70·1	74·5	80·2	83·4
11	70·1	74·8	79·8	83·0
12	70·4	74·8	79·2	83 0
13	70·1	74·5	80·5	82·7
14	70·4	74·8	78·8	83·0
15	70·4	75·5	79·8	82·7
16	70·2	75·5	79·8	83·0
17	70·1	75·5	78·8	83·0
18	70·7	75·5	80·2	83·4
19	70·7	75·5	79·8	83·4
20	70·7	75·1	80·5	83·7
Mean	70·3	74·7	79·2	83·4

Reducing the means of these results, as before, to the maximum standard of 100, we have the following numbers:—

Third Series.

Pressure of air in receiver in ins. of mercury.	Mean illuminating power. Experimental.	Mean illuminating power. Reduced.
30·2	83·4	100·0
29·2	79·2	95·0
28·2	74·7	89·6
27·2	70·3	84·3

It is thus evident that the combustion of an amount of gas which will give a light equal to 100 candles when the barometer stands at 31 inches, will yield light equal only to 84·3 candles when the barometer falls to 28 inches. Such a variation in the luminosity of gas-flames with the oscillations of the barometer will obviously elude the ordinary modes of taking the illuminating power of gas, inasmuch as the standard light with which the gas is compared is also subject to the same influence. Still, although the relative light of gas as compared with candles may remain nearly or quite unaltered, yet its absolute illuminating value depends greatly upon the height of the barometer at the place where it is burnt. Thus a quantity of coal-gas which in London would yield a light equal to 100 candles would, if burnt in Munich, give an illuminating effect equal to little more than 91 candles; whilst if used to light the city of Mexico, its luminosity would be reduced to 61·5 candles. These numbers are independent of the change of volume by reduced pressure. If equal volumes of the same sample of coal-gas were consumed in London and Mexico, the illuminating effects would be as 100 : 46·2, the temperature being the same in both cases.

An inspection of the above three series of observations, reveals the fact that the rarefaction of air, from atmospheric pressure downwards, produces a uniform diminution of light until the pressure is reduced to about 14 inches of mercury, below which the diminution of illuminating power proceeds at a less rapid rate. This uniformity of relation between pressure and luminosity will be more clearly seen from Plate XIX. diagrams 1 and 2, in which the luminosity is represented by the ordinates, and the pressure by the abscissæ measured from the origin B. If therefore the luminosity were simply proportional to the pressure, the curve of luminosity would coincide with the diagonal drawn from A to B in diagram No 1. Inasmuch, however, as the diminution of light is more rapid than the diminution of pressure, the lines A C and A D, representing the experimental results of the first and second series of observations, fall between this diagonal and the ordinate corresponding to the point A. Diagram No. 2 shows the results of the third series of observations: in order to render it as open as possible, only that portion of the square is given through which the experimental line A B passes. The line A C in diagram No. 1 represents the average results of the first series of observations, whilst A D indicates those of the second series. It will be seen, from an inspection of both

series, not only that the lines are nearly coincident, but that they do not, down to 14 inches pressure, deviate much from a straight path. This is obviously due to an equal, or nearly equal diminution of light for each equal decrement of pressure down to about 14 inches, below which pressure both lines deviate markedly from their previous direction, indicating an alteration in the rate of the diminution of luminosity. The mean results of the three series of observations give approximately 5·1 per cent. of the luminosity at 30 inches pressure as the diminution of light corresponding to each diminution of 1 inch of mercurial pressure down to 14 inches. The following Tables exhibit the illuminating effect actually observed compared with that calculated from this constant:—

First Series.

Pressure in inches of mercury.	Illuminating power.	
	Observed.	Calculated.
29·9	100	100
24·9	75·0	74·5
19·9	52·9	49·0
14·6	20·2	22·0
9·6	5·4	− 3·5
6·6	·9	−18·8

Second Series.

30·2	100·	100
28·2	91·4	89·8
26·2	80·6	79·6
24·2	73·0	69·4
22·2	61·4	59·2
20·2	47·8	49·0
18·2	37·4	38·8
16·2	29·4	28·6
14·2	19·8	18·4
12·2	12·5	8·2
10·2	3·6	−2·0

Third Series.

30·2	100	100
29·2	95·0	94·9
28·2	89·7	89·8
27·2	84·4	84·7

The dotted lines in diagrams Nos. 1 and 2 represent the calculated luminosity according to the above Tables. The experimental lines above 14 inches pressure do not in any part of their course deviate more from the calculated line than might be expected from the usual errors of experiment. The law of the diminution of the light of gas-flames by reduction of pressure from 30 inches to 14 inches of mercury, may therefore be thus stated. *Of* 100 *units of light emitted by a gas-flame burning in air at a pressure of* 30 *inches of mercury*, 5·1 *units are extinguished by each diminution of one mercurial inch*

of atmospheric pressure; or, more generally, *the diminution in illuminating power is directly proportional to the diminution in atmospheric pressure.*

It must, however, here be remarked that the above determinations only establish the constant 5·1 for the particular quality of gas with which the experiments were performed. It still remained to be ascertained whether a flame from gas of a different quality would be amenable to the same rate of reduction; a fourth series of observations was therefore made with gas naphthalized to such an extent as nearly to double its illuminating power. The consumption of gas by the experimental flame was, as before, ·65 cubic foot per hour. The following results were obtained:—

Fourth Series.

No. of Observation.	Illuminating power of Experimental Flame compared with Standard Flame at 100. Pressure of air in receiver =					
	6·9 in. mercury.	9·9 in. mercury.	14·9 in. mercury.	19·9 in. mercury.	24·9 in. mercury.	29·9 in. mercury.
1	1·0	7·3	28·0	55·6	85·3	114·0
2	1·0	7·2	28·0	55·5	85·2	114·0
3	1·1	7·2	28·1	55·6	85·2	114·1
4	1·0	7·3	28·2	55·6	85·3	114·2
5	1·0	7·5	28·1	55·8	85·3	114·5
6	·9	7·5	28·1	55·9	85·5	114·6
7	·9	7·2	28·2	55·8	85·6	114·8
8	1·0	7·2	28·2	55·7	85·6	114·9
9	·9	7·2	28·1	55·9	85·5	115·1
10	1·0	7·3	28·2	55·9	85·7	115·2
11	1·1	7·4	28·3	55·5	85·4	115·2
12	1·1	7·5	28·3	55·4	85·3	115·3
13	·9	7·5	28·3	55·5	85·7	115·5
14	·9	7·6	28·5	55·2	85·7	115·4
15	·9	7·5	28·3	55·3	85·5	115·4
16	1·0	7·6	28·3	55·1	85·9	115·5
17	1·0	7·6	28·4	55·2	86·0	115·5
18	1·1	7·5	28·4	55·3	86·0	115·4
19	1·1	7·5	28·5	55·3	86·0	115·2
20	1·1	7·6	28·3	55·7	85·6	115·2
Mean	1·00	7·42	28·24	55·54	85·58	114·95

The following Table, calculated from the above, shows that these results are completely in harmony with those obtained with unnaphthalized gas, thus proving that the rate of diminution of luminosity in rarefied air is the same for all hydrocarbon gases, of whatsoever quality; the two last columns being nearly identical down to 14·9 inches pressure.

Pressure of air in inches of mercury.	Mean illuminating power. Experimental.	Mean illuminating power. Reduced.	Illuminating power. Calculated.
29·9	114·95	100	100
24·9	85·58	74·4	74·5
19·9	55·54	48·3	49·0
14·9	28·24	24·5	23·5
9·9	7·42	6·4	− 2·0
6·9	1·02	·9	−17·3

This series was continued down to 4·6 inches pressure, but the illuminating power of the flame could then no longer be measured by the photometer.

β. Influence of Compression.

The foregoing experiments having demonstrated a very remarkable diminution of light in candle- and gas-flames by a reduction of atmospheric pressure, it became interesting to ascertain the effect of compressed air upon the luminosity of similar flames. At the very outset of this part of the inquiry considerable difficulties presented themselves, since it became necessary to abandon a gaseous combustible, which could not be compressed to the necessary degree, and then delivered at a uniform pressure through a burner, without very complex apparatus. I was thus compelled to resort to solid or liquid combustibles, the irregularities of which were still further increased by the space within the combustion-chamber being necessarily more confined, in order that its walls might the better sustain high pressures. These difficulties in the way of accurate determinations were, however, by no means the most formidable; for it was soon found that any considerable increase of atmospheric pressure caused both candle- and oil-flames to throw off large quantities of fuliginous matter, the formation of which could not be prevented by any amount of draught that could be established in the chimney of the apparatus. Hence, although the luminosity of the flames was greatly increased, yet it was obviously much less so than would have been the case under conditions of more perfect combustion. In fact it soon became evident that the determinations of increase of luminosity by compression must be made in a manner precisely the reverse of that employed for the corresponding determinations in rarefied air; for whilst in the latter case the experiments were made with flames, which at ordinary atmospheric pressure were saturated with carbon particles, in the former it was found necessary to commence with flames which were very feebly, or not at all luminous at common pressures. Such is the effect of compressed air in determining the precipitation of carbon particles within the flame, that a small alcohol lamp, which at the ordinary pressure burnt with a pure blue flame, became highly luminous when placed under a pressure of four atmospheres; and it can scarcely be doubted that at a pressure of five or six atmospheres, its luminosity would be equal to that of sperm oil burning at atmospheric pressure.

The apparatus employed, and which is shown in Plate XVIII. fig. 2, was very similar to that used in the previous part of the inquiry. A is the gas-governor regulating the supply to the standard flame B. H H is the photometer arranged as before, and *a a* are the ground plates, rods, and screws for securely closing the ends of the glass cylinder containing the experimental flame. The comparatively thin and wide cylinder used in the rarefaction experiments was here replaced by the strong glass cylinder D, 12 inches long, 2 inches internal diameter, and $\frac{5}{8}$ inch thick; *e* is the lamp furnishing the experimental flame; it is fixed upon the rod *l* passing through a stuffing-box in the lower plate, and enabling it to be adjusted to any height. *b* and *c* are the glass chimney and stopcock arranged as before, except that *c* now opens at once into the air. The

interior of D is connected with the pressure-gauge G by the tube *i*, whilst the cock *d* communicates with the compressed-air reservoir F by means of the tube *f*. E is a condensing syringe communicating with F by the tube *k*. By this interposition of the reservoir F, a very constant pressure could easily be maintained in the cylinder D.

The experiments were made in the following manner. The cover *a* being removed, and F charged with compressed air, a gentle stream of the latter was turned into D through the cock *d*. The lamp *e* was now lighted and the cover *a* firmly screwed into its place, the cock *c* being wide open. The admission of air through *d* was then regulated so as to produce in the chimney *b* that degree of draught necessary for obtaining the maximum amount of light in the experimental flame. After the latter had been allowed to burn for about half an hour, so as to bring the surrounding glass to a temperature which afterwards remained tolerably uniform, a series of photometrical observations were made. The egress of air through the cock *c* was then gradually diminished, whilst *d* was fully opened, so as to establish a free communication, and consequently an equality of pressure, between the reservoir F and the cylinder D. The pressure, as indicated by the gauge G, was now adjusted, by the more or less rapid working of the pump E, to that required for the next series of observations. In practice it was found impossible, with the same liquid in the lamp, to extend any series of observations over a greater range than one atmosphere, owing to the experimental flame beginning to smoke at the higher pressure, if it possessed a measurable illuminating power at the lower one.

Owing to the difficulties above mentioned, I have only been able to obtain satisfactory determinations between one and two atmospheres. In these determinations the lamp was supplied with amylic alcohol—a liquid which, whilst affording an appreciable amount of light in the experimental flame under one-atmosphere pressure, was found to burn under two atmospheres without smoke, although at a somewhat higher pressure it began to evolve unconsumed carbon. The following results were obtained:—

No. of Observation.	Illuminating power of Experimental Flame compared with Standard Flame at 100.		
	Pressure in receiver in inches of mercury =		
	29·7 inches.	59·7 inches.	59·5 inches.
1	21·0	55·6	55·3
2	21·0	56·2	55·4
3	21·2	56·3	55·3
4	21·1	56·3	55·4
5	21·2	56·1	55·4
6	21·1	56·0	55·3
7	21·2	56·0	55·4
8	21·5	55·9	55·5
9	21·6	55·6	55·5
10	21·6	55·4	55·5
11	21·0	55·4	55·3
Mean	21·2	55·9	55·4

These numbers approximate to those calculated in accordance with the law already given for pressures below that of the atmosphere, thus confirming that law for pressures up to two atmospheres, as is seen from the following comparison, in which the experimental numbers are reduced to the standard of 100 at atmospheric pressure.

Pressure.	Illuminating power.	
	Observed.	Calculated.
1 Atmosphere	100	100
2 Atmospheres first ..	263·7	253
2 Atmospheres second	261·3	253

Further determinations, in which the illuminating power at three- and four-atmospheres pressure was compared, yielded results differing widely from this law, and indicating a much more rapid increase of light; but as the liability to errors of observation increases greatly at these higher pressures, I place very little confidence in the numbers obtained, which I will nevertheless here briefly state in the same form as the last Table. In these experiments the lamp was fed with a mixture of five parts of vinic alcohol and one part of amylic alcohol. The lamp when fed with this mixture had no appreciable illuminating effect under ordinary atmospheric pressure.

Pressure.	Illuminating power.	
	Observed.	Calculated.
3 Atmospheres	406	406
4 Atmospheres	959	559

In endeavouring to trace the causes of this variation of luminosity, it will be convenient first to consider the general conditions upon which the light of flames depends. The luminosity of the flames generally used for artificial light emanates from two sources; viz., first, from the ignition of minute particles of carbon floating in the shell of flame; and secondly, from the incandescence of gaseous matters. The latter source of illumination probably does not usually furnish more than one per cent. of the total amount of light; consequently nearly the whole of the light given out by flames under ordinary circumstances is due, as Davy first pointed out*, to the ignition of solid carbonaceous matter. The light emitted by incandescent gaseous particles becomes, however, much more prominent at very low pressures; and as this light is not materially influenced by pressure, it causes the deviation from the law of diminution of light, seen at the lower extremities of the lines A C and A D in Plate XIX. diagram No. 1. In order to gain a clear conception of the mechanism of a candle- or gas-flame, we must picture to ourselves first a core of gas or vapour containing hydrocarbons, and secondly a shell

* Philosophical Transactions, 1817, p. 64.

of ignited matter closely surrounded on its outside by atmospheric air. The uninterrupted supply of gas or vapour to the core forces the contents of the latter constantly through the ignited shell, at the inner wall of which, those hydrocarbons that cannot exist at a bright red heat, either undergo decomposition into light carburetted hydrogen and free carbon, or imperfect combustionin to water, carbonic oxide, and free carbon, or finally perfect combustion into water and carbonic oxide, or even carbonic acid, without any separation of free carbon. The nature of the decomposition or combustion which these hydrocarbons undergo on coming in contact with the ignited shell, thus evidently depends upon the amount of oxygen which gains access to the interior of the shell; if that quantity be insufficient to convert the whole of the carbon of the hydrocarbons into carbonic oxide, the residue will be precipitated, and the flame will be a more or less luminous one; whilst if the amount of oxygen present be sufficient, after burning the hydrogen, to consume the whole of the carbon to carbonic acid or even to carbonic oxide, no light will be produced from the incandescence of carbon particles.

Now it is well known that the light of any flame may be increased by increasing the number of carbon particles simultaneously floating in it, provided those particles are consumed before they leave the flame, and are not evolved as smoke. I have also elsewhere shown* that the light of gas-flames, and doubtless that of candles and oil also, greatly depends upon the heat of the flame, the rise in temperature caused by merely heating the air supplied to a gas-lamp, by the waste heat of the flame itself, being sufficient to increase the light to the extent of 67 per cent. without any increased consumption of gas. Such being the conditions necessary for the *increase of light*, it is scarcely necessary to add that the reversal of these conditions, viz. the decrease in the number of particles of carbon existing in the flame at a given time, imperfect combustion allowing the escape of unconsumed carbon, and decrease of temperature in the flame, determine a *diminished luminosity*.

One of the first causes which naturally suggests itself to account for the diminution of light by decreased atmospheric pressure, is the diminished amount of oxygen in a given bulk of the supporting medium rendering combustion imperfect, and thus either causing particles of carbon to escape unconsumed, or determining their conversion into carbonic oxide instead of carbonic acid. The effect of the first would be to diminish the luminosity, whilst the second would have the effect of decreasing the light indirectly by diminishing the temperature of the flame. A careful inspection of a gas- or candle-flame, burning in an atmosphere undergoing gradual rarefaction, does not afford the slightest evidence of smoke, or even of an increased tendency to throw off fuliginous matter; on the contrary, the tendency to smoke obviously diminishes as the rarefaction progresses; whilst, on the other hand, the fact that an increase of pressure beyond that of the atmosphere causes the most smokeless flames to become smoky, renders utterly untenable the assumption that the escape of unconsumed carbon is one of the causes of diminished luminosity in rarefied atmospheres. Whether or not there is imperfect combustion in

* Ure's Dictionary, 1860, article "Coal-gas."

the sense of an escape of carbonic oxide instead of carbonic acid from the flame, could not be thus decided, and demanded a closer investigation.

To determine this point I collected samples of the gases escaping from the chimney *b* (fig. 1) of the experimental flame (which in this case was that of a sperm candle) when burning under atmospheric pressure, and again when burning under a pressure of only 8 inches of mercury. These gases were first treated with caustic potash to absorb carbonic acid; they were then exploded with an equal volume of electrolytic water-gas, and subsequently with excess of hydrogen. The explosion with water-gas caused no contraction, proving the absence of carbonic oxide. The following numbers were obtained:—

I. Gases from candle burning at atmospheric pressure:—

	Volume of gas.	Temperature.
Gas used	256·0	7·0° C.
After absorption of carbonic acid ...	238·1	7·0 „
After explosion with electrolytic gas..	238·0	7·0 „
After admission of hydrogen	373·7	7·0 „
After explosion	283·6	7·0 „

II. Gases from candle burning at 8 inches mercurial pressure:—

	Volume of gas.	Temperature.
Gas used	290·8	7·1° C.
After absorption of carbonic acid ...	267·2	7·1 „
After explosion with electrolytic gas..	267·3	7·1 „
After admission of hydrogen	457·8	7·1 „
After explosion	367·9	7·1 „

These numbers give the following per-centage composition of the two samples of gas:—

	I.	II.
Nitrogen	81·28	81·58
Oxygen	11·73	10·30
Carbonic acid	6·99	8·12
Carbonic oxide	0·00	0·00
	100·00	100·00

These results prove that in both cases there was no escape of unconsumed combustible gas; consequently *the diminution of light in rarefied atmospheres is not due to imperfect combustion in any form.*

Taken in connexion with the experiments in compressed air, in which imperfect combustion attended with the evolution of fuliginous matter was very marked, these data lead to the remarkable conclusion that *the compression of air renders the combustion of gaseous matter less perfect*; and that, within certain limits at least, *the more rarefied the atmosphere in which flame burns, the more complete is its combustion.* Thus it is evident, not only that no diminution of light can arise from imperfect combustion in rarefied

atmospheres, but also that no reduction of temperature in the flame can occur from the same cause.

A second cause of the diminution of the light of combustion in rarefied atmospheres, and its increase in compressed ones, might be sought for in a possible difference between the temperatures of the flame in the two cases. It is well known that if air be allowed to escape from a vessel into a vacuum, a considerable diminution of temperature ensues in the vessel from which the air escapes; and inasmuch as the gaseous products of combustion assume a larger volume in rarefied atmospheres than in compressed ones, it can scarcely be doubted that the pyrometric thermal effect of a flame must be diminished to some extent by rarefying the medium in which it burns; nevertheless this effect may be nearly or quite neutralized by the smaller amount of refrigeration caused by the rarefied atmosphere. In order to elucidate this point, a spiral of platinum wire was ignited to visible redness in a flame of hydrogen; on then rarefying the air around the flame and wire, no appreciable alteration in the temperature of the platinum spiral could be noticed. A similar experiment was tried with an alcohol flame, and with the same result. A spiral of platinum wire placed under the receiver of an air-pump, was ignited to visible redness by a voltaic current; on exhausting the receiver, the glow of the platinum gradually increased nearly to whiteness. On readmitting the air, it again diminished to dull redness, showing that the refrigerating effect of rarefied air is much less than that of air at the ordinary pressure. Thus, whilst the temperature produced within a given flame is lowered by rarefaction, the escape of heat from its exterior is hindered by the same process,—the result apparently being that the actual temperature of the flame undergoes but little alteration. This confirms DAVY'S conclusion, that *rarefaction and compression, within certain limits at least, do not exert any considerable influence upon the heat of flame.*

Although an inquiry into two of the possible causes of the diminution of the light of combustion in rarefied atmospheres has thus failed to afford any explanation of the phenomenon, yet one of them indirectly points to what I believe to be the conditions determining the variation in illuminating power. If it be true that combustion is more complete in rare atmospheres than in dense ones, it follows that the light of a smokeless flame must decrease with a diminution of pressure, since, with more perfect combustion, that is, with freer access of oxygen to every part of the flame, there must be a diminution of unconsumed carbon separated within the flame, and consequently a diminished amount of light evolved. In fact, the appearance of the experimental flame during the progress of rarefaction on the one hand, and of compression on the other, can scarcely leave a doubt on the mind of an observer, that the variation of luminosity depends essentially upon the admission of oxygen to that portion of the shell of flame where particles of carbon are usually precipitated, and where consequently the region of luminosity is situated. That an admission of oxygen or air to the interior of a luminous flame has the effect of greatly diminishing or even practically annihilating its luminosity, has been long known

and even utilized in the wire-gauze and BUNSEN's burners, where heat and not light is the object of the combustion of gas. But it may be asked, what conditions are there in the combustion of flame in rarefied air, that favour the admission of a larger proportion than usual of air to the interior of the flame? In reply, it may be stated that there are two conditions in such combustion, both of which directly tend to produce this result. The first of these conditions, and the one to which I conceive nearly the whole of the effect to be due, is the greater mobility of *rarefied* gaseous bodies, which must produce a more rapid admixture of the flame, gases, and external air than would otherwise take place. The second condition is the gradual, though slow, increase in the volume of the flame as the atmospheric pressure decreases, thus causing the flame to present a gradually increasing surface of contact with the exterior air. This alteration in the volume of flame by diminished pressure is more strikingly seen with a sperm candle than with gas. When such a candle burns under a pressure of two atmospheres, its flame presents the appearance of a sharp spike scarcely one-fourth of an inch in diameter at its lower and broadest part, the apex being lost in the dense smoke which issues from the upper portion of the flame. If the pressure be now diminished, the diameter of the spike markedly increases, especially about its centre, until at one-atmosphere pressure the flame assumes its ordinary appearance. On now rarefying the air, the transverse diameter of the flame goes on increasing until, when the pressure is reduced to about 6 mercurial inches, the flame becomes nearly globular with a diameter of about three-fourths of an inch.

Now, as the amount of combustible matter in the flame was maintained constant in the photometrical experiments detailed above, it follows that the increased external flame surface must so alter the conditions of combustion as to bring the constant amount of combustible matter into contact with a gradually increasing quantity of oxygen. That a large amount of air does, even under ordinary circumstances, gain access to the interior of gas- and candle-flames, has been proved by the interesting researches upon the gases of these flames recently made by HILGARD*, who found 64 per cent. of nitrogen in the interior of a candle-flame, and by LANDOLT†, who detected 66 per cent. of nitrogen in the interior of a gas-flame; on no occasion, however, did these experimenters find oxygen in the luminous portion of the flame, although it was found in the blue or non-luminous section. I conceive therefore that these consequences of diminished pressure, viz. increased gaseous mobility and augmented volume of flame, are quite competent to explain the variations in luminosity resulting from alterations in the pressure of the supporting medium, and that *these variations in illuminating power depend chiefly, if not entirely, upon the ready access or comparative exclusion of atmospheric oxygen as regards the interior of the flame.*

In conclusion, the influence of atmospheric pressure upon the phenomena of combustion may be thus summed up.

* Ann. der Chem. und Pharm. vol. xcii. p. 129. † POGGENDORFF's 'Annalen,' vol. xcix. p. 389.

1. The rate of burning of candles and other similar combustibles, whose flames depend upon the volatilization and ignition of combustible matter in contact with atmospheric air, is not perceptibly affected by the pressure of the supporting medium.

2. The rate of burning of self-supporting combustibles, like time-fuses, depends upon the rapidity of fusion of the combustible composition, which rapidity of fusion is diminished by the more rapid removal of the heated gases from the surface of the composition. Hence the rate of burning of combustibles of this class depends upon the pressure of the medium in which they are consumed. In the case of time-fuses, the increments in the time of burning are proportional to the decrements in the pressure of the surrounding medium.

3. The luminosity of ordinary flames depends upon the pressure of the supporting medium; and, between certain limits, the diminution in illuminating power is directly proportional to the diminution in atmospheric pressure.

4. The variation in the illuminating power of flame by alterations in the pressure of the supporting medium depends chiefly, if not entirely, upon the ready access of atmospheric oxygen to, or its comparative exclusion from, the interior of the flame.

5. Down to a certain minumum limit, the more rarefied the atmosphere in which flame burns, the more perfect is its combustion.

XXVII. *Account of Experiments made at Holyhead (North Wales) to ascertain the Transit-Velocity of Waves, analogous to Earthquake Waves, through the Local Rock Formations.* By Robert Mallet, *C.E., F.R.S.*

Received June 18,—Read June 20, 1861.

In my "Second Report on the Facts of Earthquake Phenomena" in the Report of the British Association for 1851, the transit-velocities were experimentally determined of waves of impulse produced by the explosion of charges of gunpowder, and these velocities shown to be

In wet sand	824·915 feet per second,
In discontinuous granite . .	1306·425 feet per second,
In more solid granite . . .	1664·574 feet per second,

the range of sand employed having been that of Killiney Strand, and of granite that of Dalkey Island, both on the east coast of Ireland. These results produced some surprise on my own part as well as on that of others,—the transit-velocities obtained falling greatly below those which theory might have suggested as possible, based upon the modulus of elasticity of the material constituting the range in either case.

I suggested as the explanation of the low velocities ascertained, that the media of the ranges (like all the solids constituting the crust of the earth) were not in fact united and homogeneous elastic solids, but an aggregation of solids more or less shattered, heterogeneous, and discontinuous, and that to the loss of *vis viva*, and of time, in the propagation of the wave from surface to surface, was due the extremely low velocities observed.

The correctness of this view, and a general corroboration of the correctness of the experimental results themselves, have since been made known by the careful determinations by Nöggerath and Schmidt respectively, of the transit-velocities of actual earthquake waves in the superficial formations of the Rhine country and of Hungary, and by myself in those of Southern Italy, all of which present low velocities coordinating readily with my previous experimental results.

In the Report above mentioned, I suggested the desirableness of extending the experimental determination of wave-transit to stratified and foliated rocks, as likely to present still lower velocities than those obtained for shattered granite, as well as other important or suggestive phenomena. The operations in progress at the Government quarries at Holyhead (Island of Anglesea, North Wales), of dislodging vast masses of rock by means of gunpowder for the formation of the Asylum Harbour there, appeared to me to present a favourable opportunity of making some experiments upon the

stratified rock formations of that locality, by taking advantage of the powerful explosions necessary at the quarries. These quarries are situated (see Map, Plate XX.) on Holyhead Mountain on its N.E. flank, in metamorphic quartz rock, and in 1852 (a vast mass of material having been already removed) presented a lofty, irregular, and nearly vertical scarp, reaching to 150 feet in height above the floor of the quarry in some places.

From this wall of solid rock the process of dislodgement was continued, not by the usual method of blasting, by means of small charges fired in jumper holes bored into the rock, but by the occasional explosion of large mines, containing at times as much as *nine tons* of gunpowder lodged in one or in three or more separate foci, deep within the face of the cliff, and formed by driving "headings" or galleries from the base of the mural face into the rock. From the charges of powder placed in bags at the innermost extremities of these headings, which were stopped up by several feet of "tamping" of stone, rubbish, and clay, conducting-wires were led out to a suitable and safe distance; so that on making by these the circuit complete between the poles of a powerful SMEE'S galvanic battery, a small piece of thin platinum wire adjusted within the charge of gunpowder became heated, and ignited the powder. The explosion thus followed instantaneously the making contact between the poles of the battery.

Experience has enabled the engineers charged with the work so exactly to proportion the charge of powder to the work it is intended to perform in each case, that no rock is thrown to any distance; the whole force is consumed in dislocating and dropping down to its base as a vast sloping talus of disrupted rock and stone the portion of the cliff operated on; in fact, at the moment of explosion the mass of previously solid rock seems to fall to pieces like a lump of suddenly slacked quicklime. The shock or impulse, however, delivered by the explosion upon the remaining solid rock, behind and around the focus, and propagated through it in all directions outwards, as an elastic wave of impulse, was at an early stage of the operations remarked to be so powerful, that it could be felt distinctly in the quaking of the ground at distances of several hundred yards, and was sufficient even to shake down articles of delf ware from the shelves of cottages a long way off from the quarries.

Early in 1853 I visited those quarries, and examined generally the adjacent locality and rock formations, and having satisfied myself that these operations could be made available, I applied to my distinguished friend, the late lamented Mr. RENDELL, C.E., the engineer in chief of the Asylum Harbour, and readily obtained from him permission to make such experiments as should not interfere with the progress of the works.

The prosecution of these experiments having been favourably represented to the British Association for the Advancement of Science, and to the Council of the Royal Society, a sum of money was voted by each of these bodies respectively, and placed at the author's disposal, with the desire that he should undertake and conduct the experiments.

It was not, however, until the summer of 1856 that my own avocations, and various preliminaries, allowed any progress to be made with the experiments themselves. Ne-

gotiations had to be entered on with several parties; with the occupier of some land at Pen-y-Brin, about a mile to the east of the quarries, where the most suitable spot for placing the seismoscope (the observer's station O, see Map) was found, for permission to enter his land, and level down to a horizontal surface the face of the rock, here occupying the surface of the ground, and to erect an observer's shed over it; and with the Electric Telegraph Company, for the hire of insulating telegraph poles and wires, and for their erection over the range intervening between this spot and the highest reach of the quarry hill.

As these great blasts are fired only occasionally and at uncertain intervals, and being prepared *must be fired without postponement*, and within a given hour of the day, namely, during the workmen's dinner hour (12 to 1 P.M.), when the quarries are clear of men, and therefore safe from accident, it became at once obvious that very frequent journeys, both on my own part and on that of such assistants as I should require, would have necessarily to be made to and from Holyhead; and to economize as much as possible the large expenditure that must thus arise, I applied to the City of Dublin Steam Packet Company, and to the Chester and Holyhead Railway Company, through their respective Secretaries, representing the scientific character of the undertaking, and requesting on their parts cooperation, by their permitting myself and my assistants, with any needful apparatus, to pass free to and from Holyhead by their respective vessels from Kingstown Harbour. After much fruitless correspondence I regret to say that both these Companies refused to render any assistance whatever, a boon the refusal of which greatly increased the expenditure for these experiments. Lastly, I placed myself in communication with Messrs. RIGBY, the contractors of the vast works of the Quarries and Harbour, and in August 1856 received from them the assurance of every assistance that they could afford consistently with the prosecution of the works. To them, to Mr. R. L. COUSENS, C.E., the acting engineer for their Firm on the works, and to Mr. G. C. DOBSON, C.E., chief engineer on the work under Mr. RENDELL (since under Mr. HAWKSHAW), my thanks are due for the best and most cordial assistance upon all occasions.

The position for the observer's station and seismoscope upon the levelled floor of rock at Pen-y-Brin having been fixed upon, the first operation necessary was to obtain an accurate section of the surface in the line between that and the quarries, a geological section of the rock formations along the same line, and with precision the exact distance in a straight line, from some fixed point adjacent to the quarries, to the observer's station. The fixed point chosen at the quarries was the flagstaff at the bell, which is rung whenever a blast is about to be fired, this being so placed that from it measurements and angular bearings with the line of range, OW (Map), from the various sites of future explosions could readily be made, and thus the exact distance of each focus of explosion (to be hereafter experimented on) from the seismoscope at O ascertained, the flagstaff always remaining undisturbed as a fixed terminal at the quarry end of the range.

The whole surface, O to W, was carefully levelled over, and the distances chained, as

given in Plate XXI. Section I. The roughness of the ground and its inclination, however, rendered direct measurement of the range of wave-path with sufficient accuracy impracticable, and it was found necessary to obtain it trigonometrically. For this purpose a base-line of 1432 feet in length was measured off along the rails of the tramroad which connects the quarry with the east breakwater, between the points A and B (Map, Plate XX.), where the road fortunately was found straight and nearly quite level.

This was measured with two brass-shod pine rods, each of 35 feet in length, of the same sort, and applied in the same manner, as I used in 1849 for measuring the base of one mile on Killiney Strand, for the particulars of which the "Second Report on Earthquakes," &c., Report. Brit. Assoc. 1851, p. 274, &c., may be referred to. The base was measured forwards and backwards, with a result differing by less than 3 inches. The flagstaff at the spot marked W in the Map, is not visible from the observer's station, owing to some intervening houses and other objects; a staff was therefore set up at S upon the hill-side. The point O was connected by angular measurements with the extremities of the measured base A and B; the triangles OBS and OSW were then obtained, whence that OBW was arrived at, from which, finally, the distance OW (the constant part of the range) was ascertained to be $=4584{\cdot}80$ feet. The triangle OBW was used as a check upon that OSW, as the angles at O, S, and W had to be taken, owing to local circumstances, smaller than is desirable. The lengths of the side OW obtained from the two triangles separately closely agreed; and as a further check, the side SW, which gave, trigonometrically, a length of $671{\cdot}07$ feet, when actually measured as a base of verification, gave $672{\cdot}05$ feet.

I was also enabled to connect the side OS with a trig point P, upon the western breakwater, and another at R, the positions of which are defined upon the accurate surveys of the harbour in Mr. Dobson's, C.E., possession, as a further means of verification; we may therefore view the length of the constant part of the range between the observing station and the flagstaff, its other permanent terminal, as equal to 4585 feet, neglecting fractions.

The base of the staff at S was found to be $68{\cdot}78$ feet above the level of the horizontal surface of the rock at Pen-y-Brin (the observing station O), and the base of the flagstaff at W is $5{\cdot}70$ feet above the same point O. The levelled surface of rock at O is 84 feet above the mean tide-level of the sea in the Asylum Harbour; and the average rise and fall of spring tides at Holyhead is 18 feet; the line of rock, therefore, through which the range passes is, except as respects surface water, permanently dry to a considerable depth. The majority of the headings are driven into the face of the quarry cliff horizontally, at from 10 to 20 feet above the level of the floor of the quarry, which is on nearly the same level as the point W. Hence, practically, the actual range of transmission through the solid rock, of the impulse from each heading when fired, to the seismoscope at the observer's station, may be considered as a horizontal line, and no correction of distance is required for difference of elevation at the two extremities of the observing-range in the deduction of our results.

The Island of Holyhead, as may be seen on consulting the sheets (Nos. 77 and 78) of the Geological Survey of England and Wales, consists mainly of chloritic and micaceous schist or slate, and of quartz rock. The latter forms the N.W. portion of the island; and in it alone are situated the Harbour quarries, upon the side of Holyhead Mountain (as it is called), the same rock rising to its summit, which is 742 feet above the sea, mean tide-level. The junction of the quartz and of the schist or slate rock runs in azimuth N. 24° E. where it crosses the line of our range, which it intersects at an angle horizontally of 73° 30′.

The schist or slate rocks here overlie the quartz, abutting against the flank of the latter, apparently unconformably, and having an inclined junction whose dip is towards the S.E. and probably, at the place where our range intersects, having an angle of dip of about 65° with the vertical. The point of junction is situated about 900 feet from the flagstaff W; so that about 2100 feet, on the average of our actual ranges, lay in quartz rock, and the remainder, or 3750 feet, in the schist or slate formation, taking the mean total range at 5851 feet. The general tendency of the schist is to a dip to the N.W., varying from 5° to 20° from the horizontal; but no well-defined bedding is obvious, either in it or in the quartz.

Lithologically, the quartz rock consists of very variable proportions of pure white, light grey, and yellowish quartz, and of white, or yellowish white, aluminous and finely micaceous clays. In many places the mass of the rock presents to the lens almost nothing but clear and translucent quartz, breaking with a fine waved glassy fracture, striking fire with steel, extremely hard and difficult to break, and showing a very ill-defined crystallization of the individual particles of quartz, which have all the appearance of pure quartzose sea-sand that had become agglutinated by heat and pressure coacting with some slight admixture of the nature of a flux.

The specific gravity of such portions, as determined for me by my friend Mr. Robert H. Scorr, A.M., Secretary to the Geological Society of Dublin, is 2·656. From this the rock passes in many places into a softer and more friable material, consisting, when minutely examined, of the same sort of quartz-grains, with a white pulverulent clay containing microscopic plates of mica disseminated between them. This fractures readily, but will still strike fire with steel; and its average specific gravity is 2·650.

Both, but particularly the harder variety, are found often in very thick masses of nearly uniform quality, separated by great master-joints, though scarcely to be considered as beds; but usually the mass, viewed in the large, is heterogeneous in the highest degree, massive and thick in one place, full of joints and even minutely foliated in others, and everywhere intersected by thin and thick veins of harder quartz, agglutinated sand and, elsewhere, friable sand, and of soft sandy clay.

Both the quartz rock and the schist of the island are intersected by three great greenstone dykes (of inconsiderable thickness, however), none of them interfering with our range, and by one or more *great* faults, all of which run through nearly the whole island in a N.W. and S.E. direction, and by numerous other minor faults and dislocations, some

of which may be seen as cutting through our line of range at *f*, *g*, *k*, *l*, in Plate XX. Section II.

At a short distance *behind* the quarry cliff, and seat of our several explosions, a great clay dyke occurs in the quartz rock—a wall, in fact, of about 20 feet in average thickness, running in the direction marked on the Map (Plate XX.), and with a dip of only about 20° from the vertical. This consists of strongly compacted clay, nearly pure white, and more or less mixed with fine sand and grains of mica, but cannot be called rock, though continually passing into stony masses. Lying as it does in rear of our experimental headings, it was of some value, as presenting a dead solid *anvil* to the pulse from each explosion, in the contrary direction to that of the observed wave of impulse, and hence causing a larger and more distinctly appreciable wave to be transmitted in the direction towards the seismoscope.

The schist rock, in colour, passes from fawn-colour and light greenish ashen grey into a rather dark tea-green. It owes its colour to disseminated thin layers of chlorite, and probably of black or green mica in minute scales, between which are thicker layers of quartz, presenting identically the same mineral characters as those of the quartz rock beneath. These layers, owing to the small relative hardness and cohesion of the chlorite and mica, present planes of weakness and of separation; the rock is, in fact, everywhere thinly foliated, the average thickness of a plate seldom exceeding 0·2 of an inch, and averaging about one half that thickness. These foliations are twisted, bent, doubled up, and distorted in every conceivable way. The contortions are often large, the curves having radii of some feet, with minor distortions within and upon them; but most commonly they are small; so that it is rare to get even a hand specimen presenting flat and undistorted foliations, while, quite commonly, hand specimens may be found presenting within a cube of 4 or 5 inches two or three curves of contrary flexure, often in all three axes, and with curvatures short, sharp, and abrupt, almost angular. There is a general tendency observable in the greater convolutions to conform more or less to the surface-contour of the country; so that the largest and flattest folds are found to occupy, with an approach to horizontality, the topmost portions of the great humps or *umbos* of schist rock that form the characteristic of the landscape, and so rolling off in folds smaller, steeper, and more convoluted towards the steeper sides, as though these masses had slipped and doubled upon themselves when soft and pasty.

Occasionally, however, where deep cuttings have exposed the interior of such surface-knolls, it is found sharply convoluted and twisted in all directions, and without any relation to the existing surface of the country. Everywhere this mass of minutely structured, convoluted, and foliated rock is cut through by joints of separation, with surfaces in direct and close contact, and by thin seams and veins of hard and sometimes pretty well crystallized quartz, now and then discoloured by oxide of iron, and with minute cavities filled with chlorite and mica, and with others of agglutinated quartzose sand, whose bounding-lines pass off rapidly, but *gradatim*, into the prevailing substance of the rock. It is by no means of equal hardness. Some portions (and these occur with-

out any order or traceable relationship throughout the mass) are much thinner in the foliation, and the layers of chlorite and mica nearly as thick as those of the intervening quartz, both being so attenuated, that to the naked eye the edge of the foliation presents only a fine streaky appearance of lighter and darker green-grey tint. The softest, however, readily strikes fire with steel; and throughout the whole mass of the rock, for the length of our range, it is so hard, coherent, and intractable as to be only capable of being quarried by the aid of gunpowder and with very closely-formed jumper holes.

The specific gravity of the densest portions of the schist rock reaches 2·765, that of the softer averages 2·746. When the rock, whether hard or soft, is broken, so that the applied surfaces of the foliations are visible, they are often found glistening and greasy to the feel, from flattened microscopic scales of mica, or possibly of talc.

The quartz rock fractures under the effect of gunpowder into great lumpy masses, with much small rubbish. The schist under that, from jumper-hole blasts, breaks up into coarse, angular, knotted and most irregular wedges, the foliations breaking across in irregularly receding steps, and (throughout our range at least) a stone with a single flat bed being perhaps unprocurable. Both rocks are absolutely dry, or free from all perceptible percolations of surface-water issuing as springs, nor does the rain penetrate their substance by absorption for any appreciable depth,—both indications of their generally compact structure.

The faults with which our range is intersected in four places, at a horizontal angle of about 75°, are not far from vertical, dipping a few degrees to the N.W. They occur at the points marked *f*, *g*, *k*, *l*, on the Geological Section (Plate XXI. Section II.); and the disturbed and shattered plate of rock between each pair respectively appears to have sustained a downthrow (or the rocks at either side the contrary) of a few feet, 10 to 12 probably. The surfaces of the walls of those faults, so far as I can judge from rather imperfect superficial indications, appear to be in close contact; and such is the character of all the small faults that intersect the formation hereabouts.

I have been thus tediously minute in describing the character of the rocks throughout our range because, if experimental determinations of earth-wave transit are to become useful elements of comparison, in the hands of the seismologists of other countries, with the observed transit-times of natural earthquake-waves, and a means of controlling such observations, it is essential that the means be afforded of accurately comparing the rock formation traversed in all cases.

From what has been described, it will be remarked that the rock here chosen for experiment presents in the highest degree the properties capable of producing dispersion, delay, and rapid extinction of the wave of impulse, so far as its structure is concerned, although the modulus of elasticity of a very large proportion of its mineral constituents (silex) is extremely high, and its specific gravity as great as that of Dalkey granite. Added to its minutely foliated and mineralogically heterogeneous character, with its multiplied convolutions, we have five great planes of transverse separation in the range, one of these forming the plane of junction of the quartz and schist, with

innumerable minor planes of separation at all conceivable angles to each other in both rocks; and yet we have highly elastic and dense materials forming the substance of both rocks, and their general mass remarkably free from open veins, fissures, or cavities.

We have also two different rocks, the one transmitting the impulse into the other, yet neither so widely differing from the other in molecular and other physical characters as to make any great or abrupt effect upon the wave at the junction probable. In fact, widely to the first glance as the quartz rock and the schist rock appear to differ, there is less real distinction of physical character between them than would be supposed: both are composed of the same siliceous sand, in about the same size of original grains, variously enveloped, in the one in chlorite and mica, and in the other in white or grey clay and mica; both have, in ancient geological epochs, doubtless derived their materials by degradation and transport from a common source as respects their main constituent, the silex; both have been submitted to approximately similar pressures, and probably like temperatures; and the agglutinating flux has probably been mainly the same for both, viz. the minute proportions of alkalies derived from the waters of an ancient ocean.

The main difference in physical structure, viewed upon the broad scale, between the quartz rock and the slate is this (as regards our experiments):—that the great joints and planes of separation on the whole approximate to *verticality* in the former, while in the latter, with the exception of some larger faults and dykes, the planes of separation are twisted and involved in all directions, but tend more to approach *horizontality*.

More interesting conditions could thus scarcely be found for experimental determination of the transit-rate of earth-waves, or more desirable for future comparison with that of earthquake-waves themselves; much more instructive, indeed, were the actual conditions than if the means of experiment presented by these vast quarry operations had been in the most regular, undisturbed, and horizontal stratified rock, like some of the mountain limestone of Ireland, or the finest and densest laminated roofing-slates of Wales. In such ranges we can predict that the transit-velocity would at least be high. In the medium chosen for these experiments it was impossible even to guess what it might be found.

I proceed to describe the instrumental arrangements made for the observation of the impulse-wave transmitted from the blasts chosen, and for the determination of the transit-time along the range of wave-path. Over the surface of solid rock that had been chiselled down to a level tabular surface at (O) Pen-y-Brin, a timber shed was erected of sufficient size to place the observer, an assistant, and all the instruments proper to that spot, under cover and secured from the wind. The side to the N.W. was open, to permit of observation along the line of range, with the means of partially closing it in high winds.

Along the line of the boundary-wall of the railway next Pen-y-Brin, and thence along up to the highest and most distant point of the quarry cliffs, a line of telegraph-posts was planted; and upon these, two properly insulated iron wires were hung, in such a

manner that at any point along their length over the quarry cliffs, a pair of branch wires (covered with gutta percha) could be led off, and in like manner another pair to the apparatus in the observing-shed at Pen-y-Brin, thus giving the means of galvanically connecting the extremities of the range, in any way that might be required.

The mines in use at the quarries frequently consist of two, three, or four separate chambers and charges, which are all fired simultaneously (see Plate XXII.); but each charge is fired by a distinct pair of wires, igniting a fine platinum wire interposed in the circuit and immersed in one of the powder-bags. The arrangement of this platinum wire in its hollow wooden frame to prevent disturbance, and its connexion with the large conducting-wires, are practically the same as those adopted by me in 1849 at Killiney, and will be found fully described in "Second Report on Earthquakes," &c., Report of British Association for 1851, p. 277.

When several charges are to be fired simultaneously, all the electro-positive wires from each chamber are collected into one mercury-cup in connexion with one pole of the battery, and all the electro-negative wires into another mercury-cup. Upon making contact between the latter and the second pole of the battery, the current at the same moment ignites all the platinum wires passing through each pair of wires as a separate conducting-path. This method requires considerable battery power, but is the only certain or reliable one for firing simultaneously a number of separate charges. When an attempt is made to pass the current from one pole of the battery through a single pair of wires, and through all the fine platinum priming-wires in succession to the return pole, there is extreme risk that the first or second platinum priming, owing to its attenuated section of wire (in virtue of which indeed alone it becomes ignited at all), may interpose so much resistance to the current as to prevent the ignition of the third, or fourth, or other subsequent primings, or that the first priming-wire may get absolutely fused or broken by the first-ignited powder, and so cut off all communication with the others before they have been heated sufficiently.

A neglect of this obvious consequence of Ohm's law of resistance appears to have been the cause of failure very recently, in an attempt to ignite a number of mines of demolition, simultaneously, at Chatham. From the great magnitude of the charges frequently fired at Holyhead, and the very serious consequences that failure of ignition would involve, the battery power habitually employed is wisely of superabundant power. It consists of a Grove's battery of thirty-two cells, each exposing 96 square inches of platinum element. It is but justice to my friend Mr. R. L. Cousins, C.E., to whose assistance in these experiments I am so much indebted, to add that during the several years he has controlled these vast blasting-operations a single failure of ignition has never occurred.

For the above reasons, and from the necessity that in the event of any failure of such apparatus as I might require for experiment, in making contact and firing the mine at a given moment, the power should still be reserved to Mr. Cousins to fire it directly afterwards in the usual way, so as not to interfere with the works, I was led, finally,

to devise the following magneto-galvanic arrangement, by which, at a signal given from the summit of the quarry cliff (where the firing-battery is usually placed, nearly above the mine or heading then to be fired, and at a safe distance back from the edge of the cliff, usually about 100 yards) that all was ready, I should myself, stationed at the observing-shed (O), be enabled to complete the contact and fire the mine, and do so in such a way as to register by means of the chronograph the interval of time that elapsed between the moment that I so made contact (or fired) and the arrival of the wave of impulse through the rocks of the range or wave-path, when made visible by, and observed by me in, the seismoscope.

For this purpose such an arrangement was required as, upon contact being made by me at the observing-shed (O), should set in motion such a contrivance, situated upon the quarry cliff, at the remote end of the telegraph wires, as should there instantly close the poles of the great (Grove's) firing-battery and so fire the mine, and in the event from any cause of this result not taking place at the preconcerted moment, that then it should be free to Mr. Cousens or his assistants to close the poles of the firing-battery by hand in the ordinary way.

In Plate XXIII., in which (fig. 1) this arrangement is figured (without reference to scale), A is one of the headings seen in the cliff-face at part of the quarries. Above the cliff at B is placed the Grove's firing-battery; the conducting wires from its poles pass down the face of the cliff and into the heading, uniting at the platinum priming-wire in the midst of the charge of powder, the further end of the wires terminating in mercury-cups at the contact-maker C (about to be described). From the electro-magnet of the contact-maker, the two insulated wires are led along upon telegraph poles from the summit of the cliff down to the observer's station at Pen-y-Brin, where they terminate also in mercury-cups, one forming the + and the other the − pole of the contact-making battery E placed there. This battery consisted of six of the usual moistened-sand batteries in use for telegraph purposes.

The chronograph (D) was placed upon the levelled rock adjacent to this battery, and conveniently for its lever (m) being acted on by the left hand of the observer, when lying at full length upon the ground, with his eye to the seismoscope based upon the rock at F, its optic axis being situated in the vertical plane of the line of wave-path or range F A. Close to the seismoscope, and at the same level as the eyepiece of that instrument, a very good achromatic telescope was adjusted upon its stand, so as to bring the heading about to be experimented on, together with the whole face of the cliff and the firing-battery, &c., within its field,—the eyepiece of this telescope being fixed at about a distance of 6 or 8 inches from that of the seismoscope, and so that the eye of the observer, while lying at ease, and with the left hand upon the lever of the chronograph (m), could be instantly transferred from the one instrument to the other. In this state of things, when the proper signal (by the exhibition of a red flag) was made, and at a preconcerted time as nearly as was practicable, by those stationed at the firing-battery at B, that "all was ready," I applied my eye to the seismoscope and pressed down the lever (m) of the

chronograph with a sharp rapid movement; this instantly closed the poles of the contact-making battery C, causing the galvanic current to pass through the electro-magnets of the contact-maker away at the quarries at C. This directly closed the poles of the GROVE'S firing-battery at B, and fired the mine. The moment I observed the arrival of the wave of impulse propagated through the range from the explosion at A in the seismoscope at F, I withdrew my hand from the lever of the chronograph (*m*), and thus stopped the instrument, the interval of time between its having been started and stopped thus registering the (uncorrected) time of transit of the wave for the distance A F. It will now be necessary briefly to describe the several instruments separately. The seismoscope and chronograph have been already fully described in the account of the experiments made in 1849 at Killiney and Dalkey (Second Report on Earthquakes, &c., Report of Brit. Assoc. 1851), to which reference may be made.

Briefly, the seismoscope (fig. 3*, Plate XXIII.) consists of a cast-iron base-plate, on the centre of the surface of which is placed an accurately formed trough (*b*), 12 inches long, 4 inches wide, and 2 inches deep, containing an inch in depth of pure mercury, with its surface free from oxide or dust, so as to reflect properly. The longer axis of this trough is placed in the direction of the wave-path, the base of the instrument being level. At the opposite end of the trough are placed standards with suitable adjustments; that at the end next the centre of impulse carries a tube (*c*) provided with an achromatic object-glass at its lower end, and a pair of cross wires (horizontal and vertical); its optic axis is adjusted to 45° incidence with the reflecting-surface of mercury in the trough. At the other end of the trough an achromatic telescope (*a*) with a single wire is similarly adjusted, so that when the moveable blackened cover (*e e*) is placed over the trough, &c., no light can reach the surface of the mercury except through the tube *c*. The image of the cross wires in the latter is therefore seen through the telescope *a*, clearly reflected and defined in the surface of the mercury, so long as the fluid metal remains absolutely at rest; but the moment the slightest vibration or disturbance is by any means communicated to the instrument, the surface of the fluid mirror is disturbed and the image is distorted, or generally disappears totally. The telescope magnifies 11·39 times linearly, and the total magnifying power of the instrument, to exalt the manifestation to the eye of any slight disturbance of the mercurial mirror, is nearly twenty-three times. Its actual sensibility is extremely great. In the present case, however, this was not needful, as the impulse transmitted from these powerful explosions produced in all cases the most complete obliteration of the image, and in those of the most powerful mines experimented on caused a movement in the mercury of the trough that would have been visible to the naked eye. Indeed in that of the 24th of November, 1860, the amplitude of the wave that reached the seismoscope was so great as to cause the mercury to sway forwards and backwards in the trough to a *depth* that might have been measured.

After the earth-wave has reached this instrument, a certain interval of time is necessary for the production of the wave in the mercury, and for its transit from the end of

the trough next *c*, where it is produced, to the mid-length where it is observed. This involves a correction in the gross transit-time as observed with it. For the methods by which the constant for this (seismoscope correction) was determined, I must refer again to Report of Brit. Assoc. 1851, pp. 280, 281. It amounts to 0″·065 in time; and as the effect of this will in every observation appear to *delay* the arrival of the earth-wave at the instrument, this constant in *time*, converted into *distance*, must be *added* to the rate of wave-transit otherwise obtained.

The chronograph (originally devised by WHEATSTONE) is shown in fig. 1*, Plate XXIII. It consists, in fact, of a small and finely made clock, deprived of its pendulum, but provided with a suitable detent, shown more at large in fig. 4*, by which the action of the weight upon it is kept always arrested, but can immediately be permitted to take place in giving it motion, upon pressing the hand quickly upon the lever *g*.

The running down of the weight causes the anchor and pallets of the escapement (*k*) rapidly to pass the teeth of the escapement-wheel (*a*), so that the clock "runs down" by a succession of minute descents, and thus the motion is practically a uniform one. It follows that as more weight is added this velocity becomes greater, and by such addition the instrument may be made to measure more and more minute fractions of time.

It registers time upon two dials (fig. 2*), each with an index; one of these is fixed on the axis of the escapement-wheel (*a*), and its dial is divided into thirty smaller and six larger divisions. The pinion on this axis is to the wheel upon the weight-barrel (*b*) as 1:12. This carries the other index, and its dial has twelve divisions, so that one of its divisions corresponds to an entire revolution of the former one. The value in actual mean time, due to the movement of the instrument as thus recorded, requires to be ascertained by reference to a clock beating seconds, so that the number of revolutions of the index *b*, and parts of revolutions of that *a*, during an interval of, say, thirty seconds, may be determined by the mean of several experiments. For the methods of performing this with the necessary correctness, I again refer to "Second Report on Earthquakes," &c., Report of Brit. Assoc. 1851, pp. 287, 289, &c.

On the present occasion, as a considerable time elapsed between the successive experiments, during which the oil on the instrument more or less changed its state, and as some were made in summer and others in winter, it became necessary to rate the chronograph anew for each experiment, or at least to verify the former rating; for this end it was necessary to provide a suitable loud-beating seconds clock, with a divided arc to the pendulum, as none such could be procured at Holyhead. The same weight was constantly used with the chronograph; and the extreme differences found, in the rating during the several years that these experiments have been in progress, were no more than the following:—

November 1856. Value in mean time of one division of the dial (*a*) = 0″·01485

May 1861. Value of same . . . . . = 0·01806

Taking for illustration the former value of the smallest division of the dial (*a*), we see

that each division of the dial (*b*) is equal one revolution of the index (*a*), and equal

$$0''{\cdot}01485 \times 30 = 0''{\cdot}4455,$$

and one revolution of the index (*b*) equal

$$0''{\cdot}4455 \times 12 = 5''{\cdot}346,$$

an *absolute* rate of movement of the instrument not widely differing from that employed in the experiments of Killiney and Dalkey, with which it is desirable that the present results should be comparable. Half a small division of the chronograph can be read; we therefore in these experiments possessed the means of recording time to within $0''{\cdot}0074$, or to nearly $\frac{7}{1000}$ths of a second.

The additional apparatus of the chronograph consisted merely of such arrangements that the releasing lever (*g*), when pressed down by the hand applied to the wood insulator at *m*, should dip at *i* into a mercury-cup, and so make contact by the wires (*b*, *b'*) between the poles of the contact-making battery (E).

It remains to describe the contact-maker (fig 2, Plate XXIII.). *c* is the base of the instrument of mahogany, carrying a vertical and bent arm (*d*) of cast iron, into the upper forked end of which the central iron bars, of about $\frac{7}{8}$ths of an inch in diameter, of the electro-magnets *a*, *a* (seen in plan in fig. 3) are secured by a cotter. The coils of covered wire round these are continuous, the wire (*b*) from the *s* + pole passing at its further end from the first coil over to the second, and at the extremity of the latter passing off to the *s* — pole by *b'*, the junctions being effected by mercury-cups in the usual way. *n* is a sliding piece of wood, secured upon the base *c*, when adjusted in place, by the screw at *s*; this carries a wrought-iron lever armature (*e*), whose arms are as 8 : 1, the shorter and rather heavier end being adjusted so as to be beneath the poles of the electro-magnets, and at such a distance beneath them that, upon passing the current through the coils, the magnets shall readily attract the short end of this lever, snatch it up into contact with the poles of the magnets, and in doing so depress the other or remote end of the lever. The latter extremity of the lever is provided, as seen more at large in figs. 4 and 5, with a forked pair of copper poles amalgamated, which, when depressed by the action of the electro-magnets, dip into the mercury of the cups *f* and *f*, and in doing so close the holes of the firing-battery, the conducting wires from which (*h* and *h*) dip respectively into mercury-cups, which by a tube bored through the wood are in permanent communication with *f* and *f* (cups) respectively. The lever and forked poles, &c., are provided with various screw adjustments as to position, range, &c., and a slender spring beneath the lever, ensuring that it shall not be accidentally moved by wind, or other cause, until acted on by the powerful grasp of the magnets.

This instrument was found to answer admirably well. It may be observed, in passing, that it gives the means of exploding mines at almost any distance, through telegraphic wires and by any moderate contact-making power, and may admit of valuable applications hereafter for the explosion, at a determinate instant, of mines for purposes of warfare.

It is obvious that a certain *loss* of *time* must occur at this contact-maker, in reference to our experiments—that in fact the total time registered by the chronograph at D is too great, by the minute interval that elapses between the arrival of the galvanic current in the coils at *a*, and the dipping of the poles *f*, *f* into the mercury-cups. With the same battery power at E and conducting-wires, this *delay* is practically constant. Its amount, however, required to be determined, and the *time*, when converted into *distance*, *added* to the gross transit-rate previously ascertained.

For this purpose the following little apparatus was employed. Its principle, though not the precise details of its construction, is shown in fig. 6, Plate XXIII. Upon a vertical steel spindle (*s*) revolving upon an agate step at bottom, and in a polished brass collar at top, a cylindric barrel is placed of 1 inch diameter, having an escapement-wheel and anchor escapement (*v*) at its lower end, all the parts being made as light as possible. Upon the upper end of the spindle a circular disc of Bristol board (cardboard), *f*, of $12\frac{3}{4}$ inches diameter, is secured by a light screw-collar (*t*) gripping the disc firmly, so that it and the spindle must revolve together. Both the upper and under surfaces of the card-disc, for an inch or two from the circumference, towards the centre, were slightly rubbed with violin-player's *hard* rosin, and the whole, resting upon its base B, placed so that the disc should rotate horizontally. A fine *elastic* silk thread is wound a few turns round the barrel, and passing over the sheave (*r*) sustains a weight (W), by the descent of which, when required, rotation can be given to the disc, &c., the weight itself being large in proportion to the inertia of the rotating parts. By suitable changes in the disposition of the parts of the contact-maker (chiefly in getting the cast-iron arm *d*, fig. 2, out of the way), it was placed at C with respect to the disc, so that the lower poles of the electro-magnets (*a*, *a*) were just above the upper surface of the card-disc, and the short end of the lever armature (*e*) just below the same, the card running free in the small space between, and the centre of the magnet-poles being exactly at a radius of 6 inches from the centre of the disc. Nearly at right angles on the disc to this, the chronograph (D) was placed and firmly fixed; a fixed point (shown in part only in the fig., *g*), formed of a bit of cylindrical mahogany, with its lower end rosined, was so fixed as to be about $\frac{1}{15}$th of an inch above the upper surface of the disc. The lever (*m*) of the chronograph, divested of its forked pole, and having a small rectangular rod of brass substituted, was so adjusted that its sustaining spring beneath should press this brass terminal up against the under surface of the disc at *p*, directly below the fixed point or stop (*g*), and, bending the card-board there, press its upper surface into contact with the lower end of *g*.

Thus the weight W being free to descend, this arrangement at *p* acted as a detent to keep the disc from moving; but when the lever (*m*) was pressed down to start the chronograph the disc immediately became released, and began to revolve by the action of the weight W. At E the contact-making battery, or one of equal power, was placed, one of its poles being connected, through the rheostat (R), by conducting-wires with the coil of the electro-magnet (*a*) and terminating at the + pole at the mercury-cup (*n*), which was in connexion with the other, or — pole of the battery.

The rheostat was adjusted so that the resistance equalled that of the conducting-wires along the telegraph poles between C and D, E (fig. 1, Plate XX.). In this state of things, when the lever (m) of the chronograph was pressed down, the disc (f) instantly commenced rotating, but directly afterwards the electro-magnet (a), whose current was established by the first movement, attracted the lever armature (e) through the disc, and the latter was arrested by being gripped between the pole of the magnet and the armature. The arc of the circumference of the disc then, at the centre of the magnet-pole (*i. e.* with 6 inches radius), that was intercepted between the marked spot (p) whence it started and that at which it was arrested, became a measure of the time lost or elapsed between starting the chronograph at the observer's station and making contact at the firing-battery in the actual experiments. The arc thus intercepted was converted into time, from the descent of the weight (W), by the common formula $t=\frac{\sqrt{s}}{4}$, s being given and equal to $\frac{1}{12}$th the length in feet of the arc described by the circumference of the disc before being arrested; and this was capable of being controlled by measuring by the chronograph itself the actual time of a given number of successive revolutions, and parts of revolutions, of the disc, the total number of complete revolutions made being taken by reckoning the coils wound off the barrel. Upon a mean of ten experiments with this apparatus, the delay at the contact-maker appeared to be no more than 0″·0143, which converted into distance, at the greatest transit-rate observed, gives a correction of 17·3 feet per second, and at that of the least of 12·8 feet per second, both additive.

It may be remarked that the small error due to inertia, &c. in this apparatus tends nearly to correct itself, the extremely small time lost at starting of the disc being very nearly equalled by its tendency to be carried a little too far by the velocity impressed; the whole inertia also of the disc-barrel, &c. was extremely small in proportion to the moving weight W.

Another correction requiring to be attended to in these experiments was the *time* of *hang-fire* in the charge of the mine, that is to say, the time required for the burning of such a portion of the whole charge of powder as should be sufficient to rupture the rock around, and so start off from the focus the wave-impulse perceived in the seismoscope—in other words, the time lost between the instant of first ignition of the powder, viewed as simultaneous with that of making contact at the firing-battery B, and the starting of the wave of impulse to be measured.

In my former experiments at Killiney Bay, it will be recollected that it was in my power to determine this experimentally and rigidly, the moderate charges of powder there employed admitting of this, and that I found it amount for 25 lbs. of powder to 0″·050513, or to about $\frac{1}{20}$th of a second. Such is, in fact, the time that the full charge of a 68-pounder takes to burn. But in the present case direct experiment was impossible; and the value for this correction can only be approximately obtained, by observing the time that elapsed in some instances between the moment of making contact at B and the first great visible movement of rock at the face of the heading. This observation I

made in three instances, noting the time by a delicately made chronoscope by M. Robert, Rue du Coq, Paris. The results gave 0″·05, 0″·04, and 0″·08 for the time of hang-fire respectively, noting from the first visible movement of rock at the face of the heading. This would give a mean of 0″·0566, or very nearly 0″·06 for the time of hang-fire, which can be viewed, however, only as an approximation. It must vary slightly with every different "heading," depending as it does upon a great variety of conditions, but probably much more upon the exact proportion subsisting in any given case between the actual resistance of the rock to the powder employed than upon the absolute quantity of the latter, although the total mass of powder burnt is also an element. The greatest *observed difference* between the greatest and least hang-fire amounted to 0″·03, which, converted into distance at the mean transit-rate of our experiments, would give a *possible* maximum error due to this cause of about 31 feet per second; the *probable* error cannot be more than about one-half that amount. This correction, converted into distance, is also *additive*.

By the methods thus described the experiments were commenced and conducted up to the middle of 1857. Great trouble and difficulty, however, were experienced from the outset in keeping the arrangements in working order and so as to be efficient when wanted at the very brief notice that could be afforded me beforehand by the officers in charge of the works, when suitable headings were about to be fixed. The entire line of telegraph wires, the observer's shed, &c., were exposed to mischief and depredation and to injury in that tempestuous place by storms, &c. The long intervals between the experiments involved preparations and adjustment of every part of the galvanic apparatus afresh; upon each occasion, and for the most trifling repairs, workmen had to be brought from Conway, or even from Manchester, as also, in every case, to make good the branch conductors from the telegraph wires. The length of the range and hilly character of the ground also produced much difficulty, in being assured that all was right from end to end against the moment at which the firing was obligatory, as well as great personal fatigue at a moment when composed ease and freedom from fatigue were most desirable for good observation.

These difficulties, in great part foreseen, had early caused me to turn my attention to the practicability of so adjusting, at the observing-station, a telescope of large field and clear definition, and so disposing the Grove's firing-battery and other apparatus at the quarry cliff, that all could be clearly seen from the former point, and the act of making contact at the firing-battery observed by myself with distinctness and certainty, the two extremities of the range being thus, as it were, visually brought together.

Two attempts to experiment in the summer and autumn of 1857, rendered abortive by derangements of the galvanic apparatus, caused me finally to abandon it, though unwillingly. I found, however, with some satisfaction, that, subject to the *possible* fatality of a cloud settling over the quarry cliff, and so shutting it out from sight just at the critical moment, the telescopic arrangement, on trial, really seemed to offer quite as accurate results as the more complex method, and more difficult to manage, of galvanic

contact-making; and the new mode was thus continued to the end of the experiments. The firing-battery being so disposed upon the sloping brow of the quarry cliff facing my station as to be clearly visible to me, as well as every movement of those employed there, a code of signals was arranged between myself and Mr. COUSENS, by which we should mutually become cognizant of the state of preparation, &c., and successive acts at our respective stations. When all was ready at both ends for the explosion, the final signal was made by Mr. COUSENS, by elevating a bright-red flag (mounted upon a short and light staff) to a vertical position, the lower end resting on a fixed point; a pre-arranged interval of a few seconds (usually 10″) intervened, when he dropped the red flag, rotating it upon the lower end of the staff held in the right hand, and with the left made contact of the poles of the firing-battery at the same instant that the flag reached the horizontal position. Standing facing me, and as distinctly observable by me upon each occasion as though I had been close beside him, my own eye and attention were directed to Mr. COUSENS's left hand; at the instant that I observed the contact made by him I released my chronograph, and at once transferred my eye from the eye-piece of the observing telescope to that of the seismoscope. A moment elapsed before my own eye adjusted itself to the focus of the latter; but the length of transit-period of the wave (always above 4″) gave ample time for this, and then at the disappearance of the cross wires, as in the former case, I arrested the chronograph. The only source of time-error introduced by this plan was that of the probability of some slight inequality of speed in dipping the poles to make contact, on Mr. COUSENS's part (which may be called his personal equation), and the introduction of a somewhat larger value than before to my own personal equation—in the former arrangement that being due to consent between my *hand* and observation by the *eye* of *one* object, in the latter, between the *hand* and observation of *two* objects.

As regards the first, several experiments were made by Mr. COUSENS and myself at the firing-station, by his repeatedly lowering the red flag and making (the movement of) contact, the contact-maker (fig. 2, Plate XXIII.) and chronograph being so arranged as to register the total interval of time, in each case, between the first visible motion of the red flag, and the completion of contact; others were so made as to register the time between the horizontal position of the red flag, and the completion of contact. The result gave a minimum error of 0″·009, and a maximum of 0″·017. The mean error, 0″·013, is thus almost equal to the constant due to the contact-maker (in previous arrangement), with this difference, however, that the error in the present case *might* be either + or −. In twelve experiments, nine were + or additive; that is to say, the contact was made more slowly with the left hand than the flag was dropped with the right. The probability is therefore 3 : 1 that the error would be always additive, and would not exceed 0″·013 even if my observation was wholly directed to the flag; but as I directed my attention as completely as possible only to the movement of the contact-making hand, it is still less, and therefore, as not amounting to more than 6 or 7 feet per second in transit-time, may be neglected altogether. As regards my own personal

equation of observation, it will be seen, on reference to "Second Report*," &c., of the former experiments at Killiney, where it was ascertained for both observers, that its amount is much too minute to enter sensibly into the present results; and it is needless to say that this is *à fortiori* the case as respects the time lost in transmission of the galvanic current through the 12,000 or 13,000 feet of conducting-wire.

The diagrams (Plate XXII.) give, to one scale, horizontal sections of the several headings, from the experiments on which, transit-results have been deduced, and a vertical section also of No. 31, quarry No. 9, as illustrative in this respect of all the others.

The line of heading, from the face of the cliff up to any focus of charge, turns, it will be seen, thrice at right angles to itself, the object being more effectually to confine the effort of the powder when fired, and prevent the mass of "tamping" from being blown out. Results have been deduced from two headings, each of single focus; two of double focus, one of triple focus, and one of four foci,—the face of the cliff blown out varying (as marked in each case in the figure) from 60 feet to 120 feet in height, and the total weight of powder fired at one time being from 2100 lbs. up to the enormous charge of 12,000 lbs., or nearly 6 tons.

It was necessary to ascertain the exact distance in a right line from each of these headings, wherever situated, to the observing-station O, at Pen y Brin; and for this purpose, previously to each explosion, the distance of the mouth of the heading was measured with accuracy (which the ground admitted of) from the flagstaff at W (see Map, and Section I. Plate XXI.). The exact distance of the latter having been previously determined from the observing-station O, as already described, the angle of azimuth made at the flagstaff by the line of constant range (O W), and by the line joining the flagstaff and mouth of the heading, was observed in each case; and we thus had the requisite data, from which was calculated, by the usual formulæ,

$$\tfrac{1}{2}(\mathrm{A}+\mathrm{B})=90^\circ-\tfrac{1}{2}\mathrm{C},$$

$$\log\tan\tfrac{1}{2}(\mathrm{A}-\mathrm{B})=\log(a-b)+\log\tan\tfrac{1}{2}(\mathrm{A}+\mathrm{B})-\log(a+b),$$

C being the observed angle, a and b the known sides from flagstaff to O, and from flagstaff to the mouth of the heading.

Thus the actual range of wave-transit from the focus of each explosion to the seismoscope at O was finally obtained. The positions respectively of each are marked by a black dot, and numbered in order of the date of experiment upon the Map (Plate XX.), taken from M. RENDELL'S chart of 1850, published by the Admiralty. Upon it the measured base (A B) and triangulation for obtaining the constant range (O W), and for checking that measurement, are marked. The actual wave-paths are therefore in right lines, from the dots No. 1, No. 2, No. 3, &c., to the point O. The coast-line and position, approximately, of the cliff-faces of the quarries, and the superficial line of junction of the quartz rock and of the slate, are also marked. The great clay dyke passing

* Report of British Association, 1851, p. 305, &c.

through the quartz rock at the quarries in rear of the headings is marked by a pair of interrupted lines.

The Map is to a scale of 1¼ inch to 1000 feet, but is not quite exact as to filling in details on land. The important distances here concerned are therefore marked in by figures.

In the following Table our chief numerical results are comprised at one view.

TABLE I.—Wave-transit Period, Experimental Results. Holyhead.

1. Date of firing heading.	2. Number of the heading.	3. Number of the quarry.	4. Weight of powder exploded.	5. Approximate weight of rock removed.	6. Total distance of mean centre of heading from observer.	7. Total observed time of transit.	8. Observed rate of transit per second, uncorrected.	9. First time correction for the seismoscope + in distance.	10. Transit-rate with first correction (col. 9).	11. No.
			lbs.	tons.	feet.	secs.	ft. per sec.	ft. per sec.	ft. per sec.	
1856, Nov. 13 ...	No. 46.	No. 9.	3,200	10,000	6382·93	7·346	896·12	58·243	954·368	1.
1856, Nov. 13 ...	No. 10.	No. 3.	2,100	7,500	5476·67	5·653	967·93	62·913	1030·915	2.
1857, May 16 ...	No. 31.	No. 9.	3,600	9,000	6377·14	6·524	977·26	63·522	1040·782	3.
1857, Dec. 18 ...	No. 33.	No. 9.	6,200	20,000	6403·48	5·455	1173·87	76·307	1250·177	4.
1860, Nov. 24 ...	No. 80.	No. 3.	12,000	36,000	5088·13	4·161	1210·79	78·701	1289·491	5.
1861, May 11 ...	No. 84.	No. 4.	4,400	13,000	5228·59	5·249	996·11	64·747	1060·857	6.

The first result that strikes the eye at once in regarding the preceding Table is, that, with the exception of the experiment No. 1, all show that the transit-rate tends to increase in velocity with the increased quantity of powder fired,—in other words, that the loss of velocity in the same rock is less, in some proportion, as the force of the originating impulse of the wave is greater, and its amplitude therefore greater on starting.

This is apparent if the uncorrected transit-rates (col. 8) be arranged in the order of increased weights of powder exploded, thus:

TABLE II.

Number of experiment ...	2	3	1	6	4	5
Weight of powder (lbs.) ...	2100	2600	3200	4400	6200	12000
Uncorrected transit-rate (feet per second)	967·93	977·26	896·12	996·11	1173·87	1210·79

Experiment No. 1 forms the only exceptional case, and the departure is not a wide one; so that the result cannot be viewed as accidental, or due to any balancing of errors, but as the expression in so far of a fact of nature.

Nor is it due to relative differences of different experiments in the lengths of range, in the quartz rock and in the slate respectively, as might be imagined; for the experiments Nos. 2, 5, and 6 had wave-paths of about 1400 feet in quartz only, and embrace the lowest and the highest velocities, while Nos. 1, 3, and 4 had about double this range or wave-path in quartz, with velocities not widely different from each other, or from No. 2.

There are four corrections altogether applicable to the uncorrected transit-rates, col. 8, Table I., as already referred to, viz.—

1st. That for the liquid wave in the seismoscope, which, as a delay in *time*, is, when converted into distance, always +. This correction has been already applied in cols. 9 and 10, Table I.

2ndly. That for the *time* of hang-fire of each explosion in the rock; the constant in time for which has been given $=0''{\cdot}056$.

It appeared, however, uncertain whether this should be converted into distance, as probably nearly constant for every experiment, or in what way it might be variable, in relation to the weight of powder and other circumstances of each. The result disclosed in Table II., however, appears to indicate that the conversion into distance should be proportionate to the respective gross or uncorrected transit-rates, assuming, as we may now do, that these are functions of the originating impulses and resistances together, in each instance. This may not be absolutely true, but is the nearest approximation we can make. This correction in distance is also always +.

3rdly. The loss of time at making contact,—whether galvanically, in which we ascertained the constant in time to be $=0''{\cdot}0143$, when converted into distance always +; or by the hand (of the firing party), when we found it was in time $=0''{\cdot}013$, which in distance might be either + or −.

The probability being so much in favour of the latter being positive, I have ventured to apply it as always so, which also renders all the experiments more truly comparable.

4thly. The personal equations of the observer and time of transit of the galvanic current, both of which may be neglected.

Applying these several corrections, we obtain the following Table and final numerical results:—

TABLE III.—Wave-Transit Experiments. Corrected Results.

	1.	2.	3.	4.	5.
Number of experiment.	Observed rate of transit per second, uncorrected, col. 8, Tab. I.	2nd correction, for hang-fire of explosion taken in distance.	Transit-rate with 2nd correction, col. 2 + col. 10, Tab. I.	3rd correction, making contact, into distance.	Final corrected transit-rates, col. 3 + col. 4.
	feet per second.	feet per second.	feet per second.	feet per second.	feet per second.
1.	896·12	50·183	1004·551	11·649	1016·200
2.	967·93	54·204	1085·119	13·831	1098·950
3.	977·26	54·726	1095·508	13·975	1109·483
4.	1173·87	65·737	1315·908	15·260	1331·168
5.	1210·79	67·804	1357·295	15·740	1373·035
6.	996·11	55·792	1116·649	12·949	1129·598

The limits of error in these results would seem to be, that the 2nd correction *may* amount to 15·5 feet per second in excess, and the error from all other instrumental or observational sources may be estimated probably at not more than 10 feet per second, so that the results may be deemed true to within $25\frac{1}{2}$ feet per second + or −.

The general mean derivable from the whole of the experiments taken together gives

1176·407 feet per second for the transit-rate. The results, however, obviously form two groups, viz. Nos. 1, 2, 3, and 6 from the smaller charges of powder, and Nos. 4 and 5 from the greater ones.

The mean from the four first is 1088·5597 feet per second, and that from the two last is 1352·1015 feet per second; and taking a mean of means from both of these, we obtain a final result of 1220·3306 feet per second as the mean transit-velocity of propagation, in the rocks experimented on, of a wave-pulse produced by the impulse of a charge not exceeding 12,000 lbs. of powder. We may be justified in concluding that the velocity of wave-propagation (or transit) really does increase with the force of the original impulse; it would be vain, however, to attempt to deduce the law of such increase from the results before us.

The experiments of Mr. GOLDINGHAM at Madras, on the retardation of sound in moist air, and the theoretical researches of Mr. EARNSHAW, both, by analogy, rendered *à priori* probable the fact itself, now for the first time, so far as I am aware, experimentally shown.

It follows, then, on reference to my former experiments at Killiney Bay, that the rate of wave-propagation in highly stratified, contorted and foliated rock is intermediate between that for dense wet sand and for discontinuous and shattered granite. Adopting the first mean from the smaller charges of powder, as better comparable with the Killiney experiments, which were made with charges of only 25 lbs. of powder, and which would doubtless have given higher velocities with heavier charges, we obtain the following series:—

Transit-rates of Wave-propagation.

In wet sand	824·915 feet per second.
In contorted and stratified rock (quartz and slate)	1088·559 feet per second.
In discontinuous granite	1306·425 feet per second.
In more solid granite	1664·574 feet per second.

We may infer, even adopting the highest mean of these experiments, 1352·101 feet per second, for comparison with the transit-rate for discontinuous granite, and bearing in mind that the former velocity is due to the impulse originated by a *mean* charge of 9100 lbs. of powder, while the latter was due to one of but 25 lbs., that for equal originating impulses the rate of propagation of waves analogous to earthquake waves of shock must be less, generally if not always, in contorted stratified rocks than in crystalline igneous rocks analogous to granite, the amount of shattered discontinuity being the same in both.

The general mean obtained, viz. 1220·33 feet per second =13·877 statute English miles per minute, coordinates, as might be expected, with the more trustworthy of the older attempts to determine the velocity of propagation of earthquake-waves in nature*, and still more so with the more recent and exact determinations of such velocities made

* See Table 8, "Second Report on Earthquakes," &c., Report of Brit. Assoc. 1851, p. 316.

by Nöggerath*, who found it 1376 Paris feet per second; by Schmidt†, of the shock about Mincow in Hungary, and by myself in the (late) Neapolitan kingdom, after the great shock of 1857, where I found that the velocity of propagation, in the shattered limestone and argillaceous rocks of the shaken region, was even below what has been here determined for the harder and more compact rocks of Wales, also of stratified structure. Experiment and observation have thus alike sustained the three provisional conclusions anticipated by me as to the transit-velocities of earthquake waves in nature (at the conclusion of "Second Report," &c., Report of Brit. Assoc. 1851, p. 316), in passing through formations different in character.

In experimenting with these great explosions at Holyhead, I have been enabled to see that such great impulses, though offering the advantages of a greatly extended range, and hence larger total time-period for measurement, do not in reality admit, from various contingent circumstances, of greater, or perhaps of as great accuracy of transit determinations, as do much smaller explosions, such as those specially made at Killiney Bay. These great explosions, however, elicit phenomena visible in the seismoscope, which are too faint to be distinct when due to smaller charges, and which analogize closely with the succession of vibratory and wave movements observed in natural earthquakes. In the larger of these great explosions, as the impulsive wave approached the instrument, the previously steady reflected image of the cross wires did not at once disappear; the definition of the wires rapidly became obscured, the obscuration increasing for an instant to a flickering of the image, preceding its obliteration, at the same moment that the oscillation then communicated to the trough caused the mercury to sway from end to end, in a liquid wave, whose amplitude was sufficient to cause variable flashes of light to be transmitted to the eye, with the changing inclination of the reflecting-surface of the undulating mirror,—the image of the cross wires reappearing (but now oscillating with the movement impressed upon the mercury in the direction of the wave-transit) by passing through a second phase of flickering and vibration, but in the reverse order, before becoming perfect in definition as at the commencement.

I had thus presented visibly before me the "tremors" that nearly invariably are described as preceding and following the main shock and destructive surface movement in every great earthquake. The phenomena appear to be identical, however premature it may be to propose a precise and adequate explanation of their production.

There appear to be *three* elements upon which the wave-transmissive power of a rock formation mainly depends, viz. the modulus of elasticity of its material, the absolute range of its compression by a given impulse or impact, and the degree of heterogeneity and discontinuity of its parts. As has been already described, the range of wave-transit of these experiments passed through two rock formations, quartz and slate, differing in name, and in several respects in structure, yet very much alike, as has been remarked, in

* Das Erdbeben vom 29 Juli, 1846, im Rheingebiet, &c. V. Dr. Jakob Nöggerath. 4to. Bonn, 1847.

† Untersuchungen über das Erdbeben am 15 Jan. 1858. J. F. Schmidt, Astronom, Mittheilungen der Kais.-Königl. Geog. Gesellschaft, II. Jahrgang, 1858.

intimate composition. It remains to show, experimentally, that they do not differ in these conditions of transmissive power, to such an extent as materially to affect the results.

If a perfectly elastic ball be dropped upon a mass of perfectly elastic rock, whose volume may be considered as infinite with respect to that of the ball, the latter will rebound to the height from which it descended; and if the same ball, though not perfectly elastic, be dropped in succession upon like masses of two different rocks, it will rebound from each to a height less than that from which it fell, and the value of which will depend mainly upon the elasticity, the depth of the impression, and the degree of discontinuity of the rocks respectively. We have therefore thus got the means of very simply determining, in a sufficiently approximate manner, the relation between the velocity of impact and that of recoil, a quantity that bears the most intimate relation to the wave-transmissive power of rocks or other like bodies. To conduct this experiment, I dropped an ordinary ivory billiard ball upon a number of different masses of the quartz rock, and also of the slate, both *in situ*, and upon very large isolated blocks, making the impacts both transverse to the stratifications and foliation and in the same planes as these, in both sorts of rock. The ball was dropped from a constant height of 5 feet above the point of impact, and beside a graduated scale held vertically by an assistant, by means of which, after a little practice, and skill in choosing by trial a point of impact from which the ball shall rebound vertically only, it is easy to observe with considerable accuracy the height to which it recoils, the eye being gradually brought to the same level as that to which the ball rises, so as to read the scale free from parallax.

If H and h be the height from which the ball has fallen and that to which it rebounds, then

$$\frac{\sqrt{2gh}}{\sqrt{2gH}}=\frac{v}{V}=R,$$

which may be viewed as a symbol of the above relation, and closely connected with the wave-retardations respectively. In the quartz rock I obtained the following results:—

From the hardest and densest blocks or masses, and edgeways to the lamination, the ball recoiled 2·33 feet; v is therefore $=s\sqrt{h}=12{\cdot}251$ feet per second.

From the softer and more earthy masses, and transverse to the planes of lamination, the recoil was 1·50 feet, and $v=9{\cdot}822$ feet per second.

And in the slate rock,—

From the hardest and densest, edgeways to the foliation, the ball recoiled 2·00 feet, or $v=11{\cdot}341$ feet per second.

From the least hard and dense, and transverse to the planes of foliation, the recoil was 1·417 feet, and $v=9{\cdot}546$ feet per second.

The mean value for the quartz rock is thus

$$v=\frac{12{\cdot}251+9{\cdot}822}{2}=11{\cdot}036 \text{ feet per second};$$

and for the slate rock,

$$v=\frac{11{\cdot}341+9{\cdot}546}{2}=10{\cdot}443 \text{ feet per second};$$

and as H=5 feet, V=17·935 feet per second, we have

$$R_s=\frac{10\cdot443}{17\cdot935}=0\cdot576 \text{ for the slate,}$$

and

$$R_q=\frac{11\cdot036}{17\cdot935}=0\cdot558 \text{ for the quartz,}$$

numbers which differ so slightly from equality, as to indicate that there is no great difference of transmissive power in the two rocks. Indeed this is rendered certain by consideration of the experiments themselves. Previously to their commencement I expected that in every instance the range in quartz would have been extremely short in relation to that in slate, and very nearly the same in all cases. The circumstances of the works subsequently obliged me to increase the range in the quartz, and to adopt "headings" for experiment, three of which have a range in quartz of nearly double that of the other three, as seen in the two following Tables.

TABLE IV.—Shortest Ranges in Quartz.

No. of experiment.	Uncorrected transit-rate.	Range of quartz.	Range of slate.
	feet per second.	feet.	feet.
2	967·93	1600	3877
5	1210·79	1300	3738
6	996·11	1400	3829
Uncorrected mean transit-rate of Nos. 2, 5, 6.....1058·27 feet per second.			
Ratio of ranges in quartz to slate..... 1 : 2·66.			

TABLE V.—Longest Ranges in Quartz.

No. of experiment.	Uncorrected transit-rate.	Range of quartz.	Range of slate.
	feet per second.	feet.	feet.
1	896·12	2850	3733
4	1173·87	2700	3704
3	977·26	2650	3727
Uncorrected transit-rate, mean of Nos. 1, 4, 3.....1015·75 feet per second.			
Ratio of ranges in quartz to slate..... 1 : 1·32.			

In each of the two groups everything is as nearly as possible alike; there are two explosions of moderate charges, and one great explosion in each; they differ only in this, that in the first group, (Table IV.) the range in quartz, in proportion to that in slate, is very nearly double that in the latter (Table V.), being in the ratio of 2·66 : 1·32; yet, as will be observed, the mean transit-rate in both groups is almost alike, being in the ratio of 1058·27 : 1015·75. This would be obviously impossible, if either one rock or the other exercised any well-marked accelerating or retarding influence upon the transmission of the wave.

I hope shortly to be able to lay before the Society the results of some experiments upon the modulus of elasticity of perfectly solid portions of both these rocks, with

a view to the interesting question of the relation between the theoretic velocity of transmission, if the rock were all solid and homogeneous $\left(V=\sqrt{2q\,\frac{\epsilon}{\delta}},\ \epsilon \text{ being that modulus}\right)$, and the actual velocity found by the preceding experiments.

In their direct relation to Seismology the interest of the foregoing results is not as great as when some years since I commenced these experiments. At that period no knowledge whatever existed as to the relation that subsists in nature between the velocity of transit and the velocity of the particles in wave-movement in actual earthquakes. Geological observers, in fact, did not appear to be aware of any such physical distinction; and those who were so, presumed that the velocity of the particles was like that of transit, extremely great, and that some simple relation would probably be found between them.

The first determinations of velocity of the particles in wave-movement that have ever been made, namely, those by myself of the great Neapolitan Earthquake of 1857, have dissipated this notion, however, and proved that the velocity of the particles in even the greatest shocks, is extremely small, not exceeding 20 feet per second in very great earthquakes, and probably never having reached 80 feet per second in any shock that has occurred in history. No simple relation appears as yet between the transit-velocity and that of the particles; and, however interesting and important both to general physics and to Seismology may be further determinations with exactness of the former, it is to the observation and measurement of the latter, by the methods pointed out in the Report upon the Neapolitan Earthquake*, and there employed, that we must look as instruments of future seismological research.

* Now in the press. Chapman and Hall, London: 2 vols. 8vo.

XXVIII. *On the Construction of Specula of Six-feet Aperture; and a selection from the Observations of Nebulæ made with them.* By the *Earl of* Rosse, *K.P., &c., F.R.S.*

Received June 5,—Read June 20, 1861.

The period seems now to have arrived when it may be proper to lay before the Royal Society some further account of researches in sidereal astronomy, carried on with a Newtonian telescope of 6-feet clear aperture.

The observations extend over a period of about seven years, during which few favourable opportunities were lost; still in our climate, where there is so much cloudy weather, a year's work, measured by the number of hours when nebulæ can be effectively observed, is not considerable. Here in winter the finest definition we have, and the blackest sky, is usually before eleven o'clock, after which the sky becomes luminous, and the fainter details of nebulæ disappear. In spring and autumn the change is neither so early nor so decided; but the nights are shorter. Guided by Sir John Herschel's admirable Catalogue, we have examined almost all the brighter known nebulæ except a few in the neighbourhood of the pole, and a great proportion of the fainter nebulæ. No search has been made for new nebulæ; very many, however, have been found accidentally in the immediate neighbourhood of known nebulæ, but for the most part they were faint objects presenting no features of interest. In every case where any peculiarity was detected, as for instance the convolution of a spiral, dark lines, or dark spaces, a rough sketch was made, and the more remarkable objects were selected for examination on favourable nights, when the details were carefully filled in, sometimes with the aid of the micrometer. The very faint objects, and even the brighter, where there was a simple gradation of colour and no peculiarity of form, after having been examined on a tolerably good night, were rarely examined again. In our ever-varying climate, when we employ high powers and large apertures, vision is impeded more or less by the unsteadiness of the air; it is impeded also by haze; and in both respects the condition of the air varies immensely from night to night, and from hour to hour. The speculum also is not uniform in its action. With such sudden alternations of temperature, in a moist climate, it is frequently dewed, and gradually tarnishes. Artificially heating it would be a remedy; but it would be an objectionable one, and we have not employed it. From all these causes we can scarcely say that any one object has been examined under a combination of favourable circumstances; still it is not now probable that with the present instrument any remarkable additions will be made to the details of nebulæ already carefully sketched, except in very favourable states of the atmosphere. Occasionally the air is so transparent and so steady, that magnifying power may be pushed very far; and then, perhaps, something new comes out. Such opportunities, however, are rare;

and the progress made is necessarily so very slow, that I think it would be inexpedient longer to keep back this paper in the distant hope of making it in some respects more complete.

As to the instrument, a slight description of it has already been given in the 'Transactions' for 1850, but without details, and in the 'Transactions' for 1840 the process employed in the construction of specula of 3-feet aperture was fully explained; but in passing from specula of 3-feet aperture, and about twelve hundredweight, to specula of 6-feet aperture and four tons, although the same principles were our guide, difficulties were encountered which called for new contrivances and additional precautions. It will, I think, be useful to give a short account of the process by which the 6-feet specula were made, and some details as to the mounting, supplying at the same time the best answers we can to the questions, so often put, What really are the optical powers of the instrument? What are the merits and demerits of its form of mounting, after an experience of more than ten years? Would it be possible to construct a larger one, and, if so, would there be anything gained? As there seems to be a desire to employ large instruments in different parts of the world, would it be possible to lay down instructions sufficiently precise to enable a mechanical engineer, without a previous apprenticeship, to undertake the construction of large instruments as a matter of business?

About one ton and a quarter of speculum-metal can be melted in one crucible; and up to that weight there is no difficulty whatever in casting a speculum, and the instructions in the 'Transactions' for 1840 are amply sufficient to enable any engineer to do so. Each time, however, the crucible increases in circumference from the pressure of the metal, and after seven meltings we found the increase to amount to 4 inches. One ton and a quarter is therefore about the limit for a separate melting, and for larger specula we must employ several crucibles. The tin and copper must be previously combined in smaller crucibles, holding not more than three or four hundredweight, as the heat required is much greater than in the second melting. Three crucibles were employed in casting the 6-feet specula; and we proceeded thus:—

The crucibles (which had been cast by Messrs. Dewer of London, with the precautions detailed in my former paper) were placed in three separate air-furnaces upon cast-iron stands about 8 inches deep, and of somewhat larger diameter than the crucibles, to protect them from the immediate current of air passing through the fire-bars. A brick pillar from the bottom of the ash-pit relieved the furnace-bars from the weight they would otherwise have had to sustain. The furnaces were round, 4 feet diameter, and 6 feet deep to fire-grate,—constructed as an ordinary air-furnace, with a door at the ash-pit to regulate the admission of air. The three furnaces were worked by one stack. In heating a crucible, it is necessary that the temperature should be raised gradually, *beginning at the mouth*; otherwise it will be very liable to crack. To satisfy this condition, the crucibles (of course empty) having been placed on their supports, and the furnaces filled with good peat, the fires were lighted at the top; and in about *ten hours* the crucibles were of a proper temperature for the reception of the speculum-metal,

which of course was introduced gradually. In about twenty-six hours from the time the fires were lighted the metal was ready for pouring. Peat of good quality is about equal to wood in heating-power, when consumed in furnaces where there can be no accumulation of charcoal. The mould was constructed on the principles explained in my former paper; but, the scale being now so much enlarged, little matters of detail, which might have been before overlooked with impunity, were found to be of vital importance. The bed of hoop-iron was 6 feet 6 inches diameter, and 4 inches thick. We had not at the time a lathe sufficiently large to turn it; and therefore it was turned horizontally, on the machine which was to grind and polish the future speculum. To remove little irregularities arising from the imperfection of the turning-apparatus, the bed of hoops was ground for two or three days with a disc about 6 feet diameter, composed of fragments of sandstone cemented together within an iron ring. The annealing-oven was built on four arches communicating with two low chimneys. The floor being laid upon the arches, could easily be heated to redness. The interior of the oven was 8 feet by 10. For want of room, the brickwork at the ends was but 2 feet thick, the sides nearly 4. The thrust of the arch was, in the usual way, sustained by bolts. The crucibles were raised from the furnaces by a crane and tongs just as at the Mint, and placed in rings swinging on trunnions a little above the centre of gravity of the mass. The metal being of a proper temperature, levers were fixed upon the trunnions, and at a signal the crucibles were simultaneously inverted as rapidly as possible. The operation of pouring was accomplished in about three seconds. If the metal was not poured rapidly, the conducting-power of the iron surface is so great that partial solidification would take place, and the casting would be imperfect. In about twenty minutes the metal was solid throughout; the frame containing the sand forming the sides of the mould was then removed, and the speculum, being grasped by an iron ring, was drawn into its place by a capstan. The temperature of the oven was red, just perceptible in the dark, about 900°. All the apertures were then closed; and in about six weeks the speculum was cool. When removed from the oven the speculum was found perfect; but the radius of curvature was much longer than it should have been, which rendered the grinding a very tedious operation. The cause, however, was obvious; the floor of the oven had been laid carefully flat to prevent warping; no other precaution had been taken; indeed, no other had been necessary with the 3-feet specula.

The speculum was removed from the oven to the bed of supporting levers in the following manner:—A pit was dug about 4 feet deep, near the oven, commanded by a crane. The speculum, weighing about four tons, was drawn out of the oven in the same way that it had been drawn into it. Planks were provided for the speculum to slide upon to the edge of the pit, into which it was lowered gently, the ring still grasping it. The speculum was now resting principally upon its edge, the face supported by the side of the pit. By means of *wooden* handspikes, and with little effort, the speculum was made to rest entirely on its edge, bearing upon the soft earth. Two bars, 7 feet long each, and 2 inches square, one of them cranked in the centre, were placed against the

back in the shape of a cross. To prevent metallic contact, the bars had been bound round with woollen cloth. Strong planks were placed against the face, and screw bolts were passed through the planks and projecting ends of the bars. The speculum was thus encased, and was easily raised by the crane *face up*. In the mean time a strong wooden platform had been made with three iron pillars securely fixed in it, about 2 feet long each, and so disposed as to support the speculum with the least strain. The frame carrying the supporting levers, to be hereafter described (Plate XXIV. fig. 1), was placed upon this platform, the three iron pillars passing through interstices in the levers; and the speculum was lowered till it rested upon the pillars, the levers being considerably below it. The bars and planks encasing the speculum were then removed, and the frame and levers raised by the crane till the speculum was completely supported by them. It now rested on its levers, and was taken to the grinding-machine. I have been thus minute in describing the means we had employed in removing the speculum from the oven, turning it over, and placing it on its bed of levers, as in the arts they have never to deal with a material at once so heavy and so brittle; and we were guided by long experience, which others may not have had.

This speculum had been more than a month upon the grinding-machine, and was just ready to be polished, when it was broken by an accident. Immediate preparations were made for recasting it. While the speculum had been in the annealing-oven we had finished a powerful lathe for turning the grinding-tools, with a slide-rest moving in the proper curve. The bed of hoops was placed upon that lathe, and its radius of curvature adjusted: the floor of the oven also was cut roughly to the same curve. As we were anxious to guard, as far as possible, against contingencies, and to secure a working speculum with the least delay, we were satisfied to employ an alloy somewhat lower than on the former occasion, and an ingot of speculum-metal was added which contained more than the proper proportion of copper. A little additional copper diminishes the brittleness considerably, while it increases the liability to tarnish.

The speculum was successfully cast, but the surface was covered with minute fissures, about the breadth of a horse-hair. These we resolved to grind out. The grinding was very tedious, partly owing to the metal being a little below standard, and partly to the deepness of the fissures. After the first day's grinding, the fissures, which previously were scarcely perceptible, became much enlarged, owing to the edges chipping away; and the whole surface thus became, as it were, covered with large wrinkles. The process of abrasion is necessarily extremely slow, as both the velocity and the pressure are kept within very narrow limits, to prevent the evolution of heat, which would crack the speculum. The grinding continued for nearly two months, the machinery working for part of the time at night; and a few of the fissures were so deep that even then the traces of them were perceptible. The speculum was then polished; and its performance fully equalled our expectations.

A telescope intended to be constantly employed requires two specula. We had now leisure to encounter delays and difficulties in endeavouring to procure a second speculum

free from the defects of the one already finished. We had satisfied ourselves that the fissures were owing to our having employed the bed of hoop-iron in the state in which it was when taken from the turning-lathe. The surface, though nicely turned, was not as smooth as the surface of a solid disc would have been: a slight yielding at the edges of the hoop-iron, and a slight spreading under the pressure of the tool, had produced little irregularities; and although the surface had been carefully "black-washed," the speculum-metal had encountered too much friction in the act of contracting after it had become nearly solid, and thus had been filled with superficial rents. On the first occasion there had been no fissures, but the bed of hoop-iron had been ground; the remedy was therefore obvious.

The third speculum was successfully cast; but on opening a small aperture, and looking into the oven before it was quite cold, it was observed that the speculum was cracked through the middle. The temperature of the speculum was found not to be quite uniform; and that circumstance, taken in connexion with the direction of the crack, seemed to point out the cause: the ends of the oven, from want of room, had been made thinner than the sides. The first speculum had probably been strained by the same cause, and rendered more fragile.

The oven being ready, an attempt was made to cast a fourth speculum, which failed. We had each time, before the bed of hoop-iron became cold, saturated it with tallow to prevent the formation of rust between the hoops, which would have rendered the surface impervious to air; but just before it was again employed it was made red-hot, and the tallow burned out. On this occasion, by an oversight, the bed of hoop-iron had not been sufficiently heated, and there remained some of the tallow unconsumed, which, being vaporized in large quantities, produced an ebullition which made the casting as porous as pumice-stone. This speculum, of course, was not annealed, and the following day it was in small fragments.

The fifth speculum, being in every respect a perfect casting, without the slightest blemish and of a proper curvature, was ground and polished in about a month. It is desirable that the bed of hoop-iron when the metal is poured should be warm, so as to prevent the possible deposition of moisture; but if much hotter than this, it at once dries up the sand, and it is difficult to make the mould secure. In the whole of the operation I have described, one of the difficulties is to time each stage. If the mould was prepared too long, the bed of hoop-iron might become cold and damp; on the other hand, if the mould was not ready when required, it might be hazardous to keep the crucibles so long at the pouring-heat. It may, perhaps, be as well to add that the crucibles, when in the pouring-gimbals, require to be thoroughly skimmed, as particles of coal falling upon the hoop-iron would be immediately entangled in metal not rising to the top: the skimming should be done promptly, lest the metal fall below the proper temperature. Any considerable delay in drawing the speculum, when solid, into the annealing-oven would be fatal; therefore there should be ample capstan power to overcome the difficulty which usually is experienced in detaching the speculum from the mould.

The last speculum is but 3½ tons, and is therefore considerably weaker than its predecessor; and by carefully comparing the two specula at low altitudes, we have been made thoroughly sensible of the great importance of strength in preventing flexure. There are little irregularities in the action of the supporting levers, which are much more injurious to the definition of the weaker speculum than the other; and although these irregularities may be susceptible of further diminution, I think there would still be sufficient gain to make it worth while to cast a third speculum considerably heavier than either of the others.

In the 'Philosophical Transactions' for 1840 I have endeavoured to explain the principle upon which the bed of hoop-iron acts; some, however, seem to have attributed larger effects to it than I have, and of a different kind. It has been supposed by some that a molecular change takes place, somewhat similar to that which has been observed in the case of very small portions of speculum-metal rapidly cooled, while by others the change has been compared to the "chilling of cast iron," to which I think it bears no analogy: cast iron when chilled becomes almost as hard as hardened steel; there is an exudation of graphite—in fact, a chemical change the exact nature of which seems to be imperfectly understood: there is no such change in speculum-metal, it becomes actually softer. To obtain sound castings, all which seems necessary is so to manage the process that solidification must begin at one surface and proceed regularly to the other. By employing the bed of hoop-iron the object is effected with certainty; but the engineer may employ other means, perhaps sufficient for the purpose, which, under varying local circumstances, may be cheaper and more convenient.

Possibly some useful hints may be gathered from a slight glance at the successive steps by which we obtained a clear view of the principle by which the founder should be guided in making large castings of speculum-metal.

About the year 1827, on commencing a series of experiments on speculum-metal, I procured a small flat speculum from Mr. Tully, and two similar specula from Mr. Cuthbert, as specimens of the art in its most advanced state. I also procured from Mr. Cuthbert several small unwrought castings of about two ounces weight to practice upon. Mr. Tully's specula were cast in the ordinary way in sand, and polished with rouge: but Mr. Cuthbert's were cast in contact with iron, and so cooled instantaneously; they were polished with putty. All the specula for Mr. Cuthbert's microscopes were made in a similar manner. He was under the impression that speculum-metal cooled instantaneously was more suitable for his purpose than common speculum-metal—that it was sounder, more compact, and resisted better the action of emery. These specula were accidentally exposed to the air of the laboratory for a considerable time; and at length we remarked that Mr. Cuthbert's specula had somewhat lost their polish, while Tully's speculum was as bright as ever. The inferiority of Cuthbert's specula we attributed to an excess of copper, but with further experience we came to a different conclusion. We had several early samples of chilled speculum-metal, and corresponding samples of the same metal cooled gradually. They were obtained in this

way:—In our experiments on the alloys of tin and copper, we were in the habit of taking out a sample after each addition of tin. When cool, a small piece was broken off the sample, and the fracture and colour examined; the remainder was then hastily ground and polished on a succession of revolving laps. The experiments were very numerous; and to save the time lost while the sample was cooling, we at first applied water cautiously, and then adopted the device of pouring the sample into a ring laid upon the face of an anvil. Samples of a few ounces weight frequently cracked upon the anvil, but with water they usually flew into many pieces. We soon, however, found that the attempt to save time by cooling the samples instantaneously was a step in the wrong direction, as it was only from samples cooled slowly that reliable information could be obtained as to the qualities of the future casting. There was, however, this result, that we at length came to the conclusion that instantaneous cooling was unfavourable to permanence of polish. In the progress of these experiments we also observed that the rods of speculum-metal formed in the air-holes of damp sand-moulds, also the thin plates formed at the junction of the upper and lower moulds, were of unusual strength. We were not then aware of the fact that alloys of tin and copper are softened by sudden cooling, which would have accounted for the liability to tarnish, and the great increase of strength.

Mr. Potter, in Sir David Brewster's 'Journal' for 1831, directs attention to the *apparent* hardness and soundness of speculum-metal cooled instantaneously; but he does not appear to have operated upon a larger scale than Mr. Cuthbert, his castings not exceeding 1½ ounce. Mr. M'Cullagh seems also to have noticed the same facts; and, indeed, it is not likely they could have been unnoticed by any one who had been engaged much in speculum-casting; but the obvious fact that any considerable mass of an alloy with such large expansions and contractions as speculum-metal, and so brittle, must fall to pieces if cooled rapidly, would have forbidden the attempt to manufacture large telescopes with such a material.

In all our earlier experiments the castings were made in damp sand, precisely as in the common process of casting iron or brass. Where the founder, however, aims at the best results, especially in brass-casting, he dries the mould: he thus escapes the mischief sometimes arising from the evolution of hydrogen, which, unlike steam, makes its way through the sand with difficulty. Steam in small quantity does no mischief, because it enters the interstices of the sand, where it is immediately condensed.

In the hopes of better results we dried the moulds; but, strange to say, the castings were less perfect. At the low temperature at which specula are cast, the tin acts but very little on water, and there is no injurious evolution of hydrogen; therefore, in that respect, there was nothing gained by drying the mould, while we found, after a great number of specula had been broken up, that in dry sand the progress of solidification had been less regular than in damp sand, and that this was owing to the circumstance that in dry sand the solidification had commenced irregularly in all directions, while in damp sand the upper surface had remained longer fluid than the lower surface,

especially where the specula were of considerable thickness. To explain this, it is only necessary to remark that, when metal enters the damp-sand mould, heat will be immediately abstracted from it; and as it rises in the mould by each successive addition of hot metal, it will somewhat dry the upper sand-surface before the metal reaches it. When, therefore, the mould is full, the lower surface will be cooler than the upper: this does not happen when the mould is dry. In casting very thin specula, there is no time for successive actions; the upper and lower surfaces solidify simultaneously, and there is a tendency to separation in an intermediate plane. In some cases the separation was so complete, that a slight concussion actually divided the speculum into two discs. In discs of brass there is often, from the same cause, a very thin plane of porous metal running through the centre; and where this occurs in the plate of an air-pump, a bouching is inserted to cut off the communication between the external air and the central aperture. Keeping these facts in view, it would be naturally expected that by employing very open sand, as damp as possible, for the lower surface of the mould, and dry sand for the remainder, the best results would be obtained: and such was the case; and where other means are not at hand, specula of 10 or 12 inches diameter can thus be easily obtained, provided they are of considerable thickness. This device, however, was not successful when we endeavoured to procure thin plates to face the compound speculum described in the 'Transactions' for 1840; the solidification of the upper and lower surface was too nearly simultaneous, and therefore there was irregular contraction: consequently a metallic surface was employed, from which the plate was removed the moment it was solid.

There is yet another method of procuring excellent specula of moderate dimensions, which was dismissed, perhaps, in too summary a manner in the account of experiments in the 'Transactions' for 1840. It has this to recommend it, that it can be carried out by persons who have had no experience in the management of melted metal; and it is desirable to smooth the way, as far as possible, for beginners, who may, perhaps, by early success, be induced to proceed further. A cast-iron mould can easily be made at any foundry; it must be at least two and a half times as deep as the required speculum; it is to be placed in a temporary air-furnace, resting, like a muffle, upon two very strong deep bars, and is to be made perfectly level. The grate should be made of moveable bars, which can be withdrawn at the conclusion of the process, so that the fire may fall into the ash-pit. If charcoal is employed, the draught will be sufficient to produce the necessary heat without a chimney. The proper quantity of speculum-metal, in pieces, is then introduced, and the cover put on. It is important that no pieces of charcoal should get in before the metal is melted, as they will often be found in the face of the speculum. As soon as the metal is melted, the cover is taken off, and the moveable bars are drawn out. The metal is then stirred with a broad flat tool, passed everywhere over the surface of the mould to detach air-bubbles, and without loss of time a jet of water is thrown against the bottom of the mould, through a rose with exceedingly small holes, and distributed evenly. The action of the water must be suspended the

moment the temperature of the mould is reduced to a dark red, lest it should crack; but the operation should be repeated at intervals of a few seconds, to keep the reduction of temperature permanent. As soon as the metal is solid at the surface, the furnace is to be closed up completely, and the speculum is thus annealed, the furnace acting as an annealing-oven. The blocks of speculum-metal, which were sawn up into plates, as described in the 'Transactions' for 1840, were made in this way, excepting that air was employed instead of water. A large hand-fan furnishes a sufficient blast; and when such an instrument is within reach, air is perhaps preferable to water, as it is more easily managed. The cracking of the moulds (the difficulty we encountered in the experiments alluded to) we subsequently ascertained was owing to excess of water. It is important that the temperature of the metal should not pass the melting-point, to prevent the large development of a crystalline structure.

Whether specula are cast according to the first process, when the moulds are of damp sand and solidification commences at the lower surface in the way I have explained—or by the second process, when the moulds are of iron and solidification also commences at the lower surface, owing to the action of some cooling medium, through the iron—or by the third process, when the same effect is produced by the exterior mass of iron which prevents the interior surface of the mould from attaining the temperature of fusing speculum-metal, the result is very similar: there is the same molecular arrangement more or less developed, and the fracture presents the same characters: the axes of the crystals are directed to the cooling surface, in obedience to the general law, stated by Mr. Mallet in the following words, that when the particles of crystalline solids* "consolidate under the influence of heat in motion, their crystals arrange and group themselves with their principal axes in lines perpendicular to the cooling or heating surfaces of the solid—that is, in the lines of direction of the heat-wave in motion, which is the direction of least pressure within the mass."

It is scarcely necessary to add that there is no resemblance in this molecular arrangement to that of a small speculum cooled instantaneously.

Enough has now been said to enable a skilful founder to follow the course which we pursued in casting specula of 6-feet aperture. The principles are, in fact, the same which he must have had in view in executing works of unusual difficulty in cast iron: with speculum-metal, however, the difficulties are far greater, and therefore every part of the operation must be more rigorously governed by sound principles.

The photograph of the speculum on its supporting levers (Plate XXIV. fig. 1) will give perhaps all the information which may be required as to the general nature of the arrangement. The ring in which the speculum is suspended was removed, as also some of the apparatus connected with the levers, to prevent confusion. The diagram fig. 2 represents in plan the arrangement of the levers. The cast-iron carriage, of about 1½ ton weight, carries three ball-and-socket joints, directly under the centre of gravity of three equal sectors, into which the speculum may be supposed to be divided. The

* Mallet 'On the Construction of Artillery,' p. 7.

centre of the ball is in the centre of gravity of the triangle, not merely as respects its plane, but thickness also. These three triangles, which we call primary, carry at their angles, by ball-and-socket joints, nine secondary triangles, supported at their respective centres of gravity: and they, in a similar way, carry twenty-seven tertiary triangles, each carrying three gun-metal balls of 1½ inch diameter,—in all, eighty-one balls, which support twenty-seven equal portions of the speculum. Between the balls and the speculum twenty-seven thin brass plates are interposed, attached to the speculum by pitched cloth, not so much with a view of giving support between the balls, which would probably be quite unnecessary, but to make a smooth surface for the balls to roll upon without grinding the back of the speculum true. In each ball there is a small hole, and a thin brass wire is inserted in it, and secured with a wooden peg; this wire passes through a small hole in the lever, and is attached to a thin brass spring at the back, which yields as the ball rolls, and brings it back to its proper place whenever the ball is free from pressure. Without this contrivance, a very slight jerk, when the plane of the speculum is nearly vertical, would cause the balls to fall from their places. In practice, the motion of the balls is of course very slight.

It is evident that so long as the speculum is horizontal, equal portions are carried equably, and it is almost as free from strain as if it was floating on mercury. As soon, however, as we incline the speculum to the horizon, the lever apparatus does not act so perfectly. It will have been observed that the levers are not in the same plane; and this is disadvantageous in two ways: first, although the primary triangles balance in every position on the ball-and-socket joints, and therefore are indifferent as to position, the centres of the ball-and-socket joints carrying the secondary triangles are unavoidably above the plane of the centres of the primary supporting balls, and still more are the ball-and-socket joints carrying the tertiary triangles; consequently the secondary and tertiary triangles, by their weight, exert a force tending to make the planes of the primary triangles rotate in a vertical plane, and so disturb the equilibrium. The tertiary triangles in a similar manner, but to a much less injurious extent, act upon the secondary triangles. The lever apparatus, deducting the primary triangles, weighs about 600 lbs.; were it lighter it would not have the necessary solidity; and the disturbing action of the weight is so considerable that, when not counterbalanced by subsidiary contrivances, the action of the speculum at low altitudes is much impaired by it. The contrivance we employ is a system of levers, connected by wires with the ball-and-socket joints which support the secondary triangles, and acting at right angles to the plane of the speculum. The primary triangles, thus relieved from all lateral strain, are in a condition to do their duty effectively; and that seems to be sufficient in practice. Of course another set of levers might be attached to the ball-and-socket joints supporting the tertiary triangles; and then the whole system would be perfectly counterpoised in every position; that, however, seems to be scarcely necessary. It is evident that, were it not for the balls interposed between the levers and the speculum, any lateral motion of the speculum would introduce a disturbing force which would destroy the equi-

librium. Lateral motion, however, must always exist in the different positions of the telescope, owing to the elasticity of materials; and it must act injuriously in some degree, in proportion to the force required to move the speculum on the balls. Great care has been taken to make the fittings of the ring in which the speculum hangs as perfect as possible, and to connect its joints and bearings with the iron carriage, so as to reduce the lateral motion of the speculum to the smallest possible quantity. We have not tried Mr. LASSEL'S ingenious arrangement for relieving the edge pressure. Unless there were holes half through the speculum the experiment could not be fairly tried. Our 3-feet specula are also suspended in a ring, and are supported on fewer points by the lever apparatus, which, at a slight sacrifice of theoretical accuracy, has been thrown into one plane. We have rarely perceived any flexure of importance, except where the action of the levers had been impeded by rust; but the 3-feet specula, which weigh about thirteen hundredweight, are very much stiffer than the 6-feet specula, as is obvious on common mechanical principles. Upon the whole, I am inclined to think there is a better prospect of improving the definition of very large specula by increasing the original stiffness, than by endeavouring still further to eliminate slight disturbing forces.

The 6-feet specula were ground with an iron tool, divided into squares, precisely as the 3-feet specula. The squares were larger, about 2 inches each side, and were not formed by cutting but by casting. A tool cast from the same pattern was employed as a polisher, but the surface of the squares was cut up by turning, so as to leave no more than half an inch of continuous surface. The tools weigh about one ton one hundredweight each, and the iron is so disposed in them as to produce the utmost amount of stiffness. The photograph supplies all details (Plate XXIV. figs. 3 & 4).

In grinding, about two-thirds of the weight is at first taken off by a counterpoise acting through a system of levers attached to it in thirty-six points, on the same principle as the levers which support the speculum. As the process proceeds the contact becomes more general, the friction increases, and there is more heat developed; therefore the counterpoise is increased, till towards the conclusion the unbalanced weight of the grinder is reduced to about two hundredweight. Notwithstanding the great strength of the tool, we found that if after the grinding was over it was suspended by its centre, the flexure, after a week or two, became so great that on replacing it on the speculum and regrinding with it, the action commenced at the edge; it is therefore always, when not in use, suspended by its levers. The curvature of this tool was adjusted in the ordinary way by gauges. These, as they were to be employed in the adjustment of the speculum to focus, were made with great care. One side of each gauge was first made into a straight edge, by the well-known scraping process of Mr. WHITWORTH, and the two were then very slightly ground for a few minutes with fine emery to remove the marks of the scraper, but no more. Ordinates were then set out, an inch apart, and marked to the calculated length by means of an instrument applied to the straight edge, very similar to the joiner's gauge, but made of brass, with a fine scale and vernier. Through the extremities of the ordinates the curve was traced by means of a steel point, guided by a

curved rule about 6 inches long. A pair of these little rules were made from calculated ordinates, and ground together. In adjusting the gauges to the curve so traced, nothing was employed but the file and scraper. The gauges were then slightly ground together with the finest emery and in very small quantity, and care was taken to distribute it evenly with a camel's-hair pencil. The grinding-tool was from time to time adjusted roughly to the curvature on a turning-lathe, which was accomplished with great facility, as the slide-rest was governed by a guide of the same curvature as the gauge, and there were adjusting-screws in the face-plate, by which the tool was made to run perfectly true each time it was replaced. When the curvature of the speculum was nearly exact, the remaining little changes in the radius of the tool were made with the file. We had no means of optically measuring the focus of the 6-feet mirrors while on the engine; therefore further precautions were taken. A brass wedge was made, about 3 inches long, at one end $\frac{1}{100}$th less than the vers sine of the circle of curvature, and at the other end $\frac{1}{100}$th greater. This wedge was cut into three parts, equal, greater, and less than the vers sine. A straight edge was then laid upon the speculum; and it was considered perfect when one of these just touched the straight edge, another passed under it without touching, and the third did not pass at all. So accurately was the adjustment as to focus made in this way, that neither of the specula differed from the calculated focus more than $1\frac{1}{2}$ inch.

The machinery is precisely on the same principle as that which we employ in working 3-feet specula, already described in the 'Transactions.' Instead of belts there are chains, working in V grooves. The driving-wheels are of wood, in several layers, the grain being disposed radially. The chains are made tight by straining-pulleys, and act perfectly. This species of machinery is neither compact nor elegant: when originally designed, its principal recommendation was facility of construction and facility of alteration, both important qualities where it was doubtful whether machinery unguided by hand, acting independently of the sense of touch, would answer at all. Some are surprised that machinery so rude should be employed, and successfully, in a mechanical operation where the utmost precision is required, a precision almost fabulous, and they compare it with the beautiful machinery in the mills where textile fabrics are made: but in figuring specula everything depends upon the principle; and so long as certain motions are communicated to the tool and speculum, machinery can do no more. The tool is raised and lowered by a screw passing through a carriage which moves upon a railway over-head upon the principle of the travelling crane, and the same mechanical arrangement removes the speculum with its lever supports from the grinding-machine to the truck upon which it is conveyed to the telescope. The screw is obviously the best mechanical power to employ, as its action begins and ends slowly, and there is therefore less danger of breaking the speculum. As a further precaution in raising or lowering the grinding-tool, thin wooden wedges between the tool and speculum are gradually introduced and withdrawn. In the final adjustment of the speculum to focus, the operation is much facilitated by a judicious management of the second

eccentric; small variations in the radius of curvature are thus produced with great facility: where, however, there has been a considerable departure from the length of stroke necessary to produce a spherical figure, the speculum requires to be ground for twelve hours, or perhaps much longer, with the proper motions, before it is fit to be polished. Mr. Whitworth is of opinion that greater general accuracy of surface is obtainable by scraping than by grinding: the late Mr. A. Ross, as high an authority as any one in everything relating to practical optics, held very nearly the same language*. He attributed the defective action of the grinding-process to the unequal distribution of the grinding-powder, which, accumulating at the centre by capillary attraction, and at the edges by mechanical action, unduly shortened the radius of curvature at the centre and lengthened it at the edge. He employed the grinding-powder dry in producing flat glass surfaces, and believed he thus obtained a better result. It appears to me that by cutting up one of the surfaces into minute squares, in the way we have so long practised, the causes of unequal action are eliminated. The subject is a very important one, as there appears to be no other probable means of working solid materials into accurate surfaces for optical purposes than by some modification of the ordinary process of grinding and polishing. In the 'Philosophical Transactions' for 1840, there is a sketch of a grinding-tool such as we employed in the construction of 3-feet specula; but I have scarcely noticed experiments on grinding, passing at once to the more important subject (as it appeared to me at the time), that of polishing. Something useful, however, may perhaps be extracted from our record of very numerous experiments on grinding plane and curved surfaces. In my very early experiments, the ordinary process for procuring a true plane by grinding three planes in alternate pairs was often repeated. Till we adopted the expedient of cutting up two of the surfaces into minute squares, our success was very limited. That device apparently removed all the difficulty, and we were then enabled to make large flat mirrors which bore the optical test well in *every part*, which was not the case before. When one of the surfaces is divided into minute portions, with sufficiently large and deep intervals, there can be no capillary action such as that described by Mr. A. Ross; neither can there be an accumulation of grinding-powder anywhere, because an immediate escape for it is provided; and if the grinding-powder is employed in very small quantities, and no addition is made to it for three or four hours before the termination of the process, there will be a high degree of comminution, and only just a sufficient number of minute particles to keep up the abrading action, probably nowhere more than a single layer. We may form some idea of the accuracy thus attainable by examining with a microscope the particles of emery so comminuted. No parts can be acted upon strongly except where they deviate from a spherical surface; too violent contact then, and consequently a destructive action, is prevented by the moisture interposed. Under such circumstances, with unyielding surfaces, *time* obviously cannot enter as a disturbing element, because there is no abrasion when there is no close contact.

* Holtzapffel, Mechanical Manipulation, vol. iii. p. 1220.

The principle of Mr. WHITWORTH's method may obviously be carried out, with glass or speculum-metal, by employing small laps and grinding-powders instead of the scraper; but as a scraped surface consists of a maze of curves of varying flexure, a surface ground in detail must always, I should think, in some degree partake of the same character, and, though it may not anywhere deviate much in general outline from the required form, minute deviations must exist in every part. M. FOUCAULT seems to have been successful in improving surfaces of moderate dimensions, by his ingenious process of testing and polishing in detail; how far such a process will succeed in improving large surfaces which have been in the first instance properly wrought, has not, as far as I am aware of, been ascertained. Our practice always has been to repolish when the surface, tested by the method described in the 'Transactions' for 1840, has proved to be defective. If a few glaring defects are at once seen, the whole surface is always faulty, though in a less degree.

The only change we have made in the polishing-machinery consists in substituting an elliptic for a circular wheel in driving the second eccentric. The major axis is at right angles to the throw of the eccentric, and is to the minor axis as three to one. The band is merely a rope working in a deep groove; and a straining pulley, freely acted upon by a weight, secures the necessary tightness in all positions of the ellipse. The obvious effect of this arrangement is to diminish the time the polisher overhangs the speculum, and so to reduce, to some extent at least, a source of error. We now employ in every case a separate tool for grinding and polishing, which is a great convenience, especially as we always regrind the speculum after it has been brought in from the telescope. There seems to be no doubt that in some cases considerable change of figure had taken place. The grinding-tool, when true, will be bronzed all over, and the speculum, when examined in every position as to light, will appear uniform.

We still consider the process of polishing described in detail in the 'Transactions' for 1840 as the best, with this addition, that we employ a combination of brown soap and ammonia, instead of pure water, during the latter part of the operation. We had then tried this lubricating mixture but too recently to feel justified in recommending it. The great objection, however, to the whole process is the difficulty of carrying it out. I have had communications from time to time from many persons in whose hands it has failed; and I am not surprised; for although everything has usually gone on smoothly when we were in the midst of experiments and in constant practice, yet after the lapse of even one year, when we have had occasion to repolish a speculum, there have been often disappointments. The difficulty arises from the necessity of employing two strata of resinous matter, one so hard, and both so thin. If in preparing the polisher the hard resinous composition is suffered anywhere to come in contact with the iron, the polisher will not retain its figure, and there will be a failure. A small chip of wood in the pitch will produce the same effect. If the water or lubricating mixture is supplied too sparingly, the polisher will begin to dry in spots, the rouge and abraded matter will collect there, and the thin stratum of pitch will be compressed till the accumulated

matter resting upon the iron will act just as a chip of wood. If the lubricating fluid is a little in excess the rouge will run loose, the very hard resinous surface being able to retain but a very small quantity of it, and the incipient polish will disappear. An excess of rouge acts in the same way, while, if the rouge is not in sufficient quantity to keep up the cutting-action, the surface of the speculum loses its truth. The process therefore requires great attention throughout. Both the temperature of the water in which the speculum revolves and the temperature of the room, of course, must be properly regulated. The process does not proceed well unless the moisture between the speculum and polisher gently evaporates, so that drops of fresh fluid may be added from time to time, to carry away the undue collection of abraded matter. As the hygrometric state of the air varies, so will the quantity of fluid required to lubricate the surface; and that would be a source of considerable embarrassment, were it not that in dry states of the air the dew-point can be adjusted by a jet of steam. When the air is very damp we have no practical remedy; and therefore the operation is not then attempted.

We have often endeavoured to evade these difficulties by employing a surface less hard, supported by a thicker substratum of pitch; but there has been an evident sacrifice of truth of surface and figure, and we have failed in obtaining that very fine definition which resulted from the old process when perfectly successful. By the old process, a speculum of 3-feet aperture and 27-feet focus has been frequently made so perfect that in favourable states of the air it has defined sharply the dots and figures on a watch-dial distant 100 feet, the eye-glass being a single lens of one-eighth of an inch focus: such a speculum in ordinary weather perhaps does little more than one that is inferior to it, both, for instance, showing well the sixth star in the trapezium of Orion; but in extremely fine nights it displays its powers by resolving nebulæ in which no traces of resolution had been seen before, and by concentrating the light of minute stars and so rendering them visible.

If the vivid polish of a speculum employed in the *open air* was as enduring as that of glass, the difficulty of the process and its uncertainty without continual practice would have been no great objection to it; but when, on the contrary, it is necessary to repeat the process at intervals perhaps so long that minute details are not fresh in the memory, the task becomes the labour of Sisyphus.

A very fine speculum loses much of its light and some of its truth of surface by being repeatedly dewed, especially if it has been several times cleaned, and for the ordinary work we are engaged in will be inferior to a moderately good speculum which is quite fresh.

The preparation of a polisher in the way formerly described is one of the great difficulties; a certain degree of manual dexterity is required, which can only be obtained by practice and kept up by practice. For many years we have very often prepared it in an easier way; some pitch of the proper consistence for polishing at 55° is put into warm water; and when soft, a little is taken out and rolled upon a wet board to the proper thickness. There is no difficulty in this, as the roller is governed by ledges of

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proper height at each side of the board. The surface of the pitch is wiped dry, and a thin stratum of the hard resinous composition in powder is sifted on. A large flat-iron, red-hot, is then passed over at a distance of 3 or 4 inches, and so slowly as just to fuse the resinous powder without making any change in its composition. The pitch, so prepared, is cut into squares of the proper size, and thrown into cold water till required. The polisher, warmed to about 80°, is then brushed over with very soft pitch, and, when the temperature has fallen to about 65°, the square pieces of pitch are arranged in their places and soon become quite fast. The whole of this operation requires little experience, and can be managed by common workmen—a great advantage. It has, however, this disadvantage, that the pitch is somewhat thicker than we should wish. In the 6-feet polisher the squares are 2½ inches; and although the soft pitch in the circular grooves will no doubt yield a little, still we have a larger continuous surface than we had by the original process, and therefore the pitch requires to be thicker. The reason why with the long transverse strokes the pitch must be so thin is evidently this, that the polisher passes so far beyond the edge of the speculum. If we coat a polisher with pitch alone and of some thickness in the ordinary way, and then proceed to polish, we shall find that, if at any time we stop the machinery for a few moments when the polisher is at the extremity of the stroke, the pitch will change its figure. The change of course will be less as the stroke is shorter; but by prolonging the time, even with a very short stroke it will still be perceptible. So considerable is the change of figure under such circumstances, that after some time a distinct mark will be made by the edge of the speculum, and the projecting portions of the pitch becoming comparatively protuberant, unusual force will be required to effect the next stroke. These continual changes of figure, slight as they may be, will produce excessive action on the outer portions of the speculum. To meet this evil, if we diminish the length of the strokes much, we impair the self-correcting action to which we are mainly indebted for success. To explain this, let us suppose the throw of the first eccentric, B (see figure in 'Transactions' for 1840), to be reduced to a small quantity, and the action of the second eccentric, G, to be reduced in the same proportion, the speculum continuing to rotate; if the polisher and speculum are not truly portions of the same sphere, there will be unequal action at the centre or edge of the speculum, according as the polisher is more or less convex than it ought to be. In the first case, a depression will be formed at the centre of the speculum of a diameter proportional to the throw of the eccentrics; in the second case there will be an annular depression at the edge of the speculum. It is plain that the speculum cannot be restored to truth till the remainder of the surface has been lowered to the depth of the depression: this, however, will not be accomplished in practice if unequal action is continued even for a very short time. It may be thought that rigid identity of figure might be secured in the first instance; but this is practically impossible: the rouge cannot be distributed with perfect regularity; besides, as the temperature of the polisher varies, so does its radius of curvature. But even if perfect coincidence was secured at the beginning, it would not long continue. With very small motions the

abraded matter would not be equally distributed, and, collecting in excess in some place, unequal action would be set up before the pitch had time to yield. If the excess was not at the centre, the depression would assume the character of a ring. The pitch at length yielding, the ring would not increase, but it would continue, and, a similar cause arising in another part of the polisher, a second ring would be formed, and so on. I have seen a surface of an annular character all over, the breadth of the rings depending on the adjustments of the eccentrics. Why the depressions once formed continue with so much persistency is evidently owing to the yielding character of the pitch, which, when the depression is of large area, becomes protuberant, precisely as it does where it overhangs the speculum, and so the cutting action is to some extent continued. An annular surface is produced by grinding, under similar circumstances, but the rings change their places frequently. The annular surface is always best-marked when the action of the second eccentric is suspended completely. To see the annular surface, the speculum must be slightly polished by rubbing it all over for a few minutes with a small lap covered with soft pitch and rouge. I need, perhaps, hardly add that the character of these surfaces can only be seen when they are examined by the light reflected from the watch-dial, in the way described in the 'Transactions' for 1840. As the throw of the eccentrics is increased the rings gradually disappear; and when they reach the proper positions the surface becomes quite uniform. We have often found it very useful, when the figure of the polisher was not satisfactory, to throw another movement into gear connected with the guide D, by which an occasional stroke was given of increased length: the cause of unequal action is thus immediately removed if it does not arise from some defect in the construction of the polisher, such as the contact of some unyielding substance with the iron. The experiments I have just referred to were of a very early date, but they were numerous and made with great care; I have therefore not thought it necessary to repeat them.

In the first polishing-machine we made, the polisher was connected with the eccentric B by means of a rigid bar passing through the guide D, the guide being furnished with an adjustment at right angles to the line joining the centres of the speculum and eccentric. The guide was equidistant from the centres of the eccentric and polisher, and the path of a point in the polisher was similar to that of the crank-pin of the eccentric. We found, however, that when the movements were very small the surfaces both in grinding and polishing became somewhat annular, and when the movements were large the figure was spoiled. We therefore substituted a jointed rod for the rigid bar, and added the second eccentric. From time to time we have returned to the rigid bar, tempted by its obvious advantages, and hoping in some degree to free it from its defects. It is an advantage that with it the movement of a point in the polisher is as the circumference of a circle, while with the jointed bar under similar circumstances it is as the diameter. In the one case there will be more than three times the amount of motion there will be in the other, and the polisher will overhang the speculum but for a moment at each stroke, instead of dwelling for a much longer time twice during

each revolution of the second eccentric, and therefore there is not the same necessity for employing a very thin substratum of pitch; the process therefore is a much easier one. We have found that with the rigid bar, and, indeed, with the jointed one, a slight periodical movement of the guide D contributes much to free the surface from an annular character, for reasons which are obvious. The guide D is mounted now like the eccentric G, and a band from a small pulley on the axis of B, acting on a large one on the axis of D, effects the object in a very simple way.

When a speculum has been truly *ground* by the machinery acting with transverse strokes, the rigid bar will polish it on very easy terms, and for all the ordinary work of the observatory it will be sufficiently perfect.

We have long been in the habit of resorting to the rigid bar when out of practice and we required at once a fresh speculum.

A speculum of 3-feet aperture, which has usually been uncovered in all weathers for visitors, has frequently been so polished, and it has borne well a quarter-of-an-inch lens when tested with a watch-dial while on the engine. When a speculum of 6-feet aperture was last polished a rigid bar was employed; and the result was tolerably successful.

Since the publication of Mr. Lassel's experiments we have several times tried simple pitch, the movement being given by the rigid bar, but we have not succeeded in obtaining as good a surface or as fine definition as when the polisher was prepared in a more elaborate manner.

The combination of soap and ammonia which we employ may be prepared in this way. Half a pound of brown soap is dissolved in one quart of warm water, and one quart of strong water of ammonia is added. The bottle is then corked and shaken from time to time, for a week at least: we think it improves by keeping. One ounce of this mixed with eight ounces of water makes the lubricating fluid. The mixture should be made the day before, and kept in an open vessel, so that the excess of ammonia may evaporate. We were at first apprehensive that in employing this mixture we were endangering the hard film of the polisher, and so perhaps sacrificing to some extent truth of surface; but this was found not to be the case unless the ammonia was much in excess. As a kind of experimentum crucis, we polished specula with simple pitch rather softer than usual, employing pure water and the saponaceous mixture alternately, and found that the mixture was favourable to truth of surface instead of the reverse.

We have long ceased to make rouge, as it can be obtained of good quality from the rouge-maker.

In shaping the polisher by applying it to the speculum, we find it better that the polisher should be quite cold, while the pitch and resinous composition are slightly warm. We pass the flame of a few shavings or of wood-spirit under the polisher with its surface down, and instantly apply it to the speculum for half a minute. This is repeated till there is satisfactory contact. When the polisher was warm, we found it was difficult to avoid compressing the pitch too much. A crane makes the 3-feet polisher

quite manageable; and a travelling crane, with railway overhead and screw, effects the same thing for the 6-feet polisher. Both polishers are provided with gimbals, so that they can be instantly turned over. Though it is better to prepare the polisher fresh each time, we have often employed the same polisher successfully two or three times. In that case the polisher must be washed, and when dry the surface is to be very slightly moistened with spirits of turpentine. A thin film saturated with rouge will thus be removed; and a flame passed under will evaporate the turpentine. The polisher is then to be inverted and warmed to about 80°, the face being uppermost and again turned over. If now a flame is employed cautiously two or three times at intervals, the pitch at each square will become protuberant, bearing the hard resinous film on its surface, and the polisher will be restored very nearly to the same state it was in when originally prepared.

No one will be so ill advised as to attempt to construct a large reflecting telescope without first collecting all the information to be obtained in books. In Mr. LASSEL'S paper in the 'Transactions' of the Astronomical Society he will have an excellent guide. Should he employ an apparatus similar to ours, the speculum is first to be truly ground with the jointed bar. The throw of the first eccentric is to be one-third the diameter of the speculum, and that of the second, measured at the speculum, about one-fourth. It will be better in all early experiments to rely on pitch alone, carefully adjusted to the temperature at which it is to be used, perhaps 55°. The jointed bar which was employed in grinding the speculum is to be exchanged for the rigid bar, the eccentric and guide being readjusted. When the speculum has been successfully polished a few times in this way, an attempt may be made to obtain a better result by facing the polisher with a hard resinous composition; and finally the jointed bar may be resorted to, but at the same time the thickness of the pitch must be greatly reduced.

As to the mounting, it is simple, and any engineer could execute it without difficulty, the photographs supplying the necessary information.

The tube is supported at its lower extremity by a massive universal joint. It is counterpoised by weights which are constrained to move in a circular arc which nearly coincides with the curve of equilibrium; and a steady strain is kept upon the suspending-chain by means of three weights attached to levers, which successively come into play as the tube approaches the zenith and passes north beyond it: the levers are about two-thirds of the length of the tube, and have cross heads at their lower extremities, which are formed into bearings; and when in their places, the cross heads are all parallel to each other and to the transverse axis of the universal joint, from which they are about 5 feet distant. The levers thus move steadily in one plane, that of the meridian. A chain connects the levers at the proper intervals and the tube with them; and as the tube descends, each lever takes its place successively in a deep recess in the ground, the chain subsiding into a heap. This contrivance is effectual, and the chain has never fouled. The three weights are of different sizes, so proportioned as to reduce as much as possible the deviations from exact equilibrium at different altitudes, due to the irregular

action of the counterpoises, which move in an arc of a circle instead of the proper curve. A slow hand motion was originally fixed near the mouth of the tube, for raising or lowering it in taking measures; but we do not find it necessary. The telescope at the equator can conveniently follow an object three-quarters of an hour; and its motion is nearly equatorial: it would be almost exactly so if the pulley of the suspending-chain was in a line drawn from the axis of the universal joint parallel with the axis of the earth. The pulley, to give the chain more mechanical advantage in raising the telescope when very low, was placed above this line; but there was at the same time an arrangement made so that the chain might be brought, when necessary, by means of a grinding pulley, into the proper line. In practice we have found the movement of the telescope sufficiently equatorial without this: at a little distance, however, from the meridian, the plane of the position-circle of the micrometer deviates sensibly from a plane passing through the pole in all positions except when near the equator, as will be evident on considering the construction of the universal joint; and the distance from the meridian must be known where much precision is required in the reduction of the observations.

The motion in right ascension is by a rack the extremity of which bears by rollers on a circular arc of 40-feet radius. This rack is connected with the tube by a pinion, and the pinion is acted upon by an endless screw driven by a pulley, which pulley is driven by a band from a porter's wheel attached to the lower end of the tube. The pulley can also be moved by the observer; but this is not often necessary. The large circular arc is in pieces 5 feet long each, carefully planed, but not touching at their extremities, to guard against unequal action. The surface of each segment was adjusted separately to the plane of the meridian by a transit-instrument. And thus the means were provided for taking right ascensions with considerable accuracy. For polar distances there is a circle, 18-inches radius, at the lower end of the tube, furnished with a spirit-level; but for finding objects, there is an index of 6-feet radius connected with the transverse axis of the universal joint; so that the instrument can with the utmost facility be set roughly in polar distance. Means, of course, are provided for enabling the observer to reach the eyepiece in every position of the telescope. From 120° of polar distance to 80° this is effected by a stage, nearly counterpoised, which slides on bearers, the observer standing in a small gallery, to which he can communicate a transverse motion: the large stage is raised by a windlass; and, to guard against possible accident, there is an arrangement which locks it, completely under the command of the observer. From 80° to 50° the eyepiece is reached from the second gallery, and from 50° to 25° by the third. The fourth gallery, reaching to the pole, for which machinery was made at the same time as for the others, has never been put up, the other galleries so far furnishing ample employment for the instrument. The action of the galleries, and the way they are secured, is sufficiently evident from the photograph: they are of great strength, and in their construction, as well as in all other parts of the instrument from which there seemed to be a possibility of accident, the ordinary engineering rules as to strength have been considerably exceeded. The eyepiece arrangement consists of two adapters fixed

into one slide, so that there are always two eyepieces, a high and a low power, which may be employed successively simply by moving the slide. The slide is counterpoised, and the eyepieces fit in without screws. The telescope is perfectly steady even in a high wind, and we have had occasionally very fine definition during a strong gale.

From the experience we have now had, I think I may safely say that, where objects are to be observed only at short distances from the meridian, this plan of mounting is convenient and effective, and I do not see room for any material improvement. Where observations are carried on systematically, I do not think there is very much disadvantage in the limited movement in right ascension. Objects are best seen near the meridian, and no object can be thoroughly examined in any other position. There is, indeed, a small portion of the heavens which can scarcely be observed in the perfect absence of twilight, and an object there situated would probably be better seen, even at some distance from the meridian, when it was perfectly dark; but that seems to be of little moment. The really important disadvantage of the limited equatorial movement seems to me to be this, that, where fine nights are extremely rare, with an instrument so limited it is impossible to turn them to the best account.

It now only remains to answer in the best way I can the questions—

First, What magnifying power can be usefully employed with a speculum of 6-feet aperture? I perceive, in looking through the observations, that the single-lens eyepiece, $\frac{1}{4}$-inch focus, being a power of about 1300, is often mentioned as giving better vision than lower powers. That, I presume, may be considered the highest power it has been found advantageous to employ in general observations upon the nebulæ. With the speculum of 3-feet aperture I have occasionally employed powers exceeding 2000 in bringing out minute stars; and the speculum of 6-feet aperture has sometimes been in sufficiently good order to admit of equal or perhaps higher powers; but in our climate the opportunities of employing such powers are rare, and of short duration.

Secondly, it has been asked, Could a telescope be made of larger dimensions, and would it be of service? I feel little doubt that both questions should be answered in the affirmative. A speculum of larger aperture would, probably, on favourable nights bring out faint details of interest in the nebulæ, and add to the number of known double and multiple nebulæ. Something, however, will perhaps be accomplished in that direction in our future observations, by employing silver for the second reflexion; but if ever telescopes of equal power are erected in climates more favourable than this, perhaps more will be effected than would be possible here by pushing increase of aperture to the largest practicable limits.

In making a selection from observations so numerous, there has been considerable difficulty. It was not always easy to decide how much it would be practicable to omit without the danger of conveying an erroneous impression—without, on the one hand, perhaps unduly weakening the evidence of the fact recorded, or, on the other, unduly strengthening it. For instance: if, in the observations of a particular object, we find it recorded on six different nights that a minute star was seen involved, and that on six

other nights it was not seen at all, the twelve observations extending over a series of years, two cases might arise requiring very different treatment. First, if the nights when the star was visible were irregularly interspersed between the nights when the star was invisible, and we saw enough in the state of the atmosphere or speculum to account for the occasional invisibility of the star, it would probably be quite sufficient to enter one good observation when the star was distinctly seen. Suppose, however, that the nights when the star was seen were all included in the observations of the first three years, but that the nights when the star was not seen were in the observations of the last three years, then it would be necessary to enter all the observations, so that each person might be enabled to form his own opinion as to the cause of the discrepancy. 838 H., fig. 11, in the 'Transactions' for 1850, is a remarkable instance of this: from 1850 to 1858 the small star was not seen.

The details of faint nebulæ with curved or spiral branches have usually been made out by degrees, not only on successive nights, but often in successive years. In such cases we have not usually thought it necessary to give the early observations, or the observations on unfavourable nights, but merely a few good observations embodying the whole amount of information we had been enabled to obtain.

New nebulæ have not been looked for, our object being to scrutinize the more promising of the old ones; but new nebulæ have often been found in their immediate neighbourhood, and their places have usually been entered roughly in the observing-book, and a slight diagram made in the margin, so as to ensure their being easily found again: in such cases we have, to save space and diagrams, merely written "novæ near," and have only entered observations when the micrometer was employed. We have also, for the same reason, omitted many diagrams of known objects, where the positions and distances were merely estimated.

In the case of each object, we say "observed so many times;" that means that we have recorded observations of it on so many different nights: it may have been seen frequently on other occasions.

Where an object has been marked "observed several times," and nothing more, the inference is that with an instrument such as ours is, and in our climate, it would be waste of time to examine it further in the hope of making out details of interest.

It will be observed that the cases are very numerous where stars have been seen on the edges of nebulæ: we have taken care to enter each case, often, however, on the authority of a single good observation, as before explained.

The words "mottled" and "patchy" mean the same thing. Where the nebula is of that character, it is worth examining under favourable circumstances. The faint spirals have often been first seen as "mottled."

The word "finder" means the eyepiece with a large field. The telescope has no finder in the common acceptation of the word.

The letter "r" has been occasionally added to the description, and always in the same sense as that in which HERSCHEL employed it: I do not, however, attach much import-

ance to the expression of opinion it conveys, because the question of resolvability can only be successfully investigated when the air is steady and the speculum in fine order. In the early observations with the 6-feet telescope we had the advantage of a very fine speculum; it had been polished at the close of a long series of experiments with 3-feet specula, when by practice every refinement of manipulation was fresh in the recollection; there were also at that time several very good nights; and many nebulæ were resolved. Very soon after, the spiral form of arrangement was detected; and our attention was then directed to the form of nebulæ, the question of resolvability being a secondary object. In the mean time the speculum, which had been frequently dewed and occasionally cleaned, had lost its fine edge, and was no longer in a state to deal with the question of resolvability. Our aim was to trace out faint details, and in that respect also the speculum had lost much of its power It was therefore repolished, and, though less perfect than before, did the work we required well. Since that, we have had perhaps two or three specula as perfect as the first one; but the mass of observations have been made with specula considerably inferior to it, and, I am sorry to say, very often not as bright as they should have been. The removing a 6-feet speculum from the telescope to the laboratory, repolishing it, perhaps several times, and replacing it, is a serious operation, and has often been too long postponed. While the telescope was in constant use in all weathers, it would have been a hopeless task to attempt to keep it in a state fit for the resolution of nebulæ, and the attempt was not made. I may, perhaps, mention that with the 3-feet speculum in fine order I have often detected resolvability when there was no trace of it with the 6-feet speculum in its ordinary working-condition.

The question of resolvability, therefore, I think, must remain to be taken up separately, when the finest instrumental means are available, and when it may no longer be necessary to subject the specula to the wear and tear of constant work.

As to the nebulæ which have nuclei, some are described as increasing in brightness very gradually to the centre, others very rapidly, and some as having a stellar nucleus, or perhaps a star in the centre. These descriptions, however accurately conveying the impressions made upon the eye at the time, cannot be taken as in all cases representing real physical facts. A star may have been mistaken for a condensed nucleus, or the reverse; and it is often impossible to say which of the two suppositions is the more probable. The remarks as to the question of resolvability apply with equal force to the questions relating to the structure of nuclei. It is, however, probably worth remarking, that, while amongst the clusters there are objects which, if removed to a sufficient distance, or examined with an instrument of insufficient power, may be supposed to be representations of nebulæ with centres of varying brightness or condensation, there seems to be no cluster with a central star of such surpassing magnitude that under any circumstances it could be taken as the representative of the class of objects described as having a star in the centre.

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The little rough sketches in the margin are exact copies from the sketches made at the moment in the observing-book. There are usually several sketches of the same object made at different times; we have endeavoured to select the best.

As to the drawings, they usually represent the objects a little stronger than they appear on an ordinary night, but not stronger than on a fine night, when the air is clear and the sky black. Most of them have been repeatedly compared with the objects by different persons, and some have been several times sketched independently; so that I trust they are upon the whole accurate. The central portion of the nebula of Orion has been drawn with great care by Mr. BINDON STONEY, and Mr. HUNTER has been engaged this season in finishing the remainder; but another season will be required to complete the work.

Although there is probably no remarkable object in the list which I have not several times examined myself, for the great bulk of the observations I am indebted to the gentlemen at the time in charge of the Observatory. Mr. JOHNSTONE STONEY'S observations commenced in July 1848; and in June 1850 he was succeeded by his brother, Mr. BINDON STONEY. He continued in charge of the Observatory till May 1852; after that, Mr. MITCHELL observed for about two years.

Mr. JOHNSTONE STONEY occasionally also worked with his brother, and sometimes with Mr. MITCHELL.

Though so many of the observations were made in my absence, they are not the less to be relied on: nothing was done by an unpractised hand, and no pains were spared to ensure accuracy. I refer with as much confidence to the observations of the two Mr. STONEYS and Mr. MITCHELL as if I had on every occasion been present myself, because I know that they had thoroughly mastered the instrument and the methods of observing before they recorded a single independent observation; they were, besides, eminently cautious and painstaking.

There are no micrometer observations by Mr. MITCHELL: I now rather regret it, as several cases of suspected change have recently been brought to light in arranging the materials of this paper. The fault, however, was mine. It appeared to me so highly improbable that any change would be detected, that I requested Mr. MITCHELL to press on and not spend time on the micrometer. The most remarkable case of suspected change is perhaps H. 1905. HERSCHEL gives a drawing of it, the axes of the two nebulæ in a line. On April 11, 1850, Mr. JOHNSTON STONEY remarks the two nebulæ not in a line. April 17, 1855, Mr. MITCHELL remarks the two nebulæ are not in a line, but the axes are parallel, and gives a diagram. At the present time they are neither in a line nor parallel, but inclined at an angle of about sixteen degrees. The micrometer is employed without illumination; various contrivances were tried for illuminating the lines in a dark field; but the darkness was not absolute, and faint details were obliterated. We therefore substituted bars for lines.

In the 'Transactions' for 1850 are given Mr. JOHNSTON STONEY'S measures of H. 1622. M. OTTO STRUVE was good enough to send me measurements of the same spiral, and to

direct my attention to the fact that Mr. STONEY's positions are about two degrees in excess. A little consideration made it evident that the construction of the universal joint which supported the telescope was the cause of the error, and that a certain correction must be applied, depending upon the polar distance and the distance from the meridian. Mr. BINDON STONEY's measures of H. 1622 are also given, as also his measures of H. 2060, and STRUVE's measures of both. As OTTO STRUVE's measures are no doubt as exact as possible, it will be easy to judge of the degree of dependence to be placed upon the other measures made by us.

As to the figured nebulæ, little can be added to the information contained in the general catalogue. Plates XXV. and XXIX. figures 7 and 35, are from sketches by Mr. HUNTER, but they had been previously sketched several times by others. We preferred Mr. HUNTER's sketches, thinking they were upon the whole the most accurate, containing some additional details. Figure 43, the Dumbell, by Mr. BINDON STONEY, is based on micrometrical measurements, and is thoroughly to be relied upon. No stars are inserted which have not been measured. The powers used were low, the ordinary working-eyepiece: with high powers the faint details vanish.

The original observations are in books, in which they were entered each night: from time to time they were copied into a folio in the order of right ascension; and of that folio a copy was made for ordinary use in the Observatory. It will be easy therefore to supply, to any person who may be engaged in observing a particular object, all the information we possess. We have not given the places of the objects brought up to the present day, but merely HERSCHEL's numbers, to save space.

It is hoped that further inquiry will be suggested by the questions raised in the following observations; they have already opened up to us new grounds for further research.

INDEX TO THE FIGURED NEBULÆ.—PLATES XXV. to XXXI.

Figure.	Number in the observations.	By whom drawn.	Figure.	Number in the observations.	By whom drawn.
1	15	Mr. Mitchell.	23	1306 & 1308	Mr. Mitchell.
2	156	Mr. B. Stoney.	24	1337	"
3	232	Mr. Mitchell.	25	1385 & 1392	"
4	241	"	26	1414 & 1415	Mr. B. Stoney.
5	242	Mr. B. Stoney.	27	1441	Mr. Mitchell.
6	262	"	28	1589	"
7	311	Mr. S. Hunter.	29	1650	"
8	315	Mr. Mitchell.	30	1713	"
9	327	"	31	1905 & 1906	Mr. S. Hunter.
10	131	"	32	1946	"
11	393	Mr. B. Stoney.	33	1968	Mr. B. Stoney.
12	421	"	34	2075	Mr. Mitchell.
13	689	Mr. Mitchell.	35	1744	Mr. S. Hunter.
14	692 & 693	"	36	2084	Mr. B. Stoney.
15	765 & 766	"	37	2099	Mr. Mitchell.
16	875	Mr. G. J. Stoney.	38	2139	"
17	1011	Mr. Mitchell.	39	2172	"
18	1052 & 1053	Mr. B. Stoney.	40	2241	Mr. B. Stoney.
19	1061	"	41	2245	Mr. Mitchell.
20	1111 & 1113	"	42	2297	"
21	1202	"	43	2060	Mr. B. Stoney.
22	1245	Mr. Mitchell.			

Explanation of PLATE XXIV.

Fig. 1. The speculum upon its supporting levers, the apparatus by which the levers themselves are counterpoised having been previously removed to prevent confusion. 1. The lime-boxes, connected with the cover by sliding tubes. 2. The cover, which fits nearly air-tight. Before the cover is taken off, the lime-boxes are removed, all communication between the lime-boxes and speculum having been first intercepted by valves. Without this precaution, lime dust would make its way to the speculum. 3. Ring in which the speculum is suspended. 4. Supporting levers, which are shown in plan in fig. 10, where A represents a primary triangle, B a primary and three secondary triangles, and C one-third of the system complete, the dots being the balls. 5. The frame upon which the levers rest: a single casting. 6. A massive casting, which is bolted to the bottom of the table as soon as the speculum is in its place: it bears the wrought-iron girder (7) which holds the suspending-ring. 8. Turn-table for changing the specula: it is at the north of the telescope, close to the elevating-windlass.

Fig. 2. The same, with the weights and levers for counterpoising the secondary triangles. 9. The weight. 10. The common fulcrum of the levers. The levers are connected with the secondary triangles by slight straight rods.

Fig. 3. Grinding- and polishing-tool, seen at the back. It is suspended by gimbals; the tool can therefore be turned over easily from time to time, which is necessary in applying the pitch and resinous composition.

As soon as the tool is prepared the gimbals are removed; and it is then managed by the shackle in the centre. The shackle carries a triangle, with a lever at each angle. Each lever carries similarly two levers, connected at their extremities with T-shaped levers, which carry the tool by thirty-six points. The chain through which the counterpoise acts during the progress of grinding and polishing is hooked to the shackle, and the strain is thus distributed so equally that there is no sensible distortion.

Fig. 4. The same tool, seen in front. The straight grooves are produced by casting; and in this there is no difficulty, provided the pattern is nicely made. The little square blocks are kept in their places in the usual way by pins, and, remaining in the sand, are removed separately. The circular grooves are of course cut in the lathe.

Fig. 5 represents the telescope seen from the south-east, the stage of the first gallery being slightly raised.

Fig. 6, from the south-west, showing the machinery of the second and third galleries.

Fig. 7, from the south, showing the position of the telescope when a man enters the tube to fix the small speculum, and remove the cover of the large one, in preparing for the night's work.

Fig. 8, as seen from the north.

Fig. 9. The crane which carries the eastern counterpoise, on a larger scale.

The same parts are similarly lettered in all the figures. A, the cranes which carry the counterpoises. B, fig. 9, a guide-wheel by which the chain is kept in the axis of the crane in all positions of the counterpoise. The points of the shaft are placed eccentrically, so that it clears the wheel B when the telescope passes the zenith and moves north. F is of wood plated with iron, and is connected by rollers with the arc E, and by a rack and pinion with the tube. The pinion is driven by a wheel acted upon by an endless screw on the bar which carries the pulley N, fig. 7. The pulley N is driven by a band, and porter's wheel on the lower end of the tube, and thus the motion in right ascension is given. C, the principal counterpoises. D, the counterpoises of the stage of the first gallery. The stage is raised by increasing the action of the counterpoises D D. This is effected by chains attached to them, which pass underground to a small windlass. The counterpoises being rather less than the weight of the stage, it descends when the chains are relaxed. The stage carries the gallery G, which traverses on it in right ascension, motion being given to two of its wheels by a winch in the hand of the observer. E, an arc of cast iron made in pieces 5 feet long, not quite touching at the extremities, to guard against unequal expansion. Each piece was planed and accurately adjusted in the meridian by a transit-instrument. H, the second gallery. I, the third gallery. The galleries are supported by the beams *o*, which are plated with iron, and they are moved in right ascension by rack and pinion. P, tension bars to secure the iron framework carrying the wheels against which the beams press as the galleries are moved out. K, the cranes which carry the guy-chains, by which the counterpoises are constrained to move nearly in the curve of equilibrium. R, the chain which, passing over the pulley L, moves the telescope in polar distance. T, the chain of the principal counterpoise. In fig. 9 the chain M is seen, which raises three levers, each carrying a counterpoise. These levers successively coming into play as the telescope approaches and passes the zenith, maintain the chain R in such a state of tension, that the telescope obeys the windlass in every position. It has been found practicable so to adjust the levers that the residual error of compensation arising from the imperfect action of the guy-chains has been rendered almost insensible.

Fig. 11. Universal joint which bears the tube. A, bolts which secure it to the bottom of the tube. B B, two of the three adjusting-screws: these act against the carriage (5, fig. 1), directly under the ball-and-socket joint of the primary triangles. C, the axis perpendicular, and D the axis parallel to the horizon. The axis D gives motion to a large index for roughly setting the telescope in polar distance. E, a strong trussed framework of cast iron, secured to a solid stone foundation.

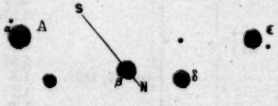
Abbreviations used in the Description of the Nebulæ.

B	denotes bright.	thus pB means pretty bright.
b	——— bright, or brighter .... —	pbM means pretty bright in the middle. (Large B applies to the neb., small b to a part.)
br	——— broad ..... —	40″ br = forty seconds broad.
cl.	——— cluster.	
d	——— double ..... —	d* = double star.
E	——— elongated ..... —	E. n. and s = elongated in the direction north and south.
e	——— extremely ..... —	eF = extremely faint.
F	——— faint.	
f	——— following ..... —	the f. of two = the following of two.
fig.	——— figure.	
g	——— gradually ..... —	gbM = gradually bright in the middle.
L	——— large ..... —	pL = pretty large.
l	——— little ..... —	lbM = little brighter in the middle.
M	——— middle.	
m	——— moderately ..... —	mbM = moderately bright in the middle.
neb.	——— nebula.	
neby.	——— nebulosity.	
n	——— north.	
p	——— preceding (when by itself or combined with n or s) ..... —	np = north preceding.
p	——— pretty (in other cases) .. —	pB = pretty bright.
pos.	——— angle of position.	
R	——— round.	
r	——— resolvable.	
S	——— small.	
s	——— south (when alone or combined with p or f); suddenly (in other cases)—	sf = south following; sbM = suddenly brighter in the middle.
v	——— very ..... —	vF = very faint.
vv	——— extremely ..... —	vvF extremely faint.

SELECTED OBSERVATIONS OF NEBULÆ.

Number in Herschel's Catalogue.	Number of times observed.	Description.
1	2	Nov. 4, 1850. Some stars seen in it, it is vF. Nothing further remarkable.
2	1	Sept. 18, 1857. 2 neb. nearly in line p. and f; about 14′ apart; the p. one is of irregular outline; F; bM. The f. one is S; R; pB; bM.
4	1	Oct. 23, 1857. E. n. and s; B nucleus.
6	6	Oct. 12, 1855. E. n. and s; has a central nucleus and a * on edge, nf. the nucleus.
13	7	Resolvability not quite certain, irreg; R; nucleus.
15	6	Oct. 7, 1855. There are 6 or 8 *s of about 14–15 mag. and several smaller ones; I counted 7 knots, the 3 northern of which are the brightest; sketched. See fig. 1.
16	1	Sept. 19, 1857. S; R, or nearly so; and lbM. [Plate XXV.
17	2	Oct. 26, 1854. Several S; F. neb. visible at once in finder. 17 is R. and bM.
21	9	Spirality suspected, but more evidence wanting. Dec. 12, 1851. Nucleus; R. Oct. 22, 1857. I suspect outlying F. nebulosity, especially p. and f.
23 25	2	Nov. 4, 1850. 23 is R; and S. 25 has nucleus, and is E; 3 others near.
26	1	Dec. 6, 1850. R; F. nucleus; 40″ br.
29	4	Aug. 21, 1852. Involves some *s, one of about 12th or 13th magnitude, E; vF.
30	1	Dec. 9, 1854. R; pL; bM.
32	2	Sept. 18, 1857. S; d. neb.; the n. one is E, sp, nf; bM.
35	5	Sept. 1849. r; L; and rather F. Oct. 7, 1855. 3 *s in it.
37	2	Nov. 24, 1854. Not L; R; bM; a B. * close sp; r?
40	2	Dec. 6, 1850. sbM; another 18′ distant.
42	2	Nov. 24, 1854. R; bM.
44	9	Oct. 16, 1855. vL; mE. np. by sf; sharp nucleus, for some distance round which, the neb. is B, and then *suddenly* decreases; there is a B. * np. the nucleus; and another involved in sf. end; another in p. border. Nov. 2, 1850. Spirality suspected.
45	1	Oct. 16, 1854. E. n. and s; many *s involved.
46	1	Dec. 7, 1857. s. of it is another neb.; E. nearly n. and s; 46 has B. centre; mE. n. and s, arms vF.
47	4	Nov. 3, 1855. Oval, and I think r; and has a * at np. edge.
50 51		Drawing not complete. The following are measures of some of the *s involved:—

		Pos.	Dist.
Dec. 18, 1851.	1.13	248°	6′ 13″
	1.14	243	5 14
	1.15	260	5 51
	1.16	260	7 42
	1.17	241	7 30
	1.18	280	4 22
	1.19	301	7 35
Dec. 22, 1851.	6.20	219	3 46
	6.21	246	3 6
	6.22	234	5 1
	6.23	258	4 46
	6.24	271	4 20
	6.25	252	5 40
	6.26	295	4 34
Oct. 25, 1851.	1.N	83	2 2
	1.2	6	4 54
	1.3	309	5 1
Oct. 28, 1851.	1.4	1	7 25
	1.5	244	1 57
	1.6	254	6 13
Nov. 24, 1851.	1.7	8	7 32
	1.8	12	6 40
	1.9	11	5 46
	1.10	349	3 0
	1.11	345	2 10
	1.12	338	4 43

Number in Herschel's Catalogue.	Number of times observed.	Description.
53	1	Sept. 19, 1857. S; R; vF; bM.
54	2	Nov. 22, 1854. pB; vS; R.
59	3	Dec. 22, 1848. 3 neb. in line, 2 of them "novæ." Oct. 23, 1856. 1st is R; pB; bM; and has nucleus; 2nd bM; E, ✱ involved; 3rd F; lE; bM.
60	1	Nov. 22, 1854. S; R; bM.
65	3	Sept. 18, 1857. S; pB. disc, in vF. haze of mottled neby.
69, 70	7	Oct. 3, 1856. 69 is S; B; R; with B. nucleus; 70 is F; E. and patchy. I sometimes thought it was formed of two knots involved in F. neby; there appears to be a nebulous connexion between them all. Nov. 15, 1857. The silvered mirror shows the object brighter than before, but no new details; definition bad.
71	7	Suspect spirality; light unequal.
72	3	Oct. 26, 1854. a F. object with two nuclei.
78, 79	4	Nov. 29, 1850. α is vlbM; β has stellar point or nucleus. I suspect δ to be a F. neb. Pos. Dist. $\alpha\beta$ 219° 5′ 35″ $\alpha\gamma$ 315 1 8 $\alpha\delta$ 81 0 44 Nov. 3, 1855. 3 neb. nearly in line, sp, nf; β is bM. and lE. p. and f; α is R; bM; with a d. ✱ np, and is the largest of the 3; ϵ is S; F; R; δ is a ✱.
80	1	Oct. 3, 1856. pL; not vF. Its brightest part is a line running diagonally, and there is a knot at either end; believed to be a spiral.
84, 85, 86	4	Nov. 4, 1850. Pos. Dist. $\alpha\beta$ 169° 2′ 19″ $\beta\gamma$ 160 4 22 $\gamma\gamma'$ 201 0 34 $\gamma\delta$ 157 3 19 $\gamma\epsilon$ 176 5 32 $\epsilon\zeta$ 199 1 41 $\delta\epsilon$* 79 4 55
87	3	Oct. 26, 1854. A d. neb., both S; R; bM.
89	8	A cl. with much unresolved neby.
90	1	lbM.
91, 92	1	3 neb. in a triangle.
96	6	Oct. 26, 1854. Lenticular n. and s. Thought I saw a ✱ at times in centre (1¼-inch single lens); a lp. this is another vF. ray, np, sf, and which has no nucleus. Oct. 16, 1855. vF; E. n. and s; has nucleus; ✱ in n. end. Nov. 3, 1855. mE; pB. nucleus, and ✱ in n. end; np. this neb. is a ✱ of the 9th mag., and about the same distance p. this ✱ is another neb. vF; mE. Dec. 7, 1855. Seen as before; comp. neb. verified. Oct. 23, 1856. F. ray has nucleus and a ✱ in n. end. Sept. 18, 1857. E. n. and s; another vF. ray p, which is E. np. sf.
98	1	vF; R; S.
99	1	Oct. 3, 1856. S; F; R; bM; has nucleus.
103	3	Is n. of the 3rd of a group of 4 ✱s in line; 3 "novæ" near. Pos. Dist. Dec. 6, 1850. $A\beta$ 28° 7′ 36″ Dec. 7, 1850. $\beta\delta$ 40 4 6 $\beta\epsilon$ 81 9 19 $A\delta$ 30 7 43 
104	1	Oct. 23, 1856. 6 neb., all visible at once in finder eyepiece; 2 of them E., the others S; R; bM.
105	1	Dec. 11, 1854. vmE; bM (speculum dewed).
106, 108	8	A variety of new nebulæ found, but observations too voluminous to transcribe.
112	6	Sketch made, but no interesting details. Nov. 30, 1850. vF, and p. a quadruple ✱. Oct. 23, 1851. 3 ✱s f. neb.; light unequal. Sept. 16, 1852. 2′ diameter; several ✱s in it; probably a F. cl.

* This should be, I think, $\delta\zeta$. A S. d. neb. suspected below at α'.

Number in Herschel's Catalogue.	Number of times observed.	Description.
113, 114	2	Both have nuclei; "nova" near. Nov. 16, 1857. 113 is E. p. and f; ✱ closely sp; 114 is R, with ragged edge and bM; "nova;" S; R; bM.
115, 121	1	Oct. 3, 1826. The p. one is a pB. S. disc in F. outlying neby. The f. one is R; bM.
116	1	Dec. 18, 1851. s. end of neb. is like a brush or broom with a split.
118, 120	2	4 neb. found, 2 have nuclei. 118 is S; R; 120 has 2 ✱s on np. edge; E. p. and f.
119	1	Dec. 9, 1854. pL; pB; bM to a nucleus.
123	2	Sept. 18, 1857. Rough sketch made; mE. np, sf; a F. triple ✱ f.
128	3	Nov. 28, 1856. L; B; mE; B. nucleus. "Nova" f.
131	27	Nov. 29, 1850. αβ 215° 0′ 51″; αγ 163 0 56; αδ 160 2 56; αε 178 3 07; αζ 192 3 44; αη 206 4 14; αϑ 224 4 58; Dec. 27, 1850. αμ 147 5 34; αλ 179 5 56; ακ 201 5 42; μν 143 6 28; αι 287 4 30; απ 341 6 45; Jan. 2, 1851. α2 5 5 18; αψ 357 4 42; α3 51 11 0; αρ 38 9 50; ατ 58 11 16; Dec. 23, 1851. αω 161 5 20; αα′ 149 6 53; αβ′ 172 6 32; αγ′ 174 7 18; αφ 205 2 22
		For previous observations see Transactions, Part II. 1850.
		Sept. 13, 1850. Large spiral full of knots; to nf. is a S. neb. B, which on a very good night might appear attached to spiral, than which it is brighter. Oct. 11, 1850. Spiral arrangement not clearly seen. Nov. 27, 1850. Arms of spiral scarcely seen; fog. Nov. 30, 1850. Spiral form very indistinct; wind very high from s. Oct. 22, 1851. Viewed for drawing, I should not have seen the spiral arrangement had I not observed it before. Oct. 25, 1851. Neby. extends for several minutes all round, perhaps for half a degree in radius. Oct. 29, 1851. Observed for drawing. Dec. 14, 1851. Sketched. Dec. 26, 1851. Drawn. Dec. 7, 1855. This neb. reaches in length through at least a field and a half of finder eyepiece. Mr. Stoney's drawing leaves out a great deal of the neby. about the centre, and ✱ suspected to left of centre of the trapezium of ✱s, perhaps others also. Nov. 15, 1857. There are 3 ✱s about the principal nucleus. Dec. 7, 1857. Carefully observed, with a view to a new sketch. Dec. 18, 1857. Carefully observed, and my sketch proceeded with. See fig. 10, Plate XXVI.
132	1	Nov. 28, 1856. B; S; R. nucleus, a ✱ p. and another n.
Nova.		Nov. 29, 1850. A S. neb. or cl. with 3 ✱s in it. *R* 1ʰ 26ᵐ. N.P.D. 60° 35′.
134, 135	2	Oct. 26, 1854. Both S; R; B.
136		Sought for four times; not found.
142	8	Dec. 13, 1848. Rough sketch made. Spiral? Dec. 14, 1848. Confirmed last night's observation; feel confident it is a spiral. Oct. 24, 1851. Centre formed of ✱s; easily seen to be such; several ✱s through the neb.
143	1	Oct. 3, 1856. vS; F; R; bM; had a ✱ close to n. edge.
147	2	Nov. 30, 1856. S; R; bM. to a nucleus.

Number in Herschel's Catalogue.	Number of times observed.	Description.
148	1	Dec. 11, 1854. S; R; bM. to a nucleus.
149	3	Nov. 30, 1856. Nucleus; E. np, sf.
150	6	A B. ray, with * in s. edge, a little f. the nucleus.
151	2	Oct. 3, 1856. Long; vF; vlbM. A B. * in p. edge.
156	3	Oct. 7, 1850. Light rather equable, a minute * in the p. part. Nov. 24, 1851. E; a * of 10th mag. nf. Sketched. Nov. 28, 1856. I see **s sparkling in it at times. See [fig. 2, Plate XXV.
157, 159	4	A group of 5 neb.; others near.
161	2	Oval; * in n. edge.
162		Looked for 8 times. Dec. 18, 1848. Found * 7th or 8th mag. in place, but saw no nebulous atmosphere.
163	1	No nucleus. R; pF; bM.
164	1	Dec. 11, 1854. vB. nucleus.
165	1	Nov. 29, 1856. More E. than Herschel describes it; vbM.
169	1	A group of 5. Oct. 11, 1850.
172, 173	1	Nov. 24, 1854. d. neb.; the p. one is pB; R; bM. The f. one is smaller and fainter, and lbM.
175	1	Oct. 11, 1850. d. neb.; about 18′ nf. (169); nf. is a 3rd F. neb.
181	7	Branches suspected several times, but not distinctly seen. Has a comp. neb. 5′ or 6′ s.
182	2	Oct. 23, 1857. S; pB; R; bM.
183	2	Dec. 7, 1850. Nucleus E. np, sf.
188	1	Nov. 30, 1856. S; F; R; lbM.
190	1	Nov. 22, 1854. eeF; E; no nucleus. A * 10th mag. p: several S. **s near.
193	2	Dec. 18, 1856. bM. to a nucleus. E. sp, nf; S. * in s. end.
194	1	Some **s in it.
195	2	Nov. 30, 1856. E. sp, nf; a F. * follows closely; there is another F. * in n. edge.
197	4	Dec. 23, 1851. I suspect a F. appendage f; a d. * f.
198	1	Nov. 24, 1854. vF; mE; almost lenticular.
199, 200, 201, 202	2	Nov. 29, 1856. All are S; R; bM.
205	1	Nov. 28, 1856. S; R; lbM; a * in centre.
207, 212		Sept. 13, 1850. Between the 2 cls. there is a red * nearer the 2nd, and 2 more red **s f. 2nd cl. of 8th or 9th mag.
208, 210	14	Nov. 3, 1855. A dark space running along s. side of nucleus of 210, and (Nov. 5, 1850) * in sf. extremity; r. Both have S. comp. nebs. s; 208 is E; * close f. centre.
212	1	No description.
213	2	Nov. 30, 1856. vS; eeF; R; vlbM.
215	2	Oct. 29, 1851. Nucleus; 5′ n. of d*.
216	1	Sept. 17, 1852. * in edge; perhaps like a snowdrop.
217	4	Sept. 14, 1850. Oval; mbM; pB; 50″ by 70″.
218	7	Dec. 27, 1850. Pos. of chink 19°.
219	1	Nov. 28, 1856. d. neb.; components unite at p. end. The s. one is L, E, and gbM. The n. one is more E. and fainter, and also bM.
221	2	Oct. 23, 1857. L. but eF; mottled; **s in it, especially one closely n. of centre.

Dec. 26, 1851.

	Pos.	Dist.
αβ	44°	1′ 57″
αγ	198	4 38
αδ	199	5 48
αε	211	2 38
ray	23	10 29

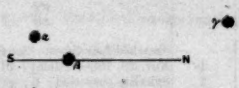
Oct. 11, 1850.

	Pos.	Dist.
αβ	12°	0′ 30″
αγ	46	1 19
αδ	118	3 20
αε	296	1 59

Oct. 11, 1850.

Pos.	Dist.
171°	0′ 25″

Number in Herschel's Catalogue.	Number of times observed.	Description.
222	1	Sept. 14, 1850. 3′ by 50″; rather F. dash of light; a conspicuous * nf. the M. outside edge.
223, 224	2	Nov. 29, 1856. 223 is pL; B; vbM; R? It seems to have some F. *mottled* neby. about it. 224 is vF; pL; vlbM.
226	3	Oct. 16, 1855. Oval; no nucleus; light pretty equable; major axis np, sf; clearly r. I can at moments see some of its *s. A B. * at s. edge.
229	1	Nothing particular.
230	2	Nov. 24, 1851. Brightest part near p. edge; E. nnf. sp; d. * n, to which neb. does not reach.
231, 233, 234, 237	2	Oct. 11, 1850. Pos. Dist. αβ 83° 3′ 43″ αγ 22 1 59 αδ 40 2 31 Another about 12′ sf.
232	9	Oct. 12, 1855. Sketched; r. See fig. 3, Plate XXV.
238	3	Dec. 12, 1848. bM. nearly to nucleus.
241	8	Sketched twice, Dec. 11, 1854, and Nov. 23, 1857. See fig. 4, Plate XXV.
242	8	Dec. 27, 1850. Pos. Dist. αN 73° 0′ 54″ αγ 81 3 19 αβ 331 0 29 αδ 282 1 39 Sketched four times. See fig. 5, Plate XXV.
244	1	Nov. 28, 1856. Patchy; pL; mbM.
246	2	Spirality suspected; E; gbM.
247	1	vS; R; F; bM.
254	1	gbM.
255	4	Dec. 27, 1856. r; has 3 *s in edge, and I think I see one just p. the nucleus.
256	1	E.
257	6	Jan. 2, 1851. 5 knots; the p. one is d. neb. Dec. 26, 1851. A ruddy * of 10th mag. p. 16′.
258	4	Nov. 30, 1850. A F. dash of light nearly p. and f; the n. edge is the best defined.
262	12	Sketched 4. Dec. 22, 1848. A blue spiral. Jan. 14, 1849. Spiral. Oct. 29, 1851. The central part is flatter on the f. side. Nov. 24, 1851. The central part is, I am nearly quite sure, spiral, sketched. Jan. 13, 1852. Spiral form of centre seen. Nov. 29, 1856. Details of drawing seen very well. Jan. 10, 1858. I can see nothing more than is given in the sketch, which appears to me correct, though perhaps it defines too well the edges of the B. central disc. See fig. 6, Plate XXV.
263	1	Dec. 7, 1850. R. nucleus.
264	6	Nov. 23, 1848. A curious object with dark spaces. Oct. 10, 1850. r. Oct. 16, 1855. Fine oval neb; has nucleus; light mottled; sometimes I thought I saw a dark bay n. of nucleus; certainly the neb. is brighter along the n. and nf. side than in the part intervening between that and the nucleus. Dec. 6, 1855. Previous suspicion as to direction and existence of dark streak confirmed; the nucleus and n. edge of neb. both seem r.
265	2	Jan. 7, 1849. F. patch, 2 *s perhaps, p. middle.
266	3	Jan. 14, 1849. vF; lE.
269	1	Very badly seen.
271	3	d., with another knot near.
273	2	Dec. 7, 1857. F; S; R; lbM.
275	6	Appendage suspected; E. n. and s; bM.
276	2	Dec. 7, 1857. vS; R; F; a S.* close sp.
277	3	Dec. 9, 1857. pB; oval; has a B. central nucleus; about 4′ n. is a F. E. neb. containing *s.
279	1	Dec. 11, 1854. Has a B. * sp. the nucleus.
280	3	Badly seen.
282	5	Oct. 10, 1850. BM; r.
285	6	Nov. 29, 1851. E. p. and f; bM.
286	8	Oct. 30, 1851. E. p. and f; bM; between this neb. and 282 there are very few *s.
287		cl.

Number in Herschel's Catalogue.	Number of times observed.	Description.
288	1	S; R; vF; bM.
289	3	Dec. 8, 1850. Double; γ * of 9th mag; a is 289, and has a F. nucleus; β "nova." Pos. Dist. $a\gamma$ 2° 2′ 53″; $\gamma\beta$ 152 2 08; $\gamma\delta$ 103 1 54.
290	5	Nov. 23, 1848. Coarse, cl. strongly honey-combed. Would probably look annular with eccentric eyehole if it were far enough to be a neb. Nov. 21, 1851. The honey-combed appearance is caused by the disposition of the brighter **s; no spiral arrangement.
292	3	Jan. 17, 1855. r. Nov. 28, 1856. Edge ragged.
293	4	Dec. 16, 1848. A multitude of nebs. knots in the neighbourhood, principally p; counted 15; many more. Dec. 8, 1855. One of them F; has a * close sf. and looks like a snowdrop.
294, 295	1	Nov. 24, 1854. Two S. R. neb.; both bM.
296	5	Dec. 19, 1848. gbM; E. sp. nf.
297, 298	10	2 "novæ." Oct. 10, 1850. Pos. Dist. $\beta\alpha$ 143° 1′ 35″; $\beta\gamma$ 11 6 1.  Nov. 2, 1850. Another 8′ n. of γ.
299	2	Dec. 16, 1854. vF; lbM. to a nuc.; mE. np, sf.
301		Scattered cl.
302, 303	4	302, r. 303, mottled.
304	1	Jan. 17, 1855. d. neb.; both vS. and bM.
305, 306, 307	8	Dec. 26, 1856. The p. one is vF. and light mottled. Oct. 7, 1850. 1st appears divided, and preceding part has a minute *. Jan. 22, 1851. f. the 3rd; 14′ is "nova."
308	1	Oct. 31, 1856. A fine d. * in a loose cl.
309	2	Oct. 26, 1854. S; R; bM.
310		Cluster.
311	9	Sketched five times. Jan. 13, 1852. New spiral of an annular form round the *, which is central. Brightest part is sf. the *; spirality is vF; but I have no doubt of its existence. Oct. 7, 1855. Annular, but with a break in s. side of annulus, or perhaps spiral. Oct. 31, 1856. I feel certain of a dark space nearly p. the central *, but the shape of the whole is only conjectural; there is a * plain np. the neb. Dec. 7, 1857. Not vF, and the break in the s. side of the ring of neb. quite easily seen; *between* this ring and the central * is not black, but filled with more F. neby. Jan. 9, 1858. Observed for a sketch; last observation correct as to shape; the brightest part is sf, and the next brightest is on the opposite side, and with ½-inch single lens the whole annulus has a mottled look. Jan. 13, 1858. The whole edge was ragged and irregular, and the whole neb. much mottled. See fig. 7, Plate XXV.
313	2	Dec. 16, 1854. vF; S; R; lbM.
315	16	Sketched five times. Jan. 2, 1851. Dark space sf. neb.; though I did not see the F. neby. beyond the channel, I conjecture that it exists and fades off imperceptibly, somewhat like the drawing. Jan. 22, 1851. Observations of Jan. 2nd confirmed; F. neby. seen; B. part r. Nov. 29, 1851. Last season's observations confirmed as to shape. Dec. 22, 1851. Previous observations confirmed. Jan. 13, 1852. s is d; ξ is the angle of the brightest part. See fig. 8, Plate XXV. Pos. Dist. $a\beta$ 57° 0′ 55″; $a\gamma$ 204 3 50; $a\delta$ 199 4 1; as 315 2 23; $a\zeta$ 223 1 42.

Number in Herschel's Catalogue.	Number of times observed.	Description.
316		Dec. 5, 1850. α and γ are bM; γ is about 10′ nf. α, and has a brush-like elongation (see 242) at each end.
317	7	
318		

		Pos.	Dist.
Oct. 7, 1850.	$\alpha\beta$	77°	0′ 56″
Dec. 5, 1850.	$\alpha\beta$	75	0 58

319	2	3 "novæ" near.
321	3	Oct. 26, 1854. Has a ✱ at n. extremity; E. np. by sf; Herschel's d. ✱ nf. is triple. Jan. 15, 1855. The conspicuous ✱ involved in n. end of neb. has a F. comp; nf. itself very distinct with 1½-inch single lens.
320	1	3 neb. nearly in a line; one "nova"; 1st pL; F; R; 2nd pF; R; 3rd dull nucleus.
322		
327	10	Sketched twice. Appears to be a spiral, but evidence not quite satisfactory. See fig. 9, [Plate XXV.
331	1	Nov. 29, 1856. 2 ✱s near edge; vS; irreg. R.
334	6	4 neb. (3 "novæ"); one of them is E.
336	6	Jan. 13, 1858. B. centre; F. neby. stretches out a long way, involving a minute ✱ p.
338		Oct. 26, 1854. A group of a few ✱s.
339	1	2 nebs. knots.
340	2	"nova" near.
343		Looked for seven times. Not found.
347	15	Dec. 11, 1850. A S. comp. p. and a d. ✱ n. Jan. 10, 1858. Looks like a F. haze enveloping 3 ✱s.
349		Large loose cl.
352	2	Close d. neb.
354		Dec. 28, 1856. Neat little cl; its centre consists of about 40 or 50 ✱s; the outlying ✱s are arranged in curved branches.
355	5	Nov. 29, 1848. Saw a multitude of ✱s and some unresolved neb.
356		Looked for four times; not found, but nights bad.
357	19	Sketch not quite satisfactory.

		Pos.	Dist.
Nov. 29, 1851.	$\gamma\delta$	351°	0′ 47″*
	$\gamma\zeta$	52	2 10
	$\gamma\eta$	40	2 16
	$\gamma\theta$	70	2 53
	$\gamma\iota$	110	3 19
	$\gamma\iota$	104	3 37
Jan. 12, 1852.	$\theta\gamma$	348°	1′ 36″.

358		Coarse cl.
359	1	Dec. 28, 1856. Looks like a ✱ in vF. neb. atmosphere. lE. p. and f.
360	43	(Neb. of Orion.) Account of detailed observations postponed, as in 50 and 357.
361	11	Observations recorded in the 'Transactions' for 1850 fully confirmed.
363	6	Nov. 30, 1850. The luminous appearance extends about 15′ all round the ✱.
365	3	Oct. 23, 1851. r: I strongly suspect it is annular.
368	8	Feb. 9, 1852. Spiral arrangement sufficiently seen to confirm former observations. Jan. 9, 1856. Appears in finder a B. oval neb., with n. and nf. edges brightest and best defined, and sp. edge fading away gradually; with higher power there is seen a decided darkness at and between the ✱s, and I can confirm previous observations as to the curve formed by the brightest part of neb. Dec. 26, 1856. Nebulosity easily traced as in preceding sketch.

370	1	Jan. 21, 1857. r? suspect ✱ in centre.
373	3	Nov. 30, 1850. Same appearance as ι Orionis, but very much fainter.
375		Jan. 17, 1855. A pretty close cl. of S. ✱s, followed by four or five B. ✱s.
378	7	Dec. 11, 1850. I saw no nebs. round 378; sf. about 20′ is a triple ✱, the middle one of which is pretty strongly nebs.; about 36′ f. (a little n.) is a d. ✱, whose brighter component is nebs.; 65′ f. 378 is a S. neb. with nucleus or stellar point.
381		
383		
384	1	No description.
385	2	A few B. ✱s; scattered.
389		Dec. 28, 1856. Very loose cl.

* Note by observer: "I have reason to believe that the distance of $\gamma\delta$ is incorrect."

Number in Herschel's Catalogue.	Number of times observed.	Description.
390		Jan. 20, 1857. A close irregular cl. of vS. ✱s; figure as sketched, one ✱ rather brighter than the rest; forms as it were a nucleus, round which the others are grouped, but principally np. side of it.
393	15	Sketched three times. Feb. 28, 1850. S. ✱ near a L. one; the L. ✱ f. the neb. has a comp; this, No. 393, is an enormous neby, which I traced f. and n. of it to a great distance, some degrees. It narrows at times to a band across the finding-eyepiece of about 6′ or 8′. I fancied the number of L. ✱s was greater in it than in the neighbourhood; I am certain the number of S. ✱s is much less. In a small space, taken at random in its neighbourhood, I reckoned upwards of 20 S. ✱s. In a similar space in it, taken at random, but 3. See fig. 11, Plate XXVII.
399	11	Feb. 22, 1851. 2 ✱s in p. part of the neb. Nothing additional to what is in the 'Transactions' for 1850.
401	9	No neby. found, and only a few ✱s arranged in pairs; no cl. Has there been a change here?
403		R; with rays.
404		Jan. 10, 1856. S. cl. of S. ✱s; oblong nearly p. and f.
406, 407	12	The southern one has nucleus.
408	1	
409, 410	7	Dec. 8, 1850. 5 nebulous knots.
		Pos. Dist.
		αγ 344° 2′ 32″
		αβ 323 1 46
		αδ 3 5 08
		αε 30 6 11
411		Nov. 25, 1851. A coarse B. cl.
413		Jan. 20, 1857. Pretty cl. of pB. ✱s; centre nearly R.
415		Jan. 8, 1851. A poor cl.
421	6	Nov. 23, 1851. v. close d. neb. below 4 ✱s. See fig. 12, Plate XXVII.
425		Feb. 13, 1852. Coarse cl.
424, 426	3	Both bM.
427		v. loose cl.
428	2	Feb. 1, 1856. vF. fan-shaped neb. involving 3 ✱s.
430	2	Jan. 31, 1851. Several knots around; 430 is E. np, sf.
431	2	Jan. 18, 1855. S. ✱ in s. edge. Jan. 25, 1857. r.?
434	10	✱ in f. edge; r.
439		Jan. 9, 1856. Loose cl; irreg; R.
443		Jan. 30, 1856. About 25 or 30 ✱s of a curious shape.
444	15	Nov. 23, 1851. S. ✱ in f. end of np. appendage, also one nf. the neb. about 40″; nothing additional to description in 'Transactions' for 1850.
446, 447, 448, 449	5	Feb. 26, 1851.
		Pos. Dist.
		αβ[a] 222° 3′ 41″
		βγ 282 5 37
		δε 291 3 49
		εζ 267 6 47
450	25	Although 21 observations have been made since the sketch appeared in the 'Transactions' of 1850, nothing additional has been discovered, except that the outer luminous ring is of unequal brightness.
453	1	Jan. 21, 1857. S. ✱ close to n. edge.
454		Neat little cl. of vS. ✱s. It looks in finder like a r. neb.
456	11	Edge filamentous; r; vlbM.
457	5	Edge filamentous; r; looks like a globular cl.
458	1	No description.
464	16	Dec. 8, 1850. Dark space more eccentric than in the drawing in the 'Transactions' for 1850. The larger of the 2 ✱s is in the dark space, the other s. of it in the neb; a 3rd ✱ close nf.
465	1	Jan. 11, 1856. pF; bM; 1E?

[a] α, β, γ and δ, ε, ζ are distinct figures.

Number in Herschel's Catalogue.	Number of times observed.	Description.
468	5	A group of 6 ⁕s; no neby.
469, 470	1	Feb. 20, 1851. A great many knots; reckoned 10 in a line nearly p. and f.
471	1	Jan. 9, 1856. d. neb.
473	1	Feb. 1, 1856. F. ray, with pB. nucleus; np. this is another neb. vF; E; with ⁕ near nucleus.
476	2	Jan. 12, 1855. A F. ⁕ p. and a nebulous knot f.
477	1	Another near, both F. S.
478	16	Jan. 20, 1855. I see 2 ⁕s in p. edge with ½-inch single lens. The smaller component of a double ⁕ touches f. edge; light mottled. On several occasions spirality suspected, and rough sketch made.
480	Frequently.	Several observers have fancied that the ⁕s exhibit some approach to a spiral arrangement, with cellular centre. No unresolved neby.
481	1	Jan. 20, 1857. ⁕ in n. edge; centre r?
482	1	Nucleus; vF; R.
483, 484	3	Jan. 31, 1851. 2 others near 483. Pos. Dist. Feb. 26, 1851. αβ 242° 2′ 48″ xy 319 5 36
486	3	Feb. 16, 1855. L; vF; lE; light mottled; suspect dark spaces round the centre; F. stellar point or nucleus; several ⁕s in edge and in it. Rough sketch made.
487	1	Jan. 25, 1857. Light mottled; B. ⁕ n; a F. ⁕ close nf. edge.
489	10	Feb. 14, 1857. Certainly a ⁕ in centre or nucleus, and neby. projecting to sp. side, but eF. Mar. 10, 1858. (Definition very good.) Nucleus stellar; the brightest part of the neb. looks r. It is pL. and mottled; suspected spiral.
491	15	Sketched 6 times. Spiral. Jan. 29, 1856. Very well seen; previous observations confirmed. I have no doubt the neb. is a spiral. The f. half of neb. is the more difficult to see well. Mar. 10, 1858. Well seen; the whole neb. looks vB. and sparkling; part is clearly r; my former conjectures as to its shape confirmed. I used the highest single lenses. Mar. 11, 1858. (Definition very good.) Observed with same results as on last night.
492	6	But never well seen.
494	1	Jan. 20, 1857. vF; E; nearly n. and s; has a sharp pB. nucleus.
495	2	Feb. 28, 1851. Centre r; E. n. and s.
496		Jan. 31, 1851. Coarse cl; lanes and openings without any ⁕s whatever.
497	4	In the centre of a triangle formed by 3 minute ⁕s; nucleus.
498	2	Feb. 22, 1857. 2 ⁕s on np. edge; S; F; R; bM.
499	3	Nucleus; F. ⁕ in p. edge; S; vF; R.
504	2	r; ⁕ in f. edge.
505	10	Jan. 27, 1852. r. Feb. 9, 1855. Centre suddenly B; irreg. R.
506	4	Jan. 17, 1855. Centre suddenly B. with outlying F. neby, which involves a ⁕ nnf.
507, 508	3	Feb. 9, 1850. A fine object; 3 neb. forming a triangle; one B, another pB; the third the last degree of faintness.
510	1	4 neb. here. The f. one is E. and has nuc; the others are S. and F.
512	5	Jan. 10, 1856. Not vS. but vvF. and flickering. Feb. 1, 1856. Nucleus and ⁕ close to s. side of it, and two very indistinct branches of neby. From the tenour of the observations no doubt it is a spiral; the twist of the branches fully confirmed.
513	9	Nov. 30, 1850. S. ⁕ in its nf. edge, perhaps not connected with the neb. The neb. had a brush-like appearance. Feb. 1, 1851. Dark space f. the ⁕ between neb. and ⁕, like the "snowdrop" neb. (see fig. 10, 'Transactions' for 1850).
514	20	Dark space suspected in centre, but never fully confirmed. Remarkable for extreme paucity of ⁕s in neighbourhood.
518	11	Nucleus surrounded with L. F. neby.
519	6	Jan. 30, 1856. 4 ⁕s in vF. neby.
521	1	Feb. 23, 1857. E; np; sf; mbM.
522	4	Light *not* equable; stellar nucleus?; ⁕ in n. edge.
526, 527	3	Feb. 9, 1855. Very close, almost touching. 526 is mbM; 527 is smaller, and lbM. Sketched.
529	7	March 9, 1852. e. close; d. neb. Jan. 20, 1857. These two are equal in size, and enveloped in F. haze of neby.

Number in Herschel's Catalogue.	Number of times observed.	Description.
530	7	B. nucleus, surrounded by very extensive neby.
531		Coarse cl.
532	7	Dec. 29, 1851. vL. lenticular ray, slightly concave towards np. direction; gvmbM; perhaps 10′ long. March 1, 1854. Uncertain whether nucleus is stellar. Query, parallel dark lines exterior to nucleus as in Andromeda. March 8, 1858. ✱ on np. edge is d.
533	1	March 11, 1858. 4 neb. here, nearly in line p. and f.
535	9	d. neb. surrounded with F. neby.
536	6	6 knots in the immediate neighbourhood, two of which have ✱ in their edges.
537	2	✱ with fan-shaped neb, very like Herschel's fig. A 2nd F. star involved in the neby.
538	1	March 1, 1856. F. bM.
540	8	r; d. ✱ in s. extremity; nucleus.
542	1	Feb. 19, 1855. vvF. nucleus; r.
549	1	Feb. 18, 1855. d. neb.
550	1	March 12, 1852. An amorphous mass of neby. of uneven character; E. p. and f.
551	3	Jan. 20, 1857. lE. p. and f; and vlbM.
553	4	Feb. 16, 1858. Nucleus; pB; E. nearly n. and s.
555	3	Dec. 29, 1851. A B. ray like 242.
556 559 561	2	4 neb, one of them vvF. and one E.
557	1	Feb. 23, 1857. vF; lE; lbM.
562	4	vF. ray; np, sf.
563	7	Feb. 16, 1858. Mottled, and suspect spiral; r. March 11, 1858. B. ✱ close f.
564	2	March 26, 1851. vbM.
566 567	1	March 13, 1850. A third, and eF. neb. found.
569	2	lE.
574	1	Jan. 8, 1851. ✱ in edge; R; S.
575	2	Feb. 9, 1855. 2 neb. found; both F. and lbM.
580	2	March 15, 1855. E. nearly n. and s.; has a ✱ touching its nf. edge, and is mbM.
581 582	5	There are here 15 knots. The positions of six of them were taken. March 26, 1851. $\alpha\beta$ 226° 0′ 25″ $\alpha\gamma$ 237 1 12 $\alpha\delta$ 263 5 9 $\alpha\varepsilon$ 125 4 13 $\varepsilon\zeta$ 120 8 8 March 14, 1850. $\alpha\beta$ 235 about 0 30 $\beta\gamma$ 245 about 0 45
584	5	The neb. involves one of Herschel's ✱s.
587	1	Mar. 15, 1855. Has a F. knot close np.
588	1	Nucleus.
589 591	4	3 found; 2 of them E. and lbM.
592	2	✱s in its edges, and suddenly condensed in the centre.
593	5	Feb. 14, 1855. Stellar points in outlying F. neby, especially two, which I can plainly see with the ½-inch single lens; sbM.
594	1	Mar. 9, 1858. E. neb. between 2 ✱s.
597 598	3	Feb. 22, 1857. Fine d. neb, both mbM. and both E, especially the f. one, which seems to have a bend at α. Query, a vF. neb. at β? Mar. 18, 1857. All the particulars of my last observation fully confirmed. "Nova" at β seen.
600	3	Feb. 19, 1855. pF; R; bM. to nucleus.
604	24	(18 times since 1850.) Nothing additional, except 3 ✱s as in diagram. Mar. 9, 1858. Very well seen; central nucleus looks r. A ✱ suspected at α, and one or more in the F. neby. at β; and a ✱ seen at times quite steadily at γ. I employed the inch and ½-inch single lenses. March 11, 1858. Seen as well as on last observation. I have now verified the 3 ✱s which I then noticed.
610	4	Mar. 24, 1857. Much mottled. Mar. 11, 1858. Has a d. ✱ in it.

Number in Herschel's Catalogue.	Number of times observed.	Description.
613	1	Feb. 18, 1855. vF; R; mottled?; ✱ in n. edge.
622 624 627	8	Feb. 1, 1856. 622 has nucleus, and is mE; its light is very unequal, and I suspect one dark lane running throughout its length; s. of nucleus.
626	2	Jan. 30, 1856. pL; vF; R; vgbM.
630	1	Mar. 5, 1851. S; lE; vgbM.
634 636	4	Feb. 19, 1855. The p. one is d; its comp. being immediately p. it, and lE. sp. by nf.
635 637	2	Mar. 9, 1852. n. one has a mottled appearance.
638	1	Jan. 25, 1851. r; 5 "novæ" near the most distant 11′.
639	10	Jan. 24, 1851. r; a S. ✱ near the middle, and another f. lenticular. Mar. 20, 1851. Patch and ✱ in p. end. At 54° 46′ N.P.D. / 9ʰ 36′ R ± A scarlet ✱ of 18th mag.
640 641	1	Several knots near.
642	1	3 "novæ" near.
644 647	2	Feb. 26, 1851. p. one eF.
650	2	psbM.
652	2	Mar. 10, 1852. L. thin F. ray.
656	4	Mar. 15, 1855. Appendage to sp. edge, or rather a twist in that end towards the north.
657	12	vF; seems to have a split in f. end.
659	2	Nucleus or ✱ in M.
660	1	Nucleus or ✱ in M; light mottled; a S. ✱ nf.
661	1	Mar. 21, 1854. eeF. with B. centre; E, principally on f. side.
663	3	Jan. 10, 1856. Lent.; vbM; has a ✱ np. Query, a break in the neb. just p. the nucleus?
665	1	Mar. 18, 1857. Found here a ✱ with vF. neby. nf. it.
667	3	Mar. 24, 1857. pF; S; R; bM.
668	6	Mar. 11, 1848. Fine ray, with vB. nucleus.
671	6	Mar. 12, 1852. E. p. and f.
675	4	S. ✱ sp. edge.
677	1	Mar. 30, 1854. vF.
678	1	Mar. 27, 1854. 3 neb; the p. one vS. About 4′ f. is a S. lent. ray running nf, sp, and s. of this latter is another neb. about 5′ distant; R; both r.
682	8	Jan. 16, 1850. A F. spiral. Mar. 20, 1854. A F. ✱ immediately f.; spiral left-handed; very faintly seen; night bad.
684 685	5	685 seems like 393, but instead of the ✱ having an approach to a nucleus. Jan. 30, 1856. About 5′ sp. 684 is a vvF. ray, extending n. and s. 684 has a B. central nucleus, with a sensible disk.
688	4	Suspected spiral.
689	7	Spiral. Feb. 1, 1856. The neby. connecting the three principal knots is vvF, but no doubt of its existence. Sketch made. See fig. 13, Plate XXVII.
692 693	10	March 15, 1850. 4 neb. here. α is 692, and β is 693. Pos. Dist. $\alpha\beta$ 51° 5′ 50″ $\alpha\gamma$ 302 5 10 $\alpha\delta$ 217 10 47 $\alpha\beta$ 53 5 52 According to Herschel, the distance from 692 to 693 is 4′; this should be carefully looked after. Mar. 22, 1857. Sketched; α and γ r; nucleus of β appears eccentric. See fig. 14, Plate XXVII.
695	5	March 3, 1850. Probably a F. spiral. March 24, 1857. ✱ in f. end; dark spaces throughout its length.
696 699 700	1	Feb. 16, 1855. They form an obtuse-angled triangle; the p. one is accompanied by 2 minute ✱s, one n. and the other nf; the next has also a minute ✱ as a comp. nf.
698	1	vF.

Number in Herschel's Catalogue.	Number of times observed.	Description.
705	1	March 3, 1851. d. ●, with neb. to n.
706		Looked for 5 times; not found.
710	5	March 11, 1858. 2 F. patches of neby. (of which one has nucleus); they form with a ● an obtuse-angled triangle, the intervening space being filled with F. neby. of a mottled character.
711	4	Feb. 18, 1852. pB; bM; E. sp, nf.
713	1	bM.
714	2	March 20, 1854. Dark spaces suspected. Feb. 9, 1855. Has a suddenly B. centre; vmE.
718	1	Has a ● closely aff.
719	2	Rather lenticular.
720	2	March 18, 1857. sp. edge is F, and not so sharp as the rest.
721	2	March 18, 1852. R; nucleus.
724	6	March 8, 1858. There is a B. streak, in which I certainly see ●s sparkling, projecting a little from the edge of neb; the neb. is much mottled, and has a stellar nucl.
727	1	March 11, 1858. F; R; bM.
728	5	Jan. 10, 1856. I think the nucleus is not quite central.
731	12	March 5, 1848. Spiral arrangement well seen. March 11, 1848. Very cold; very windy; air steady; definition excellent; mirror bore a power of 700 with great precision; telescope as steady as a rock, although wind so high. Nebula well resolved into ● points. Saw a broad band at the bottom distinctly, and 2 at the top. March 28, 1848. Resolved by a power of 800, although night hazy. March 17, 1849. Like cl. in Hercules; dark spaces in B. part.
732	1	Between 2 ●s, one of which seemed connected with the nebulæ.
735	1	Nucleus or ● in centre; 8; R.
737	4	March 18, 1857. Mottled; suspect 2nd nucleus.
739	8	Jan. 27, 1852. vF. spiral with B. centre; S. ● sf. centre involved; two others f.
743, 749	14	Both have B. L. centres enveloped in F. neby; much mottled. 743 sketched roughly twice. 749 sketched roughly four times. They are both represented as spirals, though the details are vF.
748, 751, 753, 754	3	Mar. 23, 1851. The triple neb. is probably a spiral; dark spaces in it.
750	4	Feb. 1, 1856. lE; pB; mbM.
755	3	Feb. 23, 1857. mE. n. and s; bM.
756	3	B. streak through it suspected.
757, 758, 761	3	March 17, 1849. 757 vB; L; R. 758 vB; R. 761 E; pB.
765, 766	9	Feb. 9, 1855. 765 is, I think, a spiral, with *left-handed* twist; immediately f. is 766, which is B. and well-defined. I suspect F. neby, extending from 765 and running up through the other nebulæ. Feb. 14, 1855. Seen as before. In 765 the curve to the left is brightest near its extremity. Feb. 16, 1855. Certainly F. neby. extends between the two, as before suspected. Jan. 10, 1856. Nothing to add to former observations. Mar. 19, 1857. Observed to compare sketch. See fig. 15, Plate XXVII.
768	1	Nucleus.
772	1	Another neb. n. 3′ dist.
773, 775	7	Both mottled.
774	1	Sharp nucleus; ● in nf. edge.
777	3	Mar. 29, 1856. A ● in s. edge, and a F. one in f. edge; 2 knots in n. edge. I think it r.
778, 779, 782	3	Feb. 9, 1855. Three in a line; the middle one is vB. and lenticular, and has the larger ● of a d. ● involved in f. end.
783	1	vF; lbM.
785, 787	3	Mar. 5, 1851. At sp. edge of 787 a ring suspected, within which a dark band, α then B. part. Mar. 30, 1856. 785 is E. sp. by nf, and its brightest part is nearest the p. end; also a ● in nf. edge. 787 is very curious. S N A R. bright nucleus, which is eccentric, and a dark curved passage sp. the nucleus, as in sketch. The neby. outside this dark place runs up perhaps to the streak marked α, which is vF, but of its existence I have no doubt.

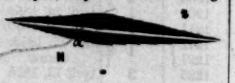
Number in Herschel's Catalogue.	Number of times observed.	Description.
786	1	Stellar pt. or nucleus E.
788	6	Jan. 1850. Probably very remarkable; bad night. Feb. 1, 1851. f. division the brighter. Mar. 3, 1851. p. division pretty well seen. Mar. 8, 1856. mE; certainly dark spaces on each side of nucleus, but not well seen; that on f. side is the more distinct. Sketched roughly 3 times.
789	1	Jan. 21, 1855. pL; considerably E; BM, but no nucleus.
790 791	2	Mar. 28, 1856. About 3′ apart; both F. and of nearly equable light. The n. one is a long narrow ray np, sf; the other is oval sp, nf.
793	1	A S. comp. dist. about 5′ or 6′.
804	3	April 9, 1852. I suspect a dark curved passage sp. centre. Mar. 15, 1855. Light mottled; I suspect a knot in p. and one in f. edge. Has a spiral appearance.
805	6	Mar. 12, 1855. Has a sharp B. R. nucleus in a disc of F. mottled neby.
806	2	Mar. 17, 1855. Oval; major axis nearly p. and f; nucleus vB.
810 815	4	Feb. 22, 1857. mE; B. nucleus; arms F; patchy. Mar. 23, 1857. pL; nucleus vB, and has a sensible disc; arms vF. and patchy. 815 is F, nearly R, lbM.
811	1	R; gbM.
812	1	Query, is there a F. ring round it?
813	5	Mar. 1, 1854. Query, an oval spiral?
814	2	April 13, 1852. Neb. does not appear to reach the ✱.
818	4	Very like H. 2172. See figure. Mar. 29, 1856. The nucleus projects into the space along sp. edge; outside this dark space there is F. neby, which I see joining the neb. at n. A F. ✱ at the opposite extremity.
831	3	April 3, 1851. Light mottled; vBM; knot in p. branch.
838	42	April 13, 1850. But one ✱ seen. Feb. 1, 1851. 2nd ✱ not seen; sky milky. March 3, 1851. 2nd ✱ not seen; sky milky. March 5, 1851. 2nd ✱ not seen. March 7, 1851. 2nd ✱ not seen. April 3, 1851. 1st ✱ only seen. Jan. 27, 1852. Only one ✱ seen. March 12, 1852. Only one ✱ seen. March 13, 1852. Only one ✱ seen. March 20, 1854. 2nd ✱ not seen, nor any of the F. details. March 30, 1856. 2nd ✱ not seen, nor minute details. March 24, 1857. 2nd ✱ not visible. March 8, 1858. 2nd ✱ not visible, nor minute details. N.B. The 2nd ✱ has not been seen since March 9, 1850.
840	5	March 17, 1855. mE. p. and f; vB. centre; the n. edge of central part seems sharpest, and outside it again I think there is F. neby; ✱ in f. edge. A rough sketch represents it like 2172.
841	3	Suspected spiral, but a vF. object.
843 844 845 846	4	844 is a B. nebulous disc in a F. oval neby.
847	1	Mar. 19, 1852. E. np, sf; vB. centre.
848	3	Feb. 18, 1852. vbM; lE.
849	1	Mar. 29, 1856. S; pB; R; mbM.
851	1	"Nova" near; both are S; F; lbM; and 851 has nucleus.
854	10	Mar. 31, 1848. Curious neb. with B. nucleus at left; a little above and towards the right is a streak; spiral; resolved very well about the nucleus, but no other part. From the right, and apparently springing from the nucleus, a vF. portion of neb. extends for nearly 15′, gradually melting away. Apr. 3, 1848. Observed with the same results as on March 31st. April 17, 1849. 2 ✱s near nucleus, one sp, the other sf. it. Feb. 25, 1854. Suspect dark spaces on either side of nucleus. Mar. 1, 1854. Neb. mottled; p. observation confirmed.
856	2	2 neb. found; the p. one has a sudden vB. nucleus, and is lE. np, sf; the other is about 15′ f; S; R; pF; vlbM.
857	4	Suspected darkness on either side of nucleus; E. See fig. 16, Plate XXVII.
858	4	Apr. 15, 1852. R. disc, BM, with vF. neby. round it of mottled character; probably it will be seen as spiral on a fine night. Mar. 30, 1856. Spiral with, I think, two arms, thus: these arms are broken and of unequal light; there are B. patches at α, β, and γ respectively; a F. ✱ p. at δ. Apr. 6, 1856. Seen as spiral. The f. branch comes down past the other, doubtless over it as at α, and seems to originate from the p. side of nucleus. Mar. 24, 1857. The spiral arms are eeF, but there is no doubt of their existence as described in previous observations.

Number in Herschel's Catalogue.	Number of times observed.	Description.
859	2	Apr. 1, 1848. pB; very long.
860	1	Apr. 9, 1852. I see nothing but a F. neb. 60″ near some ⁕s of 8th and 9th mag.
865	1	bM.
866, 869	1	Apr. 13, 1852. Large neb. is BM. It has a knot in sp. end, and a dark curved passage on p. and n. sides of centre; spiral. Small neb. f. has a S. ⁕ immediately s. of it.
875	6	Sketched 4 times. Feb. 19, 1855. 3 ⁕s in it; there is a mass of neby. f. the brightest part, with condensed portions through it. Disposed in curves? The F. ray extends many minutes s, gradually fading away. Mar. 17, 1855. There seems to be a knot at p. extremity, in which the neb. terminates in that direction, and immediately s. of this knot is a little dark bay. The branch running f. from this curves round towards centre. See figure.
879	1	Apr. 16, 1852. F. brush; night bad.
881	1	Nucleus.
882	2	Mar. 22, 1857. mE. sp, nf. and bM.
887	2	Mar. 17, 1849. Dark space f. centre strongly suspected.
891, 893, 894, 898	4	Mar. 26, 1856. Of this group 894 is the largest and brightest; its light is patchy.
895	2	Mar. 28, 1856. Irreg; R, edge ragged; sbM; nucleus.
896	2	Jan. 27, 1852. Neb. divided into two parts, and F. appendage np. Apr. 15, 1852. Black line across; comp. scarcely visible.
897	3	Feb. 22, 1857. lE. sp, nf; gbM. to F. nucleus.
901	1	Mar. 23, 1857. F; E. np, sf; lbM.
903	1	E; vbM.
908, 911	3	Jan. 27, 1852. 908 mottled, with S. ⁕ involved; sp. it is a coarse d. ⁕. 911 is irreg, with B. ⁕ in s. edge, and having dark lanes through it.
910	13	Mar. 30, 1856. Examined attentively for a long time; it appears to be of the shape annexed, which exaggerates; there can be no doubt of the bend upwards at α, and of the darkness about the nucleus; S. ⁕ at β. Apr. 6, 1856. Seen pretty much as before; the upward bend at α is at a right angle. The p. branch reaches as far as γ; and I suspect a S. ⁕ there. Mar. 8, 1858. This night is not as good as some on which I observed this object last year, but I can confirm my previous observations as to its general shape.
918	1	Mar. 3, 1851. E; in the meridian vlbM. Another brush-like 20′ np.
923	1	Mar. 22, 1857. vS; R.
925	2	There is an appendage, perhaps an independent neb; r?
930	3	Between 4 ⁕s, in the shape of a trapezium.
931, 932, 933	1	Feb. 24, 1852. 2 rays, forming an angle of about 100°; the s. one has a nucleus, and there is a knot at the n. extremity of the other.
939, 940	3	2 "novæ" near, probably a 3rd. 933 and 940 are E, the others R.
936	2	Apr. 1, 1848. A tolerably B. neb. with a smaller one f.
943	5	Spiral. Apr. 18, 1851. BM; F. neby. all round of a mottled character, knot or appendage in p. part. Apr. 10, 1852. Spiral? gbM. Mar. 1, 1854. Spiral arrangement; sky milky.
945	1	Apr. 11, 1850. Several ⁕s near it, but few others in neighbourhood.
946	3	Mar. 8, 1858. S; lE. and pF.
947, 950, 951, 953	2	All are S, R, and lbM.
948	1	bM.
959	1	Mar. 3, 1851. S; lenticular.
960	1	Feb. 17, 1855. A large number of pB. nebs. knots; I counted 8, probably there are more.
967, 968, 969	1	Mar. 28, 1856. The p. one is E. p. and f; the others are R; bM.

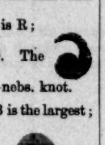
Number in Herschel's Catalogue.	Number of times observed.	Description.
971	1	Mar. 29, 1856. Neat little ray np, sf; bM.
973	1	
978	1	Mar. 13, 1852. Oval; F. nucl; another F; S; 5′ nf.
980	1	Jan. 27, 1852. gbM; R; S.
981	2	April 13, 1855. Dull nucleus; edge ragged.
982	4	April 15, 1852. Spiral probably; knot in s. edge, and a * outside p. edge; another S. neb. 3′ sf, having * immediately n. of it. April 16, 1852. Spiral; last night's observation confirmed; the spiral branch seems to start from the s. edge and go round the f. and n. sides as far as the * p. April 19, 1857. A * np. and a * in s. edge. Seen thus:—The spiral branch is B. and easily distinguished at sp. edge (α); as it extends to f. edge it grows fainter, and I can trace it no further than β. The central neb. is vB, and has a B. nucleus. The S. neb. sf. is BM. and a lE. Apr. 20, 1857. Examined with 1-inch and ¼-inch single lenses; last night's observation is correct.
983 } 984 }	1	S. * p. 983 about 1′.
985	1	Mar. 27, 1854. vF; r?
988	2	Feb. 24, 1852. lE. n. and s; bM.
992	2	Mar. 7, 1851. E; bM; nucleus.
994	1	5′ long.
1002	3	Mar. 17, 1849. Suspect it to be a spiral; though twe saw at moments ring round nucleus. Apr. 21, 1851. Spiral of the faintest class; the M. is pB, but the branches vF; conjectured form thus Apr. 17, 1855, or thus .
1005	3	Apr. 11, 1850. Fine neb, but very bad night.
1006	1	vg. vlbM.
1008	2	* in nf. side; vF; E; B. nucleus.
1009	2	Apr. 13, 1852. Oval; gbM.
1011	4	Mar. 3, 1851. Lenticular; mottled. Mar. 30, 1856. mE. sp, nf; B. nucleus; very much mottled; the larger half of neb. lies to s. side of nucleus. A B. streak running obliquely through the nucleus, and another B. patch to s. end. Apr. 6, 1856. I see two patches in s. end, also a *. Apr. 19, 1857. Sketched. See fig. 17, Plate XXVII.
1014 } 1015 }	1	Apr. 14, 1852. The s. one is E; the n. one has 2 *s involved.
1017	1	Mar. 27, 1854. Filamentous; r; * near centre.
1018	3	Jan. 10, 1856. pB. nucleus in a L. mottled disc of F. neby. Irregularly R. Another nf.
1022	1	Jan. 27, 1852. Long ray; gbM.
1030	2	Apr. 15, 1852. The neby. p. centre is mottled.
1029 } 1031 }	1	The p. one is S. and the f. one vB.
1033	4	3 "novæ"; one is S. and R, the others are E.
1038	4	Mar. 27, 1856. mE. n. and s; smbM. to a B. nucleus; a d. * involved in n. extremity; a B. * further distant n.
1040	1	Apr. 13, 1852. mE. sp, nf; * p. a S. R. neb. about 7′ np. it.
1041	1	Mar. 17, 1849. Roughly sketched; E, with a split or opening in the direction of major axis, and a * a little f. centre.
1043	5	Mar. 30, 1854. F; spiral? another neb. np. or nearly n; vF. about 5′ distant. Apr. 6, 1855. Query, of this form? Its light is certainly patchy, and the neb. is lE. nearly p. and f; np, this object is another F. R. neb. with stellar centre. Apr. 13, 1855. Suspected shape as before, stellar centre. Apr. 16, 1855. My previous conjecture as to shape is rather confirmed by Mr. J. Stoney, who saw the p. branch turned off sharply to s. (nearly at a right angle), whereas the f. bend is not so sharp; but this latter branch reaches further round and is rather fainter. The whole object is vF. Mar. 27, 1856. Last year's observations fully confirmed.
1045	1	Apr. 26, 1851. Bicentral appearance is very indistinct; the light is mottled; E. ssp. and nnf.

Number in Herschel's Catalogue.	Number of times observed.	Description.
1048	2	Mar. 29, 1856. pL; B; mbM; r.
1049	1	Mar. 15, 1855. pF; R; lbM, but no nucleus.
1051	1	Apr. 18, 1851. S. ✱ involved in f. part of it, precedes a ✱ of 9th mag. 5′.
1052, 1053	3	Jan. 27, 1852. Spiral. Apr. 9, 1852. Previous observations confirmed; S. ✱ np. it. Apr. 14, 1852. Drawing made. See fig. 18, Plate XXVII.
1058	1	Apr. 10, 1852. gibM; F. neby. round it; S. ✱ south.
1061	4	Apr. 27, 1851. Spiral; I suspect the f. branch extends to α. ✱ suspected at ρ. Pos. Dist. Nα 64° 2′ 50″ Nβ 256 2 19 Nγ 228 3 17 Nδ 15 3 53 Apr. 29, 1851. Observed for drawing. May 3, 1851. Viewed in twilight; drawn. Apr. 19, 1857. The p. branch seems the brighter rather of the two, and more suddenly curved than the f. one, and both of them look not quite so sharp as given in the drawing. See fig. 19, Plate XXVII.
1062, 1063, 1064	1	Badly seen.
1066	2	Mar. 12, 1850. Broad equable band; several conspicuous ✱s in it, especially near ends.
1081	1	Apr. 16, 1852. vS. ✱ p. and a little n. of centre; I suspect another in n. branch; gbM.
1084	1	Apr. 16, 1852. Nucleus.
1085	5	Apr. 13, 1852. Brightest part a little eccentric; ✱ p. is involved. I suspect (Mr. B. Stoney) a dark curved passage on s. of centre, probably new spiral. Mar. 30, 1856. I have little doubt this is a spiral, either ✱s , which I rather believe, or , a S. ✱ p. Apr. 6, 1856. I think spiral with one branch; a B. part at α, and I suspect a ✱ there. Mar. 24, 1857. Nothing to add to previous observations, which, however, I can fully confirm. Apr. 19, 1857. Observed.
1088, 1091	2	Apr. 21, 1851. 1st vF; 6′ ssp 2nd; 2nd vB. and mE; a d. ✱ 5′ nf, whose smaller component is blue.
1092	3	Apr. 1855. Two neb. about 14′ distant, 45° nf. Is the s. one of this shape, with a wedge-shaped division running downwards? The other neb. is lE. np. by sf; has nucleus, and is the larger and brighter of the two. Mar. 29, 1856. Last observation confirmed as to the shape of the s. one; the north one is, I think, a spiral of this shape; the branches vF.
1094	2	Feb. 26, 1851. A long ray, much resembling 242.
1105	4	April 13, 1855. pB; R; bM, but no nucleus.
1106	2	April 1, 1848. A very close cl. of faintish ✱s, preceded by a S. neb.
1107	2	March 9, 1850. A long ray with mottled light.
1108	3	April 17, 1855. Has a B. R. nucleus, surrounded by much F. neby, which is patchy and involves a B. ✱.
1110	1	April 13, 1850. E. np, sf; 88″ by 50″.
1111, 1113	4	April 26, 1851. 1111 has a B. R. centre, with nucleus; then two dark spaces concentric, with nucleus; and outside these F. neby, as in figure. (δ) 1113 has F. nucleus, or stellar point. αN 175° 2′ 25″ αβ 183 0 58 αγ 204 3 01 αδ 66 3 47 April 28, 1851. Previous observation rather confirmed; the dark spaces certainly exist, but I cannot be sure that appendages are not part of spiral branches. April 15, 1852. Last year's observation confirmed as to dark curved spaces p. and f. centre, and F. neby. outside them again. See fig. 20, Plate XXVII.

Number in Herschel's Catalogue.	Number of times observed.	Description.
1117	3	April 25, 1854. R; has nucleus; * involved f. nucleus.
1119	1	Feb. 17, 1855. vB; R; bM; has 2 S. *s p.
1120, 1121, 1122, 1124	1	Jan. 28, 1849. Observed in haze.
1128	1	Apr. 6, 1856. F; bM; a B. * in sf. edge and a patch in np. end. Neb. is fully 4′ long.
1129, 1136	2	Feb. 26, 1851. The larger is vlbM; perhaps not R; S. one r.
1131	3	March 17, 1855. L. R; nucleus; * in nf. edge; mottled.
1132	1	March 9, 1850. A ray; diminution of light in neighbourhood of nucleus; edges parallel; night bad; remarkable object.
1140	3	April 6, 1855. Very like a distant cl.
1144	1	April 14, 1852. The brightest part in advance of the centre; vS. * n.
1146	6	March 8, 1856. Irreg. shaped neb. with nucleus eccentric, and a knot or appendage at f. end. March 27, 1856. There are 4 knots or *s in the neb, besides the B. patch to sf. side of nucleus.
1147	1	March 6, 1851. S. lenticular ray; B. nucleus.
1148	1	No description.
1149	1	March 15, 1849. Lenticular, with split in direction of major axis.
1155	1	No description.
1156, 1158	1	April 6, 1855. Both are R; pB; bM.
1160	1	April 18, 1855. pF; L.
1162	2	April 25, 1854. E. p. and f; bM.
1167	1	March 9, 1850. Great ray; night bad.
1168	2	April 10, 1852. Has E. appearance np, sf; F. neby. all round it.
1171	1	March 13, 1852. E. p. and f; nucleus.
1173	7	See the 'Transactions' for 1850.
1175	4	April 20, 1857. A vL. B. E. neb; much mottled. The f. edges are comparatively sharp and well defined, but in the p. and n. edge there is a great inequality of light; nucleus E; vB. part to n. of nucleus.
1176, 1180, 1178, 1183, 1187, 1189, 1190, 1194, 1201	1	April 16, 1852. gvbM; the f. one is much fainter.
1179	1	No description.
1185	2	April 10, 1852. L; E. p. and f; arms F; * involved in p, and another * in f. arm, but a little further from centre.
1186, 1188	2	April 26, 1849. 3 in line; the f. one vF, the other two R; pB. nuclei.
1195	1	April 10, 1852. F. knot at end of p. branch.
1197, 1200	1	April 14, 1852. 1st E. * in np. extremity; 2nd F, almost planetary; another vF. and thin ray about 30′ f.
1196, 1202	5	Sketched 4 times. March 1, 1851. 1196 is bM, and has a vF. comp.; 1202 is a spiral, B. centre, and 2 knots. There is another neb. 10′ nf. About 84° 34′ N.P.D., 12h 25m R.A. There is a scarlet * 10m. and a F. E. neb. 10′ s. of it, with *s in it. See fig. 21, Plate XXVII. April 9, 1852. Last year's observations confirmed.
1204	2	March 15, 1855. pL; mbM. to a sharp nucleus; mE. p. and f.
1209	1	April 24, 1854. Lenticular nnf. by ssp; F; vlbM (night bad).
1211	1	March 9, 1850. Spiral; a F. neb. f; roughly sketched.
1212, 1221	1	Feb. 17, 1855. 1212 is B, R, and smbM. 1221 is vB; mE. sp, nf, with a suddenly B. centre.
1225	3	April 13, 1855. vB. globular centre; E. p. and f.

Number in Herschel's Catalogue.	Number of times observed.	Description.
1231	1	April 10, 1852. Not R.
1232, 1236	1	March 6, 1851. vbM; edges fade off. 1236 is vF.
1237, 1250	3	March 13, 1852. 6 knots, one E. March 1, 1854. One has dark spaces about the nucleus. March 15, 1850. 12 knots examined.
1239	1	No description.
1240	1	April 24, 1854. R; B. nucleus; outline somewhat irregular.
1242, 1251	1	March 6, 1851. Both BM; B. ✱ involved in 1st; 2nd is E.
1245	4	March 30, 1856. pB; E; nucleus. A B. streak runs up through the nucleus, growing broader at p. end; on either side of this I suspect dark spaces, and outside them again F. neby, especially to s. side of nucleus. April 19, 1857. Much E; instead of a nucleus it has a vB. narrow central streak; to the left of this I suspect a darkness; then outside this more F. neby, as in sketch. See fig. 22, Plate XXVII.
1252	2	April 17, 1855. There are here 4 neb; the 3 f. ones seem to be involved in a mass of F. neby.
1253	1	April 15, 1852. gvmbM; oval. Another 14′ sp; also vB.
1258	4	Sketched 3 times. April 12, 1849. Uncertain whether a d. nucleus, or nucleus and ✱; neb. decidedly darker in middle, following the nucleus, and rather brighter outside this. March 7, 1856. d. nucleus, or nucleus and ✱, which are eccentric, being nearer the sp. side; light uneven and patchy; suspect a darkness nf. the nucleus. March 8, 1856. Last night's observation confirmed. March 18, 1857. Seen as in the rough sketch subjoined; a ✱ close sp. nucleus.
1262	1	Feb. 16, 1855. E. sp, nf.
1271	1	April 25, 1854. L; svmbM. to a nucleus; pmE. p. and f.
1274, 1275	1	April 13, 1849. Found in this set 11 knots, of which 6 are 1203, 1237, 1244, 1253, 1274, and 1275; the remainder are "novæ," one of these latter being hollow in middle; probably a ring seen obliquely; a F. ✱ n. of its middle; seen best with single lens. Remarkable object.
1280	2	March 26, 1856. E; B. nucleus; F. extremities.
1281	1	March 17, 1849. 3 nuclei, or 2 nuclei and ✱, and F. neb. outlying.
1282	1	April 11, 1852. gbM; oval; E. n. and s.
1286	1	April 18, 1855. Like a distant cl; vB. nucleus.
1294	1	Feb. 26, 1851. 4 found.
1296, 1298, 1301		April 10, 1852. 1301 is vgvbM. 1298 is smaller, and much the same character.
1306, 1308	4	Sketched twice. March 28, 1856. A rough sketch made; suspect spirality in the n. one; the large neb. has an appendage n. of nucleus and a little f. it. March 24, 1857. Examined to confirm drawing, which I think is pretty accurate. See fig. 23, Plate XXVII.
1309, 1315	2	A d. neb.
1312	2	March 9, 1850. Another spiral; dark spaces, especially one sf. nucleus.
1332	1	
1333	1	April 22, 1854. E. n. and s; nucleus vB; light uneven.
1337	2	April 19, 1855. Seen by myself, as represented (see fig. 24, Plate XXVIII.). Mr. Stoney, who was with me, did not see the F. curve at p. extremity, which therefore needs verification. I myself felt pretty certain of it. March 29, 1856. Seen as last year; sketched. See fig. 24, Plate XXVIII.
1343, 1348	1	April 16, 1852. gvbM; 2 others, both E. about 20′ s. of 1348.
1345	3	Feb. 19, 1855. E. p. and f? B. nucleus.
1352	2	April 11, 1852. A vL. rsy; gbM; some ✱s involved.
1357	3	Roughly sketched twice. April 17, 1855. A beautiful object, very well seen in finding-eyepiece; the whole neb. (taking into account the appendage) is much broader at nucleus than elsewhere, narrowing off suddenly, and the nucleus projects forward into the dark space; and immediately opposite this the F. 

Number in Herschel's Catalogue.	Number of times observed.	Description.
		appendage is broadest and brightest. The ray is 12′ or 14′ long, and there is a F. ✱ at a. (Mr. Stone was with me). April 6, 1856. 15′ long, perhaps even longer; the ✱ opposite the nucleus is about two-thirds the breadth of the neb. distant.
1358 } 1359 }	2	April 14, 1852. A curious d. neb; some other nebs. p.
1362	3	March 19, 1857. R; bM; L. but F; ✱ involved in p. edge.
1363		Looked for twice; not found. Query, Is this 1358 and 1359?
1368	3	May 3, 1851. gmbM; lE. sp, nf; edges fade off very gradually.
1382	2	April 24, 1854. pB; has nucleus; E; F. ray f. April 25, 1854. Seen as last night, also the F. ray f; about 50′ p. is a B, R, pL. neb. f. a B. ✱.
1385 } 1392 }	6	Sketched 3 times. April 10, 1855. Somewhat curved, like 2205. The s. branch is patchy, having 2 B. spots (see fig. 25. Plate XXVIII.); the n. branch is much the brighter. A S. ✱ p. the neb. About 6′ or 7′ n. of 1385 and a little f. is 1392, not so F. as Herschel describes it; the brightest part seems eccentric, being nearer the nf. edge. From this B. part I suspect a curve round n. to sp. April 13, 1855. Seen as before. March 8, 1856. The comp. n. (1392) I suspect, as before, to be a F. spiral. March 27, 1856. Better seen than on any previous occasion; the F. branch to the left extends round as far as the p. extremity of the B. branch. The comp. neb. suspected to have a twist in it, as before; sketched.
1386	2	April 11, 1852. gvbM; E. np, sf.
1397		See the 'Transactions' for 1850.
1402	1	March 1, 1854. d. neb; F. neby. connects them.
1403	2	April 22, 1854. A remarkable object; spiral?
1408	2	April 5, 1851. L; vB; comp. neb. pB.
1409	1	March 7, 1856. lE. nearly p. and f.
1411	1	Feb. 26, 1851. p. part is broadened out; light unequal; night bad.
1414 } 1415 }	3	April 26, 1851. Herschel's two neb. form one, the joining part in middle F, and vF. production of neb, as in sketch. April 9, 1852. Last year's observation confirmed; like a caterpillar on a leaf. April 20, 1857. I can confirm former observations in every particular, and think there are two additional ✱s in f. part. See fig. 26, [Plate XXVIII.
1431	3	Spiral?
1436	1	April 13, 1852. gvmbM; S. ✱ involved in f. part.
1437	1	April 15, 1852. gvmbM; oval.
1441	8	Feb. 16, 1855. vB. ray; a dark band across on each side of nucleus, separating it from the extremities. Feb. 17, 1855. Sketched. Feb. 19, 1855. The dark spaces which are visible in finder are not black, but only portions of fainter neby. April 6, 1855. Seen as before; dark lines very plain in finder. April 16, 1855. My sketch exaggerates the dark lines; they should be broader, and not so well defined. Mr. Stone remarked a second dark line across the n. branch near its extremity. Mar. 7, 1856. Observed. Mar. 18, 1857. Dark spaces far apart, and not absolutely dark; suspect a dark space to right-hand side of nucleus. See fig. 27, Plate XXVIII.
1451	4	March 9, 1850. Another spiral; another neb. 15′ p. Feb. 26, 1851. Spiral; 2 arms, and some ✱s in f. arm; centre is B. 12′ p. and a little s. is another neb, E; and 30′ nf. is a 3rd, E. n. and s. April 15, 1858. vL. and vB. The centre itself is like an E. neb, with nucleus; this centre is enveloped in an irreg. ring or rings of nebulous light, as in the accompanying rude sketch, which does not contain all the details. sp. this object there is a S. neb. E. np, sf. and very patchy, and I suspect it to have a F. nucleus. May 3, 1858. I saw all the details in last observation, except that there was only one ✱ visible s. of nucleus instead of two, but this is not quite so good a night. The surrounding ring of neby. is of irreg. shape; it curves gently at δ, but bends more sharply at γ, where it is brightest. The centre seems to reach up to and blend with the neby. at δ.
1456	5	April 9, 1852. Spiral; bears great resemblance to 1111, April 14, 1852. F. neby; 2′ radius extends all round, in which I think I see traces of spirality which exist certainly in the central part (Note by Mr. B. Stone). A good night and speculum in good order would probably show this object distinctly. April 13, 1855. vlE. p. and f; dark ring round the nucleus; then B. ring exterior to this. The annulus, however, is not perfect, but broken up and patchy, and the object will probably turn out to be a spiral. There is much F. outlying neby. March 8, 1856. Annular at first look, but ring not perfect; centre vB.

Number in Herschel's Catalogue.	Number of times observed.	Description.
1460	1	April 25, 1854. mE.
1462	3	March 7, 1851. gvmbM.
1466	2	March 7, 1851. 8′ long; R; centre vB.
1475	3	March 1, 1851. Nucleus 2′ s. of ✱ of 10th mag. At *R* 12ʰ 43ᵐ and N.P.D. 60° 20′; "nova," with nucleus; E.
1486	11	March 11, 1848. Curious circular-shaped neb, with a dark and large spot at one side, around which is a close cl. of well-defined little ✱s. May 4, 1851. E. nearly p. and f. Herschel's dark space is a curved passage, extending from p. round the f. side of the nucleus by the n.
1498	3	Feb. 16, 1855. L. B. ray; nucleus oval and vB; there is a ✱ involved in n. edge, a little preceding the nucleus.
1499	1	Apr. 17, 1855. vF; mE. sp. nf; has a plainly seen ✱ at n. end, and either a ✱ or what looks more like a B. little knot involved in s. end.
1500	1	Numerous neb. around.
1509	4	Apr. 18, 1855. Looks sometimes like 838 when badly seen, with a B. E. patch in centre and dark spots on each side of this; sometimes dark ring is seen all the way round, but blackest to right and left. The neby. round it is mottled. In looking for this I found at about *R*. 12ʰ 48½ᵐ, and a little n. of this set a F. d. neb. E. at right angles to each other. Mar. 29, 1856. Last year's observation correct; ✱ in sf. edge; sketched. Apr. 24, 1857. Long and carefully examined; the B. centre is E. in the direction of ✱ on edge, and on either side of centre there certainly exist dark spaces, as before remarked, giving it the look of 838; yet sometimes I thought I saw it with a break in the outer annulus.
1515	1	Mar. 24, 1857. vvF; lbM; vlE. np, sf.
1525	1	Apr. 27, 1854. vF; R.
1536	1	Apr. 18, 1855. Like a distant cl; 2 B. ✱s involved.
1547	3	Mar. 12, 1852. B. lenticular ray with E. centre. May 3, 1858. Sketched; like 2172 and 1357.
1549	3	May 3, 1856. gbM; B. nucleus; B. ✱ in np. end. April 15, 1858. Very much mottled.
1551	1	Apr. 22, 1854. E. p. and f; bM.
1556	1	Spiral?
1558		cl.
1559	3	Mar. 12, 1852. Light equable; E. sp, nf.
1562	1	Mar. 24, 1857. vF; lbM; lE.
1564	2	Mar. 1, 1851. vB. centre; has an appendage parallel to major axis.
1569		cl.
1570	3	Spiral? darkness sf. nucleus.
1576, 1577, 1578	3	A group of 4.
1580	2	Apr. 25, 1854. R; bM; between 2 ✱s.
1589	7	Sketched three times. Apr. 29, 1856. The B. centre is E. but not in the direction of the neb. The whole neb. is much mottled. Apr. 15, 1858. I can add nothing to my drawing and observations of last year, which are fully confirmed. See fig. 28, [Plate XXVIII.
1599, 1600	2	Apr. 13, 1855. Both are S; R; pB; bM. Apr. 17, 1855. There is a 3rd vF. neb. nearly n. of the f. one of these two.
1604, 1605	2	Mar. 28, 1856. 1604 is lE; pB; has nucleus; and a ✱ at np. end; 1605 is R; vF; and its light equable.
1622	19	Carefully observed since drawing published in the 'Transactions' for 1850. The outer nucleus unquestionably spiral, with a twist to the left; thus
1626	2	Apr. 19, 1855. Oval; bM; ✱ np. May 3, 1856. About 5′ nf. it is a vF. nebs. knot.
1638, 1639, 1643	2	May 3, 1856. 1638 is E; nearly p. and f; bM; 1639 is S; R; bM; 1643 is the largest; pB; R. and gbM; nucleus, round which I suspect dark spaces.
1647	1	Apr. 19, 1855. Not L; gbM. to a nucleus; mottled.
1650	3	Apr. 19, 1855. L; pb; B. nucleus; seen as in sketch, but not certain whether the lower branch joins the nucleus, or is only the continuation of the upper curve. Mar. 21, 1856. The p. arm *does* appear to originate from the nucleus, which is vB. and oval-shaped. Mar. 30, 1856. Seen as before. See fig. 29, Plate XXVIII. 
1658	3	Mar. 28, 1856. F. ray n. and s; no nucleus; light; equable.
1659	2	Mar. 24, 1857. S; vF; nearly R; brightest part is on sp. side of centre.

Number in Herschel's Catalogue.	Number of times observed.	Description.
1663		Splendid cl.
1664	3	Mar. 27, 1856. pL; pB; R; sbM; about 2′ or 3′ f. is a S. F. neb.
1668	1	May 3, 1850. A single B. ✱ at n, and a d. ✱ at s. end of this neb. Another neb; R; bM; sp.
1669	1	Apr. 11, 1852. A vF. amorphous-looking neb; S. ✱ in s. edge.
1672	1	Mar. 1, 1851. ✱ or nucleus in np. edge; 2nd vF; 3′ s; both E. p. and f.
1676 1679 1680	2	All F.
1695	1	May 15, 1854. vF. in twilight; lbM.
1697	2	Feb. 19, 1855. mE. p. and f; L; pB; gbM. Mar. 24, 1857. Found here 3 neb. in a line sp, nf; all of them are bM.
1703	1	Apr. 14, 1852. gbM; L. vF. neb. 14′ s. of 1703; also a S, F, E. neb. 15′ p. and 2′ n. of 1703.
1711	4	Mar. 28, 1856. S; bM; dull nucleus; lE.
1713	6	Apr. 24, 1854. Centre pB; oval n. and s, and among several ✱s; I thought the n. end the broader, and suspected a dark space p. the nucleus. May 1, 1854. Singular object; the main body of neb. has a B. nucleus, and is E. n. and s; the southern end bends back suddenly at a sharp angle, and extends np. past the neb, ending in a B. R. patch or nucleus; 3 ✱s around the neb. Apr. 17, 1855. Mr. Stoney saw the p. branch extend *round* the s. end of the main neb. and continue on to n, when after a second turn it joined the nucleus. See fig. 30, Plate XXVIII.
1714	3	Feb. 19, 1855. pB; R; bM. to a nucleus.
1715 1716	1	Mar. 9, 1851. 3 found; all S; F; R.
1734 1735	5	Apr. 18, 1855. The n. one is spiral?; 3 ✱s in it; to myself it appeared to have a single branch running from below the nucleus round the n. and f. edges. Mr. Stoney suspects two branches. May 10, 1855. n. one suspected spiral as before; the s. one is, I think, lE. n. and s, and the ✱ between the two neb. is d.? Mar. 29, 1856. Suspect n. one as before; it is a very difficult object, and requires a fine night. Apr. 24, 1857. Last observation fully confirmed as to spirality of the n. one. I still think it has but one branch. The ✱ between the 2 neb. is d.
1741 1742	3	Mar. 28, 1856. 1741 is S; R; bM; pB; 1742 is S. ray nf, sp, and has a ✱ of 12th mag. at its s. extremity.
1743	1	Mar. 17, 1855. mE. p. and f; nucleus. Query, a knot in p. branch.
1744	8	Sketched 3 times. Mar. 1, 1851. Large spiral; faintish; several arms and knots; 14′ across at least. See fig. 35, Plate XXIX.

April 27, 1851.		Pos.	Dist.
	αN	195°	1′ 22″
	αβ	345	1 50
	αγ	273	3 31
	αδ	74	3 1
	αε	74	1 38
	αn_1	99	5 37
	αn_2	135	5 2
	αζ	10	5 19
	αη	358	5 33
	αθ	44	5 1
May 3, 1851.	αι	240	8 34
	ακ	118	3 34
	αλ	112	3 29
	αμ	72	4 0
	αν	211	6 31
	γ_1	263	2 56
	γσ	220	3 47

1745	1	Mar. 29, 1856. Has a nucleus; light very patchy; 3 ✱s in edge; vF. Query, spiral, with a right-handed twist? About 4′ f. is a S. pB. E. knot.

Number in Herschel's Catalogue.	Number of times observed.	Description.
1746		Close, rather F. cl.
1754	2	Mar. 27, 1856. S; bM; mE. np, sf.
1755	1	Mar. 9, 1851. E.
1757	1	Mar. 29, 1856. 2 neb. 3′ apart; n. one vS; bM; the other a ray p. and f; nucleus.
1762	2	Apr. 13, 1850. 3 knots near.
1764	1	Apr. 19, 1855. Long narrow ray, with a S, R, vF. neb. sf. About 15′ np. of 1764 is another vF; and about 6′ p. and 1′ n. of this last is another eeF.
1766	2	Apr. 13, 1852. bM; S. ✱ s. of it. Mar. 30, 1856. E. nearly n. and s; S. ✱ sf; B. nucleus.
1768 1769	1	Mar. 1, 1851. 1768 S; F; E; 1769, nucleus.
1770	1	Mar. 6, 1851. Another 5′ p, and another 10′ sp; vF.
1771	5	Apr. 10, 1852. Either a d. neb, or 2 knots of one neb.
1773	2	Apr. 29, 1856. mE; not F; lbM; major axis sp, nf.
1774	1	May 10, 1858. S; irreg; R.
1776		Frequently observed; nothing certain.
1778 1779	4	"Nova" near; 1st E, 2nd bM, 3rd vF.
1782 1783	4	May 12, 1850. 1782 pB; L; gbM. 1783 vB; R; nucleus. Another L. F. ray about 16′ nf. 1783.
1788 1789 1791	2	Only two found; both S; R; bM.
1790	1	May 15, 1854. pL; vF; lbM.
1792 1793	2	Both F.
1797	2	Mar. 28, 1856. R; pB. Its brightest part is nearest f. edge, and forms a curve round n.
1799	2	Mar. 29, 1856. pL; lE. n. and s.
1804	2	Mar. 1, 1851. d; bM; two others F.
1805	1	Mar. 28, 1856. Long narrow ray; F; bM.
1813	2	Apr. 13, 1852. vgvlbM; filamentary appearance of the branches quite apparent. Though unmistakeably a cl, yet on a very bad night it would be seen as a neb.
1815	1	Apr. 17, 1855. E. sp. nf; nucleus.
1817	1	A B. d. neb.
1818	1	Apr. 19, 1849. r?
1820	2	Apr. 9, 1852. S; bM.
1825	1	Mar. 6, 1851. vlbM; S. ✱ f.
1829	1	Mar. 1, 1851. Nucleus; lE.
1833	1	Apr. 14, 1852. R; vlbM.
1835	1	Mar. 29, 1856. pL; gbM; ✱ in f. edge; between this ✱ and the centre the neb. seemed black.
1840	2	May 10, 1858. lE; vF. and flickering.
1842	2	Apr. 11, 1850. Narrow ray; bad night.
1843	1	Apr. 26, 1851. Within trapezium of 4 or 5 ✱s; lE. n. and s; vlbM.
1844 1845	1	Apr. 13, 1855. The p. one is lE. p. and f, and is the larger of the two. The other is S; R; pF. and bM.
1848	2	Apr. 11, 1850. E. Central part seems unsymmetrically placed with respect to general fig. of neb. Apr. 13, 1850. 3 "novæ" near; one of them mottled, and ✱ in s. border.
1851	3	Apr. 16, 1858. S; B; with B. sharp nucleus, and ✱ involved n. of nucleus; 2 "novæ" near.
1861 1864 1865	1	Apr. 16, 1855. 1861 is a narrow ray; 1864 is S. and R; 1865 is quadruple, and suspected to be one neb. connected by F. neby.
1854	6	Nucleus; dark ring suspected, like 450, but no conclusive evidence.
1857 1863	3	Apr. 7, 1851. Light mottled; another f. about 12′, and a little s; E; bM. Apr. 13, 1852. Spirality suspected.
1870	2	r; lbM.
1872	2	May 10, 1855. pB; R; nucleus; E; mottled.
1873	2	May 15, 1854. pB; S; R; bM.
1874	2	Apr. 25, 1848. E; ✱ at each end.
1879	2	May 14, 1855. vF; nucleus or ✱ in centre.
1880	2	May 16, 1855. 2 neb, with 3 B. ✱s in the neighbourhood. Both F. and E.

Number in Herschel's Catalogue.	Number of times observed.	Description.
1881	3	May 12, 1858. Close d. neb.
1883	1	May 14, 1851. pL; mE. sep, nnf; has nucleus.
1885	3	May 12, 1858. Rather a B. ray; bM. and mottled. Its p. arm is brighter than the f. one. A F. neb. about 2′ p.
1890	3	May 1, 1854. F; pL; no nucleus; mottled.
1891, 1893, 1895	2	Apr. 3, 1854. 3 neb. in line, and another S. neb. near the f. one.
1892	3	May 1, 1854. Has a curved form between 2 ✱s, and in contact with them; there is a 3rd smaller ✱ close to the neb. on np. side.
1894	4	Mar. 1, 1851. B. in centre; E.
1898	2	Apr. 6, 1851. E. p. and f; r. Another vF; 3′ f.
1901	1	Apr. 19, 1849. 6 neb. found.
1903	1	May 3, 1851. E.
1904	1	Mar. 17, 1855. The atmosphere seems to exist.
1905	6	Apr. 28, 1848. Think the distance between the 2 neb. greater than in H's drawing. Apr. 11, 1850. The 2 neb. not in a line, and a F. connexion suspected. Apr. 17, 1855. These 2 neb. are not in a line but parallel; the distance between is considerable, but F. neby. suspected connecting them; they have a very hazy look, and the edges are not well defined. May 14, 1855. Seen as on last time. May 8, 1861. Sketched; axis not parallel, but inclined at an angle of about 16°. Fig. 31, [Plate XXVIII.
1907	3	May 3, 1856. A B. S. ray sp, nf; has nucleus.
1908	2	May 16, 1855. Looks R. pB; mbM; nucleus.
1909	8	Apr. 13, 1850. vB; oval; E. np, sf; ✱ in np. end. "Nova" near; vS.
1910	2	Apr. 13, 1855. mE. n. and s; centre vB; extremities F.
1911, 1912	2	Apr. 25, 1849. 1911 vS; ✱ close to right. 1912 rather F; vS. ✱ involved. "Nova" f. and vF.
1913	1	2 "novæ" f, apparently connected.
1914	2	Nucleus, and E.
1915	3	May 23, 1854. 2 neb. close together, n. and s.
1916		A superb cl.
1917	5	Apr. 13, 1850. Very remarkable ray, 12′ or 15′ long; α, β, γ, and δ are ✱s, of which α is F; a long split precedes the nucleus.
1919	2	Spiral?
		About 15ʰ 12ᵐ R, 33° 55′ N.P.D. A pair of new neb, about 15′ asunder, np. and sf. The sf. one a pB. ray, the other F. and S, but neatly placed at one angle of a triangle of F. ✱s.
1920	2	Apr. 28, 1851. vlbM; E. p. and f.
1923	4	Mar. 17, 1855. S; R; bM.
1924, 1925	1	Apr. 11, 1850. Elegant little d. neb.
1926	2	May 3, 1856. pF; R; bM.
1927	1	Apr. 13, 1852. 3 neb; 2 pB, the 3rd S; E; F; 15′ sp. 1927.
1928	2	Mar. 17, 1855. E. np, sf; bM; not vF.
1929	3	F. dash of light.
1930	1	Apr. 3, 1854. R; B. nucleus.
1931	2	Apr. 13, 1855. Has a ragged edge and mottled look; about 6′ or 7′ nf. there is another.
1934	6	May 6, 1850.

	Pos.	Dist.
AB	288°	7′ 53″
BC	299	6 28
CD	283	8 23

Suspect (A) to be a spiral, to be re-examined on a fine night. (B) a B. condensed oval neb. (C) vF. ray. (D) eeF; S. neb. May 14, 1850. (A) Dark spaces round on either side of nucleus, seen at moments; also a dark line running along the sf. edge, splitting off a part of neb, which has a B. knot to s, also some ill-defined dark space at n. end. Apr. 5, 1851. (A) spiral; a good deal of dark space round the nucleus, branches perhaps like 604.

Number in Herschel's Catalogue.	Number of times observed.	Description.
1936, 1937	2	May 23, 1854. d. neb; both pB; R, and mbM.
1938	3	Mar. 9, 1851. BM; S. neb. p.
1939	1	May 30, 1851. Lenticular ray; bM; 4 ✱s close s.
1942, 1943	4	May 14, 1855. Both S; R; lbM; S. ✱ closely nf. the s. one.
1946	11	May 5, 1850. Strongly suspected to be annular neb. with ✱ near the centre. Apr. 5, 1851. Like 450; dark ring plainer seen on p. part of neb; very S. ✱ n; about ⅓ diameter of neb. off. The f. part of dark ring a little broader than the p. part. May 3, 1851. Distance between nucleus and S. ✱ Dist. May 3, 1851. 0′ 26″ 0 32 0 31 Pos. Dist. May 4, 1851. 7° 0′ 25″ 5 0 28 7 0 28 May 29, 1851. The S. ✱ scarcely seen; dark ring not at all. Apr. 3, 1854. The dark ring round nucleus seen pretty well; also the minute ✱ n. of neb. See fig. 32, Plate XXVIII.
1947	2	May 22, 1854. vL; F; oval.
1950	1	vvF.
1952	1	vF; vlbM.
1953	1	Apr. 7, 1851. eF; 2 or 3 ✱s in edge.
1958	1	May 26, 1849. 2 new neb; one eF, the other S; 1958 R; bM.
1960, 1962, 1963	2	Another near.
1964	1	Apr. 19, 1855. S; F; R; bM. Another neb. 4′ nf.
1968		cl. in Hercules. May 6, 1850. Seems to have a dark streak across the B. part a little above the centre. Apr. 6, 1851. Dark lanes seen which bear some resemblance to those in Neb. Andr. Apr. 27, 1851. Sketch made; dark spaces seen through mist. May 3, 1851. Sketched. May 26, 1851. Sketched. Apr. 17, 1855. The dark lanes are quite discernible in the finder eyepiece; they do not meet in the centre of the cl, but to sff. of it (see fig. 33, Plate XXVIII.).
1969	2	Apr. 19, 1855. The nucleus is nearest the p. edge, and light mottled.
1970	8	May 5, 1850. Intense blue centre fading off to some distance all around; S. ✱s to nf, to which neb. nearly extends. May 12, 1850. I fancied once or twice there were projections p. and f. (N.B. The existence of these not satisfactorily proved.)
1971		cl; in finder eyepiece the branches have a slight spiral appearance.
1972		cl; May 30, 1851. A dark lane above the centre quite across, or rather the upper one-sixth of cluster is much fainter than the rest.
1979		cl; June 3, 1851. The outline not R; on s. side is an outlying portion separated from the chief portion by a dark passage.
1981	2	May 31, 1851. I suspect annular, but twilight leaves me quite uncertain; n. edge is the brightest. June 3, 1851. Annular, n. edge is the brightest.
1983		cl; ✱s S. and very close together.
1989	1	May 29, 1851. Seen in twilight; looked very like a ✱ of 9th mag.
2019		cl.
2023	11	Never well seen on account of twilight. Nothing additional since 1844, except a pB. ✱ sf. middle.
2036		cl.
2037	5	Aug. 28, 1850. Annular or perhaps spiral, and ✱ distinctly seen in dark part. The dark space is undoubtedly irregular in its form. Aug. 24, 1851. Annular; centre very suddenly darker than the rest of the neb; vS. ✱ in np. edge of central part.
2042		cl.
2043	2	Aug. 1, 1851. 4 ✱s in neb, and 2 more on p. edge.
2045	3	On very bad nights.
2046		cl.
2047	7	Aug. 31, 1850. Centre rather dark. Aug. 1, 1851. The dark part is a little np. middle.

Number in Herschel's Catalogue.	Number of times observed.	Description.
2049		cl.
2050	6	Aug. 28, 1850. A very remarkable object, perhaps analogous to H. 450. The ring is not easily seen, but there can be no mistake about it; under the central ✱ there is a darkness. Aug. 22, 1851. sE. np, sf.
2060	13	Places of principal stars laid down, and a new drawing made. First observation Aug. 10, 1850, and last, Aug. 30, 1851. See end of Catalogue, and fig. 43, Plate XXXI.
2064		cl.
2071		cl.
2072	8	Aug. 23, 1851. Fine annular neb. like that in Lyra; R; the dark space is sE. p. and f; ✱ easily seen in np. edge, others suspected. Aug. 19, 1855. There is a conspicuous ✱ on the inner edge of the ring at np. side, and another fainter near this on the outer side. I believe the whole of this corner of the annulus is r, and can see the ✱s sparkling near the two already described.
2075	11	Roughly sketched five times. Aug. 10, 1850. ✱ or B. nucleus nf. the middle. A dark curved line p. this plainly seen, which at moments I fancied went round the sf. part. Sept. 9, 1852. This planetary neb. is a beautiful little spiral. Aug. 12, 1855. I think spiral, of the shape annexed. Aug. 16, 1855. The night bears ½-inch single lens *well*. There is a group of 4 minute ✱s p. the neb. Sept. 6, 1856. The details in my sketch of last year seem correct. I can trace the spirality distinctly. See fig. 34, Plate XXVIII.
2079 2080	1	2079 vF; E; 2080 vF; S; R.
2081		cl.
2084	8	Sept. 6, 1850. New spiral, with three branches, of which two terminate in knots, as in sketch; a fourth branch suspected. Sept. 8, 1850. Examined and drawing made.

		Pos.	Dist.
Sept. 9, 1850.	cμ	87°	4′ 10″
	cl	263	2 47
	cα	158	1 41
	cβ	67	3 34
	cγ	221	2 21
Aug. 21, 1851.	αc	325	1 36
	αβ	213	0 33
Aug. 23, 1851.	αγ	257	2 08
	αδ	261	2 27
	αζ	302	2 03
	αl	281	3 26
	αθ	37	3 46
	αs	47	1 04
	αμ	67	3 46
	αi	162	1 56
	αx	169	2 14

F. branch (D) p. centre seen. Sept. 6, 1855. The two f. branches A and B unite in one before meeting the nucleus. I certainly see a fourth branch D, which seems to join C in the same way before reaching the nucleus. Of the four, those which terminate in knots are the brightest. B is fainter, and D much fainter still. See fig. 36, Plate XXX.

2086	1	Aug. 21, 1857. R; vS; lbM.
2087 2089	2	Aug. 27, 1857. A group of 5 neb; many ✱s among them.
2088	10	Aug. 5, 1851. The nebula resembles the Milky Way, and is full of dark uneven rifts or lanes. The p. edge is the brightest, and the M. is darker than the edges. Sept. 6, 1856. There are portions of its p. edge clearly r.
2090		cl.
2092	3	Aug. 5, 1851. Resembles the neb. 2088, though on a much larger scale; the dark spaces have a rounder or more sack-like appearance, especially at the chief bend, where the neb. is also the brightest. It has several outlying portions of flocculent neby, especially at s. end. Sept. 3, 1855. General shape that of Herschel's figure, but several dark bays in it, and many more ✱s seen in and about it.

Number in Herschel's Catalogue.	Number of times observed.	Description.
2095	1	Aug. 27, 1857. eF; vlbM; no nucleus; E. n. and s.
2097	1	Sept. 5, 1850. R; bM.
2098	11	Since published in the 'Transactions' for 1850.
2099	6	Aug. 19, 1855. The neb. has 3 knots in it; a drawing taken. Sept. 3, 1855. Seen as before, and sketch compared. Sept. 6, 1855. Observed. Sept. 6, 1856. Details as in sketch confirmed. It is vB. See fig. 37, Plate XXX.
2102	3	Sept. 29, 1850. 2′ long; E; nucleus.
2106	1	
2109	1	Aug. 27, 1857. vvF; irreg. R.
2110		cl.
2112	3	Sept. 3, 1856. bM; edges indistinct; a * in nf. edge
2120		cl.
2121	2	vF; lE. nearly n. and s.
2122		No definite cl, but sky thickly studded with stars.
2125		cl.
2127		Loose cl.
2128		cl.
2130		A red * of about 12th mag. in a scattered cl.
2132	6	Sept. 18, 1857. Centre r; mottled; * in edge.
2133		Searched for four times; not found.
2135	2	vF.
2139	11	Form not distinctly made out. See fig. 38, Plate XXX.
2142	1	Not well seen.
2143	5	Never well seen, being very low.
2146	2	E; lbM.
2149	14	Sept. 16, 1854. There can hardly be a doubt that this neb. is a cl.
2150	4	Oct. 23, 1857. lE. sp, nf.
2151	2	Sept. 20, 1857. There is a twist in the neb, but it is so F. that I cannot make out its shape.
2152	1	vF; lbM.
2154		A poor loose cl, with red * of 9th mag.
2156 } 2158 }	1	First has * in nf. edge, and is bM; the other is R; no nucleus.
2157		cl.
2160	4	S; nucleus; forms a quadrilateral with 3 *s; F. outlying neby. extensive.
2162	1	bM; E. p. and f.
2163		cl.
2164	2	Oct. 23, 1857. A vF. ray.
2165	6	About 24′ p. and 10′ n. is another vF; E. np, sf; 80″ long, 10″ broad. 2165 has a sharp nucleus, and is S.
2166	2	vF; pL.
2167	5	Oct. 2, 1856. * in centre; mottled; and * or knot in sp. edge.
2168	2	Oct. 7, 1855. E. n. and s; a vF. * nf. centre; centre B; extremities vF.
2172	17	The sketch conveys accurately the results of these observations. There are 5 knots near. See fig. 39, Plate XXX.

Sept. 12, 1849.

AB	62°	63°
AC	51	
BC	23	21
BD	116	
AD	96	98
BE	163	
AE	119	123
DE	243	
Direction of A....		174

Number in Herschel's Catalogue.	Number of times observed.	Description.
2173 } 2175 }	9	Oct. 7, 1850. Upper neb. is equable in light, and is much the fainter. Sept. 1849. Position of B 91° Position of A 159 Oct. 7, 1850. AB 97° Dist. 5′ 23″ A 157 B 91
2176	2	Sept. 20, 1857. Narrow ray sp, nf; vvF.
2178	1	Planetary?
2179	2	S; R; bM; nucleus.
2180	1	vvF.
2181	1	F; S. 7 knots found.
2183 } 2184 }	4	Nov. 27, 1850. Pos. Dist. αβ 235° 5′ 29″ αδ 73 5 44 αγ 18 5 41
2185	3	Oct. 7, 1855. S; R; pB; mbM.
2186	3	Aug. 30, 1851. E. np, sf; light uneven.
2189	4	A group of 4 neb.
2191	1	No description.
2195	3	A group of 3 involved in vF. neby.
2197 } 2198 }	2	Each has a nucleus. Pos. Dist. Sept. 29, 1850. βα 178° βα 178 1′ 35″ βα 176 1 34 βα 175 1 34 The last two observations probably most correct. Position of axis of β 225° Position of axis of α 160
2199	3	Sept. 16, 1854. * in np. edge. * seen in centre of nucleus?
2200	2	F; bM.
2201	2	Aug. 24, 1851. * p. the nucleus; E. np, sf.
2205	5	Since 1850. Nothing further. Nov. 26, 1850. Pos. Dist. βα 23° 1′ 46″
2206	2	Looks like a * seen in haze.
2208	1	vF; several *s involved.
2209	1	Mottled; * in np. edge.
2210	1	S; lE. p. and f.
2214	3	Nucleus.
2215 } 2216 }	2	Oct. 9, 1850. Pos. 223°. Dist. 2′ 52″.
2218 } 2219 }	3	Nov. 2, 1850. 4 neb. in the field.
2220	1	Nucleus.
2221	1	R; pL.
2222 } 2223 }	1	Both R, and have nuclei.
2224	3	Sept. 16, 1852. Involves a vS. * to nf. Another neb. 6′ p. and 1′ n. of it.
2226	2	Oct. 8, 1855. Outline irreg; pB.
2227	1	R.
2228	5	Like 242. Oct. 11, 1850. Much E. from np. to sf; gbM. to nucleus. Sept. 18, 1852. S. * p. nucleus, and on edge of neby.
2230 } 2231 }	1	Aug. 30, 1851. Another neb. f. 2231 about 12′, which is E. p. and f.
2232	7	Oct. 3, 1856. sf. edge is the brighter, and the more sharply defined.

Number in Herschel's Catalogue.	Number of times observed.	Description.
2236		3 or 4 conspicuous ✱s in it; not in a line between Herschel's two ✱s, the p. one of which is d.
2237	2	S; vF; R.
2241	16	Since the publication in the 'Transactions' for 1850. The outlying portions in the published sketch are parts of spiral branches. Figure 40, Plate XXX. represents it as seen on a very fine night (Sept. 16, 1852), with a freshly polished speculum which defined very sharply. Oct. 2, 1856. All the details in Mr. Stoney's drawing very well seen. Oct. 16, 1857. The spiral arms and the ✱ in centre distinctly seen.
2242		Oct. 23, 1857. R; pB; nucleus; another 6′ s; S; vF.
2245	12	Sketched 4 times. Nov. 5, 1850. I saw two knots and a dark space between them. I think the neb. is connected above the dark space. Nov. 27, 1850. 2 knots seen nearly n. and s, and a dark space between. Aug. 24, 1851. 2 knots and a dark space between, connected above by neby, as in sketch. Sept. 26, 1854. Certainly a spiral; some ✱s at moments visible. Oct. 17, 1854. Spirality distinctly seen. I thought the coil doubled in upon itself more closely than shown in Mr. Stoney's drawing, and that the central knot had a stellar nucleus. The whole neb. looked sparkling, though I could not see its separate ✱s. Nov. 22, 1854. Central nucleus stellar. The outer edge of the coil, just where it joins the external nucleus, seems brighter than the rest. Oct. 15, 1855. Seen to be spiral, as before. See fig. 41, Plate XXX.
2248	2	Oct. 8, 1855. eF; mottled and irreg. outline.
2250		Looked for 4 times; not found.
2257	1	Nov. 4, 1850. Nucleus; a F. neb. f. about 2′.
2258	1	Oct. 17, 1854. R; pB; mbM.
2260	1	Nov. 13, 1854. E; bM; a F. ✱ p.
2261	4	Oct. 24, 1857. Edge ragged; F. nucleus.
2262	3	Oct. 24, 1857. pB; R; mbM.
2264	6	Oct. 7, 1855. A F. suspicion of a dark ring round the B. centre.
2267	1	Nov. 17, 1854. 1E. n. and s.
2268	5	Nov. 22, 1854. pL; R. A ✱ precedes the nucleus (1-inch single lens); sp. this object there is a vS. E. neb.
2271	4	Aug. 24, 1851. A ✱ with a S. neb. in contact.
2273	1	Oct. 12, 1855. A ✱ p. touches the neb. A little np. is another neb. vvF.
2274 2275	15	3 neb. found Nov. 5, 1850. Pos. Dist. αβ 114° 5′ 30″ αγ 128 5 34 β 84 1 44
2278 2279 2280 2281	2	All R; gbM.
2282	3	bM.
2284		cl.
2290	6	Oct. 31, 1855. pL; pB; has a F. but pretty sharp nucleus; edges ragged.
2291	2	pB; pmE.
2297	13	Oct. 12, 1855. pL; B; E; gmbM. A decided dark lane runs through it in the direction of its major axis. The neb. is rather narrower in the middle of its length, and spreads out laterally towards its extremities, fading away very gradually. Nov. 3, 1855. Seen as before; dark streak through centre quite plain. Sept. 9, 1856. Seen very well; dark lane through centre quite plain, especially with highest single lens. Oct. 3, 1856. I think I see right-hand side of centre to be composed of ✱s. It is brighter than the opposite side. See fig. 42, Plate XXX.
2299	2	Oct. 17, 1854. R; pB; bM. to a nucleus.
2300	11	Sept. 10, 1849. 2 S. ✱s near M. Oct. 7, 1855. No nucleus; 2 ✱s seen steadily. The centre of neb. looks darker than the rest. Oct. 8, 1855. There certainly exists a dark bay in the centre of the neb. between the two ✱s.
2301	1	Aug. 24, 1851. A S. lenticular neb.

Measurements of H. 1622 by Mr. BINDON STONEY, October 4, 1851.

H. 1622, April and May, 1851. Object.	Mean of the observation of position.	No. of observation.	Greatest difference between observation and mean.	Mean of the observation of distance.	No. of observation.	Greatest difference between observation and mean.
	° ′ ″		° ′	″		″
9 N	49·37	2	1 0	88·20	2	0·3
9 *n*	22·14	2	0 23	338·7	2	1·2
9 12	323·7	1	.....	81·3	1	
9 15	336·37	1	.....	187·5	1	
9 5	115·52	2	1 15	107·7	2	0·0
9 4	97·7	1	.....	294·9	1	
9 2	45·37	1	.....	377·7	1	
9 13	296·7	1	.....	177·3	1	
9 14	326·37	1	.....	234·9	1	
9 8	217·37	1	.....	116·1	1	
9 7	196·37	1	.....	93·3	1	
9 6	175·7	1	.....	186·9	1	
9 10	201·37	1	.....	48·9	1	
9 11	333·37	1	.....	57·3	1	
9 16	249·37	1	.....	201·9	1	
5 *α*	.....	...	.....	254·1	1	
5 *β*	.....	...	.....	94·5	1	
15 *γ*	.....	...	.....	300·5	1	

Object.	H. 1622. Position as measured by			H. 1622. Distance as measured by		
	O. Struve, 1851.	J. Stoney, 1850.	B. Stoney, 1851*.	O. Struve, 1851.	J. Stoney, 1850.	B. Stoney, 1851*.
	° ′	° ′	° ′	″	″	″
N 1	51 47	52 4	.....	115·1	126·6	.....
N 2	54 48	54 0	44 27	518·0	300·0	289·27
N 3	.....	104 20	.....	.....	165·6	.....
N 4	108 54	111 57	112 42	243·63	243·6	243·95
N 5	161 47	165 35	164 11	104·4	103·2	108·67
N 6	190 24	191 42	191 54	250·0	234·0	249·12
N 7	210 51	211 2	212 35	174·2	156·6	174·47
N 8	221 25	220 49	222 43	202·6	176·79	203·57
N 9	229 26	231 32	229 37	88·57	83·4	88·2
N 10	.....	223 30	219 37	.....	106·94	133·77
N 11	277 27	279 21	266 8	121·9	109·8	92·88
N 12	.....	274 23	273 40	.....	117·67	116·15
N 13	.....	281 37	275 8	.....	239·0	227·67
N 14	.....	.....	305 11	.....	.....	240·39
N 15	309 2	310 34	308 40	189·9	175·8	183·15
N 16	.....	.....	243 30	.....	.....	286·62
N *n*	14 51	16 54	13 23	265·65	262·2	262·97

* In the reduction of these from the former Table, STRUVE's determination of N 9 has been used.

N.B. STRUVE's measurement of N 11 ought perhaps to be attributed to N 12.

List of Stars in the Dumb-bell Nebula, H. 2060, measured in autumns of 1850 and 1851. Origin taken at α (a of M. STRUVE); brightest star in sp. quarter. By Mr. BINDON STONEY.

Name in observing-book.	M. Struve's name.	X.	Y.	No. of observation.	X in M. Struve's list.	Y in M. Struve's list.
β	d	+154″·4	+ 17″·5	2	+152″·5	+ 12″·8
ζ	e	+325·3	+ 63·1	2	+321·1	+ 62·8
γ	e	+172·8	+ 98·6	3	+172·5	+ 98·8
δ	f	+172·3	+144·5	2	+174·6	+142·2
ϵ	g	+173·9	+193·2	2	+174·3	+191·5
θ	h	+246·2	+100·5	2	+251·1	+101·7
κ	k	+187·4	−121·3	2	+186·8	−121·0
μ	w	+323·5	−115·0	2	+317·7	−102·0
ν	b'	+363·6	+183·4	2	+368·3	+186·3
ξ	b	+ 23·0	+199·7	2	+ 21·6	+199·7
σ	o	+ 46·8	+149·5	3	+ 49·6	+144·0
π	p	+ 48·1	+ 61·3	2	+ 60·6	+ 57·9
τ	a'	− 10·9	− 59·8	2	− 12·3	− 57·2
η	i	+235·8	+ 70·8	1	+238·2	+ 71·5
ι	...	+223·0	+ 51·4	1		
A	...	+ 54·4	− 22·8	2		
λ	...	+304·6	+ 21·3	1		
ψ	...	+ 73·6	−136·8	1		
φ	...	+ 63·6	−122·6	1		
ω	...	+142·6	+ 60·4	1		
ν	r	+398·2	− 91·6	1	+396·9	− 74·0

A is the extremity of the bright mass of nebulosity in the sp. quarter.

[*Observations of Stars in the Dumb-bell Nebula, Messier* 27, H. 2060. By M. O. STRUVE.

The following list of observations contains all stars which can be seen, by the Pulkova refractor, in the Dumbell Nebula. Sometimes I had a faint suspicion of some stars more visible on the ground of the nebula, but they were not seen distinctly enough to allow an exact micrometrical determination of their places. It will be interesting to see how many stars more Lord ROSSE's great reflector will show in this nebula; from this comparison some approximate judgment might be formed about the respective space-penetrating powers of the two instruments. My observations extend as far as I was able to trace the nebulosity. I think there is only one star in the lists situated quite out of the nebula. This star was observed merely for the sake of control. Probably Lord ROSSE's large reflector will extend considerably further the boundaries of the nebulosity; but to compare the relative powers of the two instruments, it will be necessary to confine the enumeration of the stars to the same boundaries which are approximately indicated by the stars in the following list. As fundamental point for the triangulation, I selected the star a of the 10th magnitude, situated near the S.W. corner of the nebula. It cannot be mistaken, for it is far the brightest object in the whole nebula. All observations have been made with illuminated wires in the dark field, and with a power of 207.

Observations.

Date.	Object.	Magnitude.	Ang. Pos.	No. of measures.	Distance.	No. of measures.
1851. Sept. 1	ab	b =(11.12)	6·4	3	200·4	3
	bb'	b' =(11)	92·7	3	282·3	3 not accurate.
	ae	e =(11)	60·1	3		
Sept.	ae	.....	60·5	3	199·4	6
	ce	c =(11.12)	283·7	3	152·9	6
	ad	d =(11.12)	85·2	3		
	ed	.....	193·1	3		
	ef	f =(12)	2·8	4		
	bf	.....	110·6	4		
	bg	g =(12.13)	93·4	4		
	eg	.....	1·1	4		
	eh	h =(11.12)	87·9	5		
	ch	.....	299·0	4		
Sept. 3	ei	i =(12.13)	112·6	4		
	hi	.....	203·1	4		
	ak	k =(11.12)	123·0	4		
	ek	.....	176·4	6		
Sept. 12	ab	.....	6·1	3	201·3	6
	ae	.....	60·3	3	198·5	6
	ec	.....	103·0	3	151·7	6
	al	l =(10.11)	116·05	6	ΔR=358·7	10
Sept. 17	em	m =(12)	168·7	4		
	lm	.....	268·9	4		
	ln	n =(12.13)	258·2	4		
	mn	.....	180·3	6		
	ao	o =(13)	19·0	4		
	bo	.....	156·3	4		
	eo	.....	290·2	4		
	al	.....	116·00	4	ΔR=359·9	10
Sept. 22	ap	p =(13)	46·3	4		
	bp	.....	164·6	6		
	aq	q =(11.12)	257·9	4	99·8	6
	lr	r =(12.13)	36·2	4		
	er	.....	127·6	4		
	sr	s =(8)	258·3	4		
Sept. 26	lt	t =(12)	24·9	4		
	et	.....	142·4	4		
	st	.....	247·0	5		
	lu	u =(11.12)	118·2	6	77·9	9
	uv	v =(12)	343·3	6		
	lv	.....	89·5	5		
	lw	w =(13)	346·3	4		
	aw	.....	107·8	4		
	sw	x =(13)	256·3	4		
	lx	.....	82·9	4		
	ux	.....	37·4	6		
	ly	y =(12)	190·5	4		
	uy	.....	256·9	4		
	lz	z =(12.13)	234·6	5		
	kz	.....	130·3	4		
Oct. 3	aa'	.....	192·1	4		
	qa'	.....	113·0	4		
	as	.....	$\Delta\delta$=−42″·0	8	ΔR=600·8	16
	ab	.....	6·0	4	201·2	8
Oct. 20	ae	.....	59·9	3	198·5	7
	eb'	.....	57·2	4	161·5	6
	ec	.....	104·1	3	154·1	7
Dec. 17	cd	.....	253·8	5		
	dk	.....	166·1	4		
	el	.....	148·72	4		
	lu	.....	119·5	4	79·9	6

From the preceding observations the following coordinates, X, Y, with regard to a have been deduced. X is the coordinate in the direction of AR or $\Delta AR \cos\delta$; Y is the coordinate in declination or $\Delta\delta$. In this deduction, where the angles of position of any star were taken from three different points previously fixed, I have combined only those two directions which promised the most exact evaluation of the coordinates. Thus, for instance, the star r is related to l, e and s. In the three triangles formed in this way, the angle at r is found respectively $lre=91°\ 24'$, $lrs=137°\ 54'$, $sre=130°\ 42'$. Hence it is evident that the nearly right-angular intersection of the two sides lr and re in the triangle lre, must give the best combination for the deduction of the position of r. The relation of l and e to another and to a being previously established, the coordinates of r to a were deduced from the resolution of the triangle lre. The difference of these coordinates from the corresponding previously fixed coordinates of s with regard to a, gave the angle of position of r from s by calculation $=258°\ 37'$. This calculated angle, compared with the directly observed position $=258°\ 18'$, shows a difference of only $19'$, and bears, therefore, a very satisfying testimony to the exactness of the deduced position of r. The same proceeding having been followed in all other cases, where control observations had been made, I have got the conviction that the probable error in any of the following coordinates will be considerably inferior to one second of space. It will be comparatively smaller for the brighter stars, but somewhat greater for the very faint objects.

Calculated Coordinates of the Observed Stars with regard to a.

	X.	Y.
	″	″
q	− 97·6	− 20·9
a'	− 12·3	− 57·2
b	+ 21·6	+199·7
o	+ 49·6	+144·0
p	+ 60·6	+ 57·9
d	+152·5	+ 12·8
e	+172·5	+ 98·8
g	+174·3	+191·5
f	+174·6	+142·2
h	+186·8	−121·0
n	+224·9	−184·7
m	+225·0	−164·3
i	+238·2	+ 71·5
k	+251·1	+101·7
x	+279·6	−199·7
b'	+308·3	+186·3
w	+317·7	−102·0
c	+321·1	+ 62·8
y	+321·8	−219·5
l	+332·4	−162·2
t	+347·8	−128·9
v	+390·7	−161·7
r	+396·9	− 74·0
u	+402·4	−200·8
z	+442·0	−148·5
s	+555·8	− 42·0

Pulkova, Feb. 4, 1852.] (Signed) OTTO STRUVE.

[*Observations of Stars in the Spiral Nebula.* H. 1622.

The spiral form of this nebula is very distinctly seen in the Pulkova refractor. Unfortunately in the month of March, the best season for the observation of this object, the sky was constantly cloudy; so that I could only get three nights' observations in the months of April and May, when the twilight did not cease for the whole night. It must be attributed to this unfavourable circumstance that the following list of determinations is not so complete as it probably would have been without the twilight. The observations have been made alternately with powers of 138 and 207.

Observations.

Date.	Object.	Magnitude.	Ang. Pos.	No. of measures.	Distance.	No. of measures.
			° ′		″	
1851, April 7.	N *n*	.....	14 55	5	267·1	4
	N *a*	*a* =(11)	229 24	3	88·0	3
	N *b*	*b* =(11.12)	109 12	3	242·6	3
	a b	.....	93 42	3	298·6	3
April 28.	*a b*	.....	94 23	3	300·8	4
	N *a*	.....	228 36	4		
	N *b*	.....	108 54	4		
	n a	.....	203 42	3		
	n b	.....	153 30	3		
	a d	*d* =(12.13)	323 51	3		
	N *d*	.....	277 27	3		
	a e	*e* =(13)	112 13	3		
	N *e*	.....	161 56	3		
	N *f*	*f* =(12.13)	309 18	3		
	n f	.....	237 31	3		
	a f	.....	335 23	3		
	a g	*g* =(12.13)	215 17	3	115·5	4
	a h	*h* =(12.13)	193 29	3		
	g h	.....	87 5	3		
May 3.	N *k*	*k* =(13.14)	51 47	3		
	n k	.....	173 29	4		
	b k	.....	317 23	3		
	b l	*l* =(11.12)	27 20	4		
	n l	.....	83 17	4	355·2	4
	a e	.....	112 56	4		
	N *e*	.....	161 39	3		
	a m	*m* =(12.13)	172 43	5		
	N *m*	.....	190 44	4		
	b m	.....	238 50	4		
	N *a*	.....	229 12	4	87·0	3
	N *n*	.....	14 47	4	264·2	3

The results of these measures were deduced in this way, that I first fixed the relations between the four principal objects, namely, the centres of the two nebulæ N and *n*, and the two brightest stars *a*, *b*. In the triangle N *a b*, all distances and directions have been measured It is therefore over-determined, and the definite relations had to be deduced by the calculus of compensation. This calculus gives—

	Ang. Pos.	Distance.
	° ′	″
N *a*	229 26	88·57
N *b*	108 54	243·63
a b	94 6	298·55

To these relations must be added the mean value of the two observed relations N *n* (April 7, May 3) which gives N *n* ang. pos. $=14^\circ\ 51'$, distance $=265''{\cdot}65$.

These relations between the four principal objects form the base from which the places of the other stars are deduced by resolutions of the triangles formed by the observed directions. The following Table contains the results of my calculations. It gives the places of all observed objects with regard to N, the apparent centre of the greater nebula, and that of the star *g* to *h*.

	Ang. Pos.	Distance.
N *n*	14° 51′	265″·65
N *a*	229 26	88·57
N *b*	108 54	243·63
N *d*	277 27	121·9
N *e*	161 47	104·4
N *f*	309 2	189·9
N *h*	210 51	174·2
N *g*	221 25	202·6
N *k*	51 47	115·1
N *l*	54 48	518·0
N *m*	190 24	250·0
h g	267 5	44·7

Our observations contain several controls, by which it is proved that the deduced places of the stars, with regard to N, might be judged all exact within 2″. This exactness must be regarded as very satisfactory, if we consider the extreme faintness of the observed objects. I estimate a star to be of the 14th magnitude if it is more suspected than distinctly seen in a dark night. Hence it follows that the greater part of the stars in our list are close to the extremity of measureableness in the Pulkova refractor. Another cause which troubles the agreement of results is the indistinctness of the centre of the greater nebula N. The centre of the small nebula *n* is much more distinct: all observations of the dimensions of the nebula, or of knots in it, have been omitted by me, as they can be observed with much more accuracy by Lord Rosse's powerful telescope.

The Earl of Rosse communicated to me the following relations of sixteen objects in the nebula, as observed through his telescope. I add to that list the differences of our measures (S—R) for the objects which appear to be identical.

Designation by Lord Rosse.	Designation by O. Struve.	Ang. Pos.	S−R.	Distance.	S−R.
N *n*	N *n*	16° 34′	−1° 43′	4′ 22″·2	+ 5″·4
N 1	N *k*	52 4	−0 17	2 6·6	−11·5
N 2	.....	54 0	.....	5 0·0	
N 3	.....	104 20	.....	2 45·6	
N 4	N *b*	111 57	−3 3	4 3·6	0·0
N 5	N *e*	165 35	−3 48	1 43·2	+ 1·2
N 6	N *m*	191 42	−1 18	3 54·0	+16·0
N 7	N *h*	211 2	−0 11	2 36·6	+17·6
7, 8	*h g*	270 42	−3 37	0 34·8	+ 9·9
N 9	N *a*	231 32	−2 6	1 23·4	+ 5·2
9, 10	.....	197 57	.....	0 27·0	
N 11	N *d*	279 21	−1 54	1 49·8	+12·1
11, 12	.....	225 27	.....	0 12·6	
N 13	.....	281 37	.....	3 51·0	
14, 15	.....	297 15			
N 15	N *f*	310 34	−1 32	2 55·8	+14·1

From this comparison it is evident that all angles measured by Lord Rosse are too great. The mean value of the correction, 1° 57′, corresponds to a linear distance of −4″·7. The distances appear to be generally too small. The mean value of the differences is +6″·8. Perhaps Lord Rosse's star 2 is identical with my *l*; but in that case, Lord Rosse's distance N 2 must be an error of writing. At the distance of 5′ from N I could not see the least trace of a star in the indicated direction. In my copy of Lord Rosse's diagram the star 2 is placed at a distance of about 8′, corresponding with my observations; 10 appears to me only as a knot of the nebula, and has therefore not been measured by me. About the stars 3, 12, 13, and 14 there is no notice given in my journal. Perhaps they might be seen and measured with our refractor. The next spring I intend to repeat and to complete the series of observations, and to decide on the visibility of the not yet noticed stars.

(Signed) Otto Struve.

(Dated) *Pulkova, June* 2, 1851.]

In forming some estimate of the degree of reliance to be placed on the micrometrical measurements in this paper, we have taken advantage of the information so obligingly communicated by M. O. Struve.

As to the measures of distance, they accord with Struve's as closely perhaps as could be expected. We measure with bars instead of lines, and without illumination, that we may the better see the faint details of the outlying portions of the nebulæ; besides, we do not employ clockwork to move the telescope. Our measures of *stars* cannot therefore in accuracy compete with Struve's, but they are quite sufficient for giving precision to the drawings. As to the angles of position, the same remarks apply, with this addition, that we refer our measurements to the horizon, and reduce them to the equator. Our zero is therefore obtained from the spirit-level, which saves time, to us a great object. We proceed in this way: the level is made horizontal and read off: each

measure + or − this quantity will be the distance from the horizon, provided the telescope is on the meridian. When the measures are not taken on the meridian, but a little before or after it, there will be an error in all positions except at the equator. There will be an error owing to two causes, one the error of the zero, the level having been read off the meridian; the other the error in the reading of the position-circle, owing to the action of the universal joint which carries the telescope. The transverse axis of this joint restrains the movement of the telescope round the line of collimation as it approaches or recedes from the meridian; and consequently the plane of the position-circle, except at the equator, does not pass through the pole. The sum of the errors in each case could easily have been computed and allowed for in the reductions, had the distance from the meridian been taken simultaneously with the measurements, but this would have taken much time.

The measurements in the Dumb-bell accord pretty closely with STRUVE'S, and may, I think, be taken as a fair average of the work.

As to H. 1622, the comparison of STRUVE'S measures with those of the two STONEYS will give more than a probable amount of error at 42° N.P.D, because the stars are numerous, and some measures therefore were taken at a considerable distance from the meridian.

I have not seen the Dumb-bell since STRUVE'S letter, having been from home when it was within reach; and no attempt has been made to ascertain the largest number of stars visible in it. No stars have been inserted in the sketch which have not been measured: very many more were distinctly seen. The number of stars visible in this nebula depends even more upon magnifying power and distinctness than aperture; high powers obliterate the faint nebulous details.

The only additional information as to the limits of the nebula which has been obtained since Mr. B. STONEY'S drawing was made is contained in the following entry:—August 29th, 1854, observed by Dr. ROBINSON and Mr. J. STONEY. Mr. STONEY says, "Both Dr. ROBINSON and I agreed that the band of faint nebulosity extended further down than in my brother's drawing. My brother and I had formed the same opinion on a previous occasion."

In the observations a silver speculum is sometimes mentioned: we have employed silver occasionally for the second reflexion in this way. First, a *thin* deposit on glass by LIEBIG'S process. This, even when fresh, reflects but little more light than speculum-metal. Second, a *thick* deposit on glass, by the grape-sugar or tartaric-acid process, transferred to brass by a thin film of shell-lac: this reflects much more light; but the manipulation is rather difficult, and the surface is not very durable. Third, a surface of standard silver, polished by mechanical means. Fourth, parallel glass, silvered by the grape-sugar process; this of course is durable, but very inferior to the uncovered silver in light and in definition. These substitutes for speculum-metal have only been occasionally used, and for special purposes.

List of Nebulæ not found.

Number in Herschel's Catalogue.	Number of times looked for.	Observations.	Number in Herschel's Catalogue.	Number of times looked for.	Observations.
57	1		672	1	
162	8		706	6	
184	1		745	1	
206	1		828	1	
281	1		1307	1	
284	2	One night passing clouds.	1485	2	Once sky hazy.
314	1	Clouds passing.	1535	1	Clouds passing.
333	1		1832	1	
343	7		1948	1	
356	4	Twice, a slight milkiness suspected.	1974	1	
			2062	1	
401	9		2073	2	
468	5	No nebulosity seen.	2113	2	
546	1		2133	4	
577			2137	1	
578	1		2148	3	Once clouds passing.
590	1		2250	4	
669	1		2302	2	

This is not to be considered as a list of missing nebulæ, but merely of objects which were not found in the ordinary course of observing, and to which therefore it is desirable that attention should be directed. They have not been looked for since this list was made out.

The engravings of the Nebulæ are extremely faithful: there is, however, a slight inaccuracy which it is necessary to notice, and for which we are to blame, not the engraver. Many of the principal stars are too large. The error arose in this way. The stars were inserted in common, not Indian ink, and, the drawings during their transmission by post becoming slightly damp, the ink made its way into the paper, the dots in some cases becoming small blots. In a few instances it was necessary to set this right to prevent misconception, and some alteration was in consequence made in the Plates; but as to the remainder, we thought it sufficient to state the fact generally, that many of the principal stars were somewhat too large. This remark applies especially to figures 1 and 3, Plate XXV., and to figures 24, 28, 29, 30, 34, Plate XXVIII.

XXIX. *On the Elimination of Urea and Urinary Water, in relation to the Period of the Day, Season, Exertion, Food, Prison Discipline, Weight of Body, and other influences acting in the Cycle of the Year.* By EDWARD SMITH, *M.D., LLB., F.R.S., Assistant Physician to the Hospital for Consumption and Diseases of the Chest, Brompton, Corresponding Fellow of the Academy of Sciences and Arts of Montpellier, and of the Natural History Society of Montreal, &c.*

Received April 18,—Read May 30, 1861.

I PURPOSE in the following communication to state the results of two series of experiments in reference to urea and urinary water in the healthy human system, with a view chiefly to show their relation to exertion, food, nutrition, period of the day, and season of the year, and to enable me to contrast these relations with those of carbonic acid, which I had the honour to lay before the Royal Society in 1859, and which have been published in the Philosophical Transactions.

The first series of inquiries was made upon myself, and was continued from January 18, 1860, to March 18, 1861, and numbered 1633 observations upon the urinary water, and 1073 analyses for urea on 336 days. The observations were fewer in the months of September and October than in the other months; but from January to the end of July 1860, throughout nearly the whole of October, and from the early part of November until the end of the inquiry, the observations were not intermitted.

[The inquiries were continued daily until March 18, 1862; so that the whole series comprehends a period of two years and two months, with 635 days of actual daily observation, and nearly 1400 analyses for urea*.]

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The ordinary food was taken during this period; and each part of it was weighed and recorded, except in some of the summer months. It was of a simple kind, consisting of coffee and bacon for breakfast, at about 9 A.M.; one kind of meat, with vegetables and pudding, at 2 to 3 P.M.; tea with bread and butter at 6 P.M., and coffee with bread and butter at 9 or 10 P.M. On Tuesdays and Fridays coffee and bread and butter were taken at 1½ P.M., and dinner, with tea, at 5 to 6 P.M. No kind of alcoholic liquor was ordinarily taken; and whenever any unusual kind or quantity of food was eaten, or the hour of dining was changed, it was duly recorded. The following is an average example of the dietary used daily:—cooked meat, 3 oz.; cooked bacon, 2 oz.; butter, 1 oz.; bread, 14 oz.; pudding, 9 oz.; potatoes, 7 oz.; sugar, 2 oz.; milk, 6 oz.; coffee, 35 oz.; tea, 15 oz.; water, 10 oz.

The general habits were not interfered with. There was a fair amount of activity, both of body and mind, except on Sunday, when the rest was almost perfect. The

* The portions of the following paper which are enclosed in brackets have been added since the paper was read.

amount of exertion was duly estimated. The month of August and part of September were spent at the sea-side, when there was a little increase of food and exertion.

The aim in this part of the inquiry was to determine the amount of urea and urinary water eliminated throughout the year, under the ordinary conditions of a uniform and active life, and therefore under those to which the mass of mankind are subjected.

The urine was passed at three prescribed periods, viz. on going to bed, on rising, and again immediately before breakfast, and also at other periods of the day as necessity required. The greatest care was taken that not the least portion was lost at any time; and during the alvine evacuation a separate vessel was used. It was commonly passed into tall graduated glasses, and the quantity read off to ·1 fl. oz., either immediately or on recording the whole quantity emitted to a certain period; but when travelling ·1 part of each quantity was reserved, and a mixture of the whole was submitted to examination. The record was commonly completed on the following morning, including the quantity, appearance, specific gravity, and temperature of the urine and the temperature of the air; and the analyses for urea, chloride of sodium, and free acid were then made, but sometimes they were deferred to the second, and in winter occasionally to the third day. The quantity in each twenty-four hours was made up to 8 A.M., and an addition or subtraction was made when the night urine was passed before or after that hour, according to the rate at which it was then secreted.

The weight of the fæces was recorded, with the hours of evacuation; and during a part of the inquiry the weight of the naked body was ascertained, after the emission of urine, night and morning. Certain experiments were made during fasting, as to the influence of water and other ingesta over the excretion of urine and urinary water in the morning, and the dates and results of such experiments were recorded. Hence this series of inquiries furnishes the amount and kind of ingesta, the quantity of urea and urinary water, and certain other characteristics of the urine, the weight of the fæces and of the body, and the relations of these to each other, and to the cycle of the day and year, with the effect of certain special articles of food upon the urine. I am 5 feet 11¼ inches high, about 190 lbs. weight, in good health, and fleshy, and was forty-one years of age in the middle of the inquiry.

The second series of experiments were made upon four prisoners in Coldbath Fields Prison (by the courtesy of the Board of Visiting Justices, and with the valuable aid of Mr. LAMBERT, one of the officers of the prison), who worked the treadwheel on three days per week, and did light and routine work on the alternate days. They had been some months in prison, and were in fair health. Their ages varied from twenty-two to forty-two years, their weight from 115 to 133 lbs., and their height from 5 feet 2¼ inches to 5 feet 7 inches. They had long been placed upon the full prison dietary, consisting of cocoa and bread for breakfast, meat, potatoes, and bread for dinner, and gruel and bread for supper, and three of them gained weight a little during the inquiry.

The urine of each prisoner was collected separately up to certain prescribed hours, and the weight of the fæces, with the hour of evacuation, was recorded.

The usual food was allowed throughout the inquiry, with the following variations:—

1. The salt was reduced from the indefinite quantity of about 1 oz. daily to ¾ oz. daily (besides that contained in the bread and gruel), on the sixth day.

2. From the tenth to the thirteenth day no salt was allowed, except that contained in the bread and gruel.

3. From the thirteenth to the sixteenth day extra fat was given.

4. From the sixteenth to the nineteenth day tea was given.

5. From the nineteenth to the twenty-second day coffee was given.

6. From the twenty-second to the twenty-fifth day alcohol was given.

During the whole period the men were under the immediate observation of Mr. LAMBERT, who most carefully noted the prescribed period for the collection of the urine, the weight of the fæces, the weight of the food, the due admixture of the ingredients in the gruel, the weight of the fat and the lean meat, and the weight of the men, and ascertained that all the food was eaten; and as the men entered willingly into the inquiry (for a trifling pecuniary advantage), I feel assured that the most trustworthy results have been obtained.

Mr. MANNING most kindly undertook the laborious analyses of the food, urine, and fæces, so far as relates to the nitrogen and the mineral matter; and to him I am greatly indebted. An admixture was made of the whole urine passed in the twenty-four hours, and also of the fæces, by all the men; and from these two quantities samples were taken for analysis. Five ounces of the fæces were evaporated to dryness in a water-bath, and the nitrogen was determined in one-third of the dry matter, and the ash from 300 grains of the dry matter.

This series of experiments has therefore given the amount of urea, chloride of sodium, and urinary water, the weight and the chemical composition of the fæces, the nature and amount of the ingesta, and the total nitrogen and ash in the ingesta and egesta, and the relation of each to the other and to well-defined labour and rest. The whole number of analyses of urea was 248, and a similar number represents the inquiries into the chloride of sodium and urinary water.

The method of analysis of urea and chloride of sodium was that recommended by LIEBIG. The chloride of sodium was not removed, but its amount was determined and duly allowed for in estimating the urea. The measures employed were decimal parts of the fluid ounce, and the quantities recorded are those of fluid ounces. The solution of protonitrate of mercury was made and graduated by myself in one large quantity of 10 gallons, and was continuously used in all the experiments made since April 13, 1860; but I am indebted to Mr. DUGALD CAMPBELL for the solution used at an earlier period, and for many other acts of kindness. All the analyses of urea and chloride of sodium were made by myself. The tubes and pipettes employed were rigorously graduated by the balance; and as every effort was made to proceed in a most uniform and accurate manner, and the practice has been very extended, I trust that the errors attending so large an inquiry have been reduced to a minimum.

[In June 1861 another set of this series of inquiries was prosecuted, similar to those already detailed at Coldbath Fields Prison. They were made conjointly with Mr. W. R. MILNER, Surgeon to the Wakefield Gaol, and upon the prisoners entrusted to his care.

Mr. Milner took the general supervision of the prisoners in reference to their dietary, excretions, and weight, whilst Mr. Manning kindly determined the nitrogen and dry matter in the fæces and urine, and I made the analyses for urea and chloride of sodium. The following is an outline of the inquiry:—

Four men, of regular habits and in a good state of health, were selected. Two were weavers of wide-width cocoa-matting, which is a very laborious occupation, and two were tailors. Their ages were 19, 22, 24, and 28 years. Their height was 64¾, 66, 66¾, and 67 inches; and their weight 118 lbs. 11 oz., 125 lbs. 12½ oz., 146 lbs. 11¾ oz., and 146 lbs. 15¾ oz. The girth around the nipples was 32¾, 34¼, 35¾, and 35½ inches, giving an average of nearly 34½ inches. The total averages of age, height, weight, and girth were 23¼ years, 66·1 inches, 134 lbs. 8¾ oz., and 34½ inches.

They had been fed upon the highest class of prison dietary; but as that consisted of too much variety of food for our purpose, it was deemed advisable to give them a uniform daily dietary during one week before the experiments began, and it was thenceforward continued without intermission until the inquiry terminated.

The food supplied daily was in part fixed and in other part variable in quantity. The fixed quantities were those of meat, oatmeal, and potatoes, and the variable ones those of bread, salt, and water. Milk was given in a fixed quantity, but the amount supplied was not uniform in the two classes of prisoners. The meat consisted of 5 oz. of lean and 1 oz. of fat cooked beef without bone. The supply of oatmeal was 2 oz., and of cooked potatoes 1 lb. daily. 20 oz. of skimmed milk were given to the tailors, and 25 oz. to the weavers. The daily quantity of bread eaten was on the average 24·3 oz. by the tailors, and 30·4 oz. by the weavers, or a general total of 27·35 oz. The quantity of chloride of sodium eaten (besides that contained in the bread) was 136·5 grs. daily by the tailors, and 63·5 grs. by the weavers, giving an average of 100 grs. daily. There was considerable variation in the quantity from day to day; for whilst one of the tailors ate an average amount of 199·3 grs., the other tailor ate only 73·8 grs. The quantity of water drunk, besides that contained in one pint of gruel, was only 23·8 fluid ounces on the average; and this with the milk gave a total daily supply of fluid of 66·3 oz. The weavers drank much more than the tailors; and the total daily quantities in the two classes were 80·5 oz. and 52·1 oz. The solid food was 51·8 oz., and the fluid 66·3 oz., or a total ingestion of 118 oz. daily.

The prisoners rose at 6 A.M., and, having passed urine and fæces, were immediately weighed. The scales employed were good ones; and the weight was taken to a quarter of an ounce. The prisoners were weighed naked. The weight of fæces and urine was ascertained daily up to 6½ A.M.; and the degree of consistence of the fæces was recorded under four heads, viz. scybalous, well formed, formed but soon subsiding, soft and liquid. A fair sample of the bread, oatmeal, potatoes, meat, and milk was sent up to Mr. Manning from time to time as changes in the supply occurred. A portion of the mixed quantities of the fæces and urine of each set of prisoners was most carefully taken and sent for analysis daily; but delay in transmission sometimes occurred, so that the analyses were usually made on the third or fourth day after the evacuation. The greatest care was taken to avoid loss by evaporation or otherwise, and to prevent decomposition. The

observations included thirteen days, besides the week of preliminary dietary and preparatory arrangements.]

I propose to state the results of these several inquiries in the following order:—

1. The absolute amount of urea evolved daily (p. 765).

2. The absolute amount of urinary water (p. 769). [Its relation to weight of body, p. 771.]

3. The relations of the daily amount of urea and urinary water (p. 772). [Contrast of daily quantities during two successive years (p. 802.)]

4. The relations of urea and urinary water to the period of the day with ordinary food, during fasting, and with special articles of food (p. 774).

5. The relations of urea, urinary water, and weight of body to the cycle of the week (p. 793).

6. The relations of urea and urinary water to the cycle of the year, with the influence of temperature and atmospheric pressure (p. 796).

7. The influence of the treadwheel and other prison labour over the excretion of urea, chloride of sodium, urinary water, and fæces, both with an ordinary diet and with the addition of certain special kinds of food (p. 804).

8. Certain relations of urea, chloride of sodium, and urinary water to food (p. 818).

9. The relation of the excretion of urea to headache and stomach-derangement (p. 823.)

10. The relation of urea to carbonic acid (p. 824).

11. The relation of urea to nutrition (p. 825).

The inquiry into the relations of urea is one of extreme complexity; and I premise a scheme of the modifying circumstances which act together and which require consideration.

Relations of Urea.

1. PRODUCTION.
2. ELIMINATION.

1. Production: (chiefly) FOOD.	The amount taken into the blood.
	The nitrogen which it contains.
	The general appetite.
	The temporary variations in the appetite.
	The period of the year.
2. Elimination: (chiefly) FLUIDS.	The emission of urine.
	The general appetite.
	The variations in the appetite.
	The emission by other outlets than the kidneys.
	The atmospheric pressure.
	The atmospheric temperature.
	The state of the health.
	The period of the day.

3. The variation in the vital actions.

4. The habits of the subject of experiment.

TABLE I.—Showing the daily amount of the Ingesta and Egesta and weight of body of the Author, and also the Temperature and Atmospheric Pressure, Plates XXXIII. and XXXVI.

Date.	Ingesta.		Egesta.					Daily number of observations and analyses.		Greenwich.			Weight −15 stones (182 lbs.).		Weather, state of health, and other observations.
			Fæces.	Urea.		Urine.		Urea.	Urine.	Temperature.		Barom. daily averages.			
	Solids.	Fluids.		Total daily to 8 A.M.	Hourly.	Total daily to 8 A.M.	Hourly.			Daily average.	Daily range.		Night.	Morning following.	
	oz.	fluid oz.	oz.	grs.	grs.	fluid oz.	fluid oz.			° F.	° F.	inches.	lbs. oz.	lbs. oz.	
1860. Jan. 23.	...	56	...	422·8	17·4	59·44	2·47	7	7	40	11·5	29·190	...	...	Experiments on water.
24.	...	51·5	...	421	17·3	63·87	2·66	7	7	39·7	12	28·583	...	...	Experiments on water and bread.
25.	...	47	...	392·2	16·3	25·87	1·07	8	8	37·9	8·2	29·189	...	...	Experiments on water and coffee.
26.	...	60·5	4·75	446·5	18·6	31·87	1·32	9	9	35·7	18·2	29·562	...	...	Experiments on water and tea.
27.	...	50·5	3	330·5	14·1	45·2	1·88	6	8	42·7	12	29·239	...	...	Experiments on water.
28.	...	69·5	2·75	445	18·5	34·9	1·45	7	9	32·6	10·3	29·901	...	...	Experiments on water; black dose.
Sunday. 29.	...	54	4·25	461	19·2	40·6	1·7	2	6	40·1	9·5	29·463			
30.	...	60·5	2·84	446	18·6	60·37	2·51	6	7	39·8	13·7	28·907	...	...	Experiments on water.
31.	...	66·5	4·5	413·6	17·2	66·4	2·77	2	5	37	8·5	29·440			
Feb. 1.	...	52	4·25	434·4	18·1	57·5	2·4	2	6	32	12·7	29·647			
2.	...	52	4·75	577·3	24	54·65	2·27	2	3	33·1	6·0	29·860	...	...	Dinner party.
3.	...	54	8·5	562	23·4	60·06	2·5	2	5	34·8	9·7	30·156			
4.	...	49·5	3·7	1266	26·3	57·73	2·4	2	4	37·3	14·8	30·113			
Sunday. 5.	...	59	13·8			58·66	2·44	4	4	44·9	7·3	29·805	...	...	Headache.
6.	...	48	3·4	494·7	20·6	67·85	2·82	5	5	37·3	7·7	29·795	...	...	Living on bread and water only.
7.	...	56	3·7	474·3	19·7	41·55	1·73	8	8	36·6	14·5	29·985	...	...	Living on bread and water and tea.
8.	...	48	6·9	465·7	19·2	52·04	2·16	8	8	44·5	9·2	29·509	...	...	Living on bread and water and coffee.
9.	...	55	7·5	382	15·9	51·73	2·15	8	8	33·9	9	29·632	...	...	Living on bread and water until 3 P.M.
10.	...	50·5	9·25	528·9	22	54·35	2·26	3	4	28·5	10	29·944			
11.	...	53	none	469·9	19·5	35·2	1·46	2	4	31	14·3	29·694			
Sunday. 12.	...	50	3·93	467·7	19·4	50·15	2·08	2	5	31	15·3	30·146			
13.	...	58·8	5·8	444	18·5	67·6	2·81	5	6	27·7	19·5	30·291			
14.	...	65·5	5·68	476·7	19·8	67·6	2·81	6	8	29·6	13·5	30·415			
15.	...	56	2·9	455·3	18·8	44·41	1·43	6	6	33·5	16·6	30·292			
16.	...	58	7·2	517	21·5	67·62	2·81	6	6	25·6	10·2	29·991	...	...	Evening party.
17.	...	51	3·35	428·7	17·8	45·15	1·88	6	6	37·4	10·3	30·268	...	...	No meat; took beef tea.
18.	...	61	6	516·4	21·5	53·6	2·23	6	6	40	7·8	30·158			
Sunday. 19.	...	49·5	6·53	506	21	48·2	2·0	5	6	39·1	13·7	29·488			
20.	...	60·5	5·25	380	15·8	82·15	3·42	9	9	33·4	9·5	29·343	...	...	Experiments on water; less food.
21.	...	51·5	12·45	414·8	17·3	46·04	1·91	5	7	36·1	8·4	29·612	...	...	Wine at night.
22.	...	76	1·5	476	19·8	50·1	2·08	9	9	35·5	12·2	29·876	...	...	Experiments on water and gluten bread.
23.	...	60·25	3·85	428	17·8	72	3	8	8	31·6	16	30·076	...	...	Evening party.
24.	...	55	7·1	352	14·6	48·56	2·02	7	7	31·5	16·2	30·057			
25.	...	62	6·4	362·6	15·1	44·84	1·45	6	6	33·8	17·9	29·926			
Sunday. 26.	...	54·75	6·15	430·2	17·9	50·63	2·11	5	5	44·6	18·6	29·206			
27.	40	57	6·25	421·1	17·3	77·54	3·23	9	9	39·7	19·1	29·229			
28.	35·75	48	6·4	552·6	23	53·54	2·31	4	7	42·3	21·3	29·366			
29.	42	56	4·5	510	21·2	41·6	1·74	4	7	38·2	16·6	29·899			
Mar. 1.	35·25	58	4·25	528	22	68·6	2·85	3	7	39·5	18·6	29·916			
2.	38·75	45	6·5	447	18·6	50·27	2·09	4	6	40	13·9	29·860			
3.	43·5	50	0	555	23·1	56·27	2·34	4	6	41·8	20·2	29·917			
Sunday. 4.	46	52·5	4·0	561	23·3	41·57	1·73	2	5	41·4	13	29·596			

	5.	51·25	48	4·7	523·9	21·8	55·6	2·31	4	6	40·1	12·7	30·145	...	...			
	6.	41·25	51	4·9	456	19	53·8	2·44	4	5	37·9	17·6	30·295	...	...			
	7.	50	48	0	527	21·9	44·3	1·43	4	6	33·7	11·5	30·088	...	...			
	8.	...	55·5	6·65	651	27·1	57·68	2·4	5	6	35	10·7	30·081	...	...	Public dinner.		
	9.	42	43	5·15	737·3	30·6	44·98	1·45	5	6	33·2	12	30·014	...	...			
	10.	32	...	...	...	...	...	...	2	6	30·9	6·5	29·747	...	...			
Sunday.	11.	37·75	...	...	...	...	...	...	3	6	35·6	17	29·437	...	...			
	12.	37	...	...	...	...	...	...	4	8	37·6	11·8	29·571	...	...			
	13.	41·25	...	...	...	...	...	...	4	6	39·3	14·9	29·416	...	...			
	14.	34	...	...	...	...	...	...	4	6	40·5	12·5	29·327	...	...			
	15.	35·5	...	...	...	...	...	...	4	6	38·1	16·5	29·723	...	...			
	16.	39·5	...	...	...	...	...	...	3	5	42·3	19·3	29·888	...	...			
	17.	38	...	...	...	...	...	...	4	7	48·3	7	29·936	...	...			
Sunday.	18.	55·25	...	...	...	...	...	...	3	5	46·2	19·7	29·885	...	...			
	19.	41·75	50	6·15	433·9	18·87	73·71	2·04	3	6	44·9	17·5	30·013	...	...			
	20.	37·5	52·5	4·5	481·1	20	50·35	2·09	3	5	46·5	13·3	29·750	...	...			
	21.	44	51	5·3	499·6	20·8	41·5	1·98	3	6	43·7	11·1	29·374	...	...			
	22.	42·25	53·5	3·3	488·7	20·36	41·59	1·98	3	6	39·8	16·8	29·679	...	...			
	23.	...	54	3·3	499·1	20·8	57·37	2·38	3	6	40·8	12·7	29·293	...	...	Dinner party.		
	24.	44·5	53	8·25	502·3	20·93	52·1	2·17	3	6	39·8	15·7	28·904	...	...			
Sunday.	25.	44·5	50	2·85	581·7	24·2	54·41	2·26	3	6	42·1	14	29·221	...	...			
	26.	40·5	44·5	...	471·3	19·65	63·65	2·65	3	6	40·3	13·3	29·529	...	...			
	27.	44	50·5	13·5	435·2	18	49·24	2·06	3	5	41·6	13·7	29·556	...	...			
	28.	42	55·25	...	431·7	17·56	28	1·16	3	5	50	16	29·565	...	...			
	29.	38·25	54	8·5	404·5	16·85	40·95	1·7	3	6	50·6	19	29·385	...	...			
	30.	39·5	56·5	2·5	375·1	15·6	60·4	2·5	2	4	45·3	9·8	29·529	...	...			
	31.	36	49·5	3·8	384·8	16	50·3	2·09	2	5	47·2	7·5	28·864	...	...			
Sunday.	Apr. 1.	41·5	48	...	389·3	16·2	60·05	2·5	3	6	47	14·1	29·052					
	2.	45·5	48	7·2	355·9	14	50·84	2·11	3	6	38·8	13·3	28·998	...	...			
	3.	39	42	...	376·9	15·7	45·48	1·89	3	5	44·1	9·1	29·426	...	...			
	4.	40·25	51·5	6·0	439·6	17·9	45·85	1·9	2	5	43·4	12·3	29·625					
	5.	...	...	3·4	445·9	18·5	33·5	1·4	1	4	46·1	17·5	29·720	...	...			
	6.	...	...	7·25	589·9	24·5	47·5	1·99	1	2	48	19·4	29·700	...	...			
	7.	...	...	5·5	544·3	22·6	44·25	1·82	1	2	52·2	14·1	29·770	...	...			
Sunday.	8.	...	...	5·4	604·8	25·2	44·7	1·83	1	2	49	15·6	29·843	...	...	In the country.		
	9.	...	...	11·75	511·3	21·3	58·7	2·44	2	2	46·8	15·3	29·408	...	...			
	10.	...	...	3·1	463·1	17·6	46·7	1·94	3	4	39·3	18·6	29·684	...	...			
	11.	40·5	57	3·5	419·8	17·2	61·45	2·55	6	6	37·1	17·8	30·041					
	12.	41	49·5	13·8	410·8	17·1	46·83	1·95	6	6	40·5	12·2	29·974	...	...	Evening party.		
	13.	38·75	51	1·65	503	20·9	57·37	2·39	5	5	40·9	15·8	29·857					
	14.	...	57·5	5·4	509	21·2	77·3	3·2	7	7	37·6	15·2	30·053					
Sunday.	15.	44·75	50	...	419·5	17·5	42·4	1·78	4	4	44·5	22	30·128					
	16.	39·8	58	6·4	461·6	15	61·35	2·56	6	7	46·3	30·7	30·138					
	17.	41·5	57·5	4·2	513·9	21·4	64·69	2·69	6	6	44·9	32·1	30·059					
	18.	41	65·25	6·65	518·4	21·6	64·07	2·66	6	6	45	19·7	29·933	...	...			
	19.	39	77	8·9	534·9	22·2	88·82	3·7	17	17	37·3	10·9	29·918	...	...	Experiments on water; evening party.		
	20.	27	54	3·75	484·3	20·1	42·35	1·76	6	6	39·6	9·2	29·704					
	21.	44·25	39	...	538·8	22·4	57·03	2·37	6	6	38·7	19	29·605	...	...	Soirée.		
Sunday.	22.	52	53	4·2	473	19·7	54·14	2·24	4	4	38	22	29·674					
	23.	36·5	61	...	518·2	21·6	67·76	2·81	7	7	39·9	17·9	29·571					
	24.	38·75	47	9	483·5	20·1	64·6	2·69	7	7	36·6	9·5	29·619					
	25.	34·75	57	4·35	533·2	22·2	76·7	3·39	7	7	44·4	16	29·812					
	26.	28	45·75	...	611	25·4	47	1·95	6	6	43·6	15·5	30·026	...	...	Evening party.		
	27.	37	43	4·4	565·6	23·3	41·94	1·74	5	6	42·3	18	30·120					
	28.	...	...	...	511·9	21·3	29·46	1·22	2	5	43·4	27	30·160	...	...			
Sunday.	29.	...	...	...	713·7	29·7	42·7	1·73	1	4	47·4	32·2	30·204	...	...	In the country; black dose; purged.		
	30.	...	...	...	684·6	28·5	64·77	2·7	4	5	50·4	27	30·273	...	...			

Table I. (continued).

Date.		Ingesta.		Egesta.					Daily number of observations and analyses.		Greenwich.			Weight—13 stones (182 lbs.).		Weather, state of health, and other observations.
				Fæces.	Urea.		Urine.				Temperature.		Barom. daily average.			
		Solids.	Fluids.		Total daily to 8 A.M.	Hourly.	Total daily to 8 A.M.	Hourly.	Urea.	Urine.	Daily average.	Daily range.		Night.	Morning following.	
		oz.	fluid oz.	oz.	grs.	grs.	fluid oz.	fluid oz.			° F.	° F.	inches.	lbs. oz.	lbs. oz.	
1860. May	1.	35·5	58·5	11·7	545·7	22·7	35·6	1·48	5	6	53·1	27·6	30·125	...	...	In the country; black dose; purged.
	2.	35	57·25	34·7	528·4	22	25·2	1·05	5	7	54·8	25·3	29·931			
	3.	36·5	58	6·1	505·5	21·6	37·1	1·54	5	5	52·4	25·6	30·037			
	4.	37	49·5	7·25	473	19·7	39·6	1·65	5	5	51·3	30·2	30·034			
	5.	36·5	55	1·6	576·7	24	50·4	2·1	6	6	45·7	14·7	30·009			
Sunday.	6.	35·75	49	...	655·2	27·2	52·5	2·18	4	4	43·7	21	29·964	...	...	Experiments with gluten bread.
	7.	33	58	6·25	562·2	23·4	49·6	2·06	7	7	48·6	30·5	29·621			
	8.	37	48·5	3	553	23	40·5	1·7	5	5	52·6	20·3	29·449			
	9.	34·5	57·5	3·2	599·7	25	48·9	2·0	6	6	53·3	18·1	29·555	...	...	Eggs and bacon.
	10.	31·75	59·5	5·25	595·3	24·8	57·2	2·38	7	7	52·7	25·7	29·686			
	11.	41	49·5	3·4	454	19	37·4	1·55	5	6	56·3	10·9	29·630	...	...	Ale and full supper.
	12.	37	63	...	595·7	24·8	44	1·41	5	5	58·3	15·0	29·567			
Sunday.	13.	39·75	46	8·3	531	22	60·9	2·54	6	6	55·2	16·2	29·630			
	14.	35·5	65	4·15	509	21·2	55·1	2·29	7	7	54·1	16·5	29·705	...	...	Experiments on alcohol.
	15.	41	50	4·1	516·7	21·5	43·5	1·8	4	5	56·6	20·1	29·720			
	16.	...	...	2·6	560	23·4	53·5	2·2	6	6	51·9	18·6	29·745	...	...	Public dinner.
	17.	32·25	41·5	6·8	435	18·45	25·3	1·05	3	4	53·2	7·5	29·459			
	18.	43·5	46	10·4	517·3	21·5	24·9	1·04	4	5	54·2	19·2	29·269			
	19.	36	62	3·6	504·4	20·96	43·5	1·6	5	7	55·3	26	29·669	...	...	Experiments on alcohol.
Sunday.	20	37·75	49	1·5	574·6	24	45·1	2·3	4	5	59·5	26·3	30·046			
	21.	...	52·5	...	520·3	21·7	60·7	2·53	5	5	60·3	29	30·208	...	...	Experiments on alcohol.
	22.	...	58	8·15	486·4	20·2	44·3	1·89	6	6	61·4	26·9	30·083			
	23.	40	56	...	423·2	17·6	36·8	1·53	3	3	60·6	31	29·786			
	24.	37·25	54·5	5·25	532·2	22·5	49·1	2·04	5	6	59·3	22·9	29·857	...	...	Experiments on alcohol.
	25.	41·75	46	4·6	594·8	24·76	61·6	2·56	2	5	55·9	20·1	29·703	...	...	Went to country until 30th.
	26.	...	...	4·6	539·6	22·45	34·9	1·04	1	...	56·3	16·7	29·968			
Sunday.	27.	...	...	...	599·2	25	36·7	1·5	1	...	50·4	20·4	29·639			
	31.	42·5	72·5	4·6	655·7	27·26	54·5	3·0	3	6	49·7	11·2	29·555			
June	1.	33·5	57	3·75	519·6	21·68	65·5	2·62	5	6	55·4	18·8	29·542	...	...	Experiments on alcohol.
	2.	30·25	54	5·5	642·2	26·7	72·5	3·0	3	7	52·7	15·3	29·144			
Sunday.	3.	35·75	52·5	0·8	517	21·58	54·1	2·25	3	6	52·7	9·7	29·322			
	4.	34·75	...	5·4	497·2	20·7	69·8	2·9	4	6	53·7	16·4	29·805	...	...	Ill; headache; vomiting.
	5.	29·75	68·5	13·9	596·8	24·64	35·6	1·48	3	5	52·5	15·8	29·696	...	...	} Milk.
	6.	38	58·5	1·5	555·7	23·08	58·3	2·42	3	6	50·1	19·1	29·636	...	...	
	7.	39	71	2·5	520	21·7	63·3	2·64	3	6	49·6	13·5	29·634			
	8.	36·25	51	3·7	507·5	21·1	61·6	2·56	3	6	53·3	19·5	29·713			
	9.	43·5	65·5	...	541·8	22·5	67·4	2·8	4	7	51·2	8·8	29·522	...	...	Milk; experiments on oxygen; not well.
Sunday.	10.	37	55·75	3·4	590·8	24·6	59·6	2·48	3	6	51·9	16·5	29·576	...	...	Milk.
	11.	40·5	68	3·9	583·5	24·31	68·2	2·84	6	7	54·2	21·7	29·696	...	...	Milk.
	12.	28·5	51·4	4·8	601·3	25	66·1	2·75	3	6	54	9·3	29·353			
	13.	35·5	59·5	6	553·3	23·05	50·1	2·14	3	5	53·9	16·5	29·474	...	...	Milk.
	14.	34·25	49·5	...	493·7	20·78	58·5	2·44	3	6	53·2	20·9	29·590			
	15.	37·25	53	4·2	547	22·8	52·3	2·18	2	4	53	23·4	29·541	...	...	Milk.
	16.	...	...	lost	547·3	22·8	32·4	1·35	2	4	57	18·6	29·502	...	...	} In the country.
Sunday.	17.	...	...	,,	638·4	26·6	59·3	2·47	1	...	50·8	6·8	29·412	...	...	

	18.	...	...	,,	475	19 8	31	1·29	1	3	56·6	21·1	29·669			In the country.
	19.	...	...	7·5	446	18·5	25·5	1·06	2	...	53·7	17·2	29·617			
	20.	29·75	68·5	8·1	522·6	21·76	34·5	1·44	3	4	58·6	14·8	29·510			
	21.	40·75	53	...	510·4	21·2	59·1	2·46	3	5	57·6	9·7	29·669			
	22.	37	57	4·6	416·2	15 5	40·7	1·7	3	5	59·1	20·1	29·856			
	23.	35	61	...	404·9	16·75	39·9	1·66	3	5	60·5	18·3	29·795			
Sunday.	24.	35·5	53·5	4·0	480·5	20	53·5	2·21	3	6	62·6	16·7	29·813			
	25.	25·25	72	7·3	487	20·28	52·1	2·2	2	4	57·4	15·8	29·622			
	26.	37	44	12·1	489·6	20·4	67·8	2·82	3	8	57·3	20·2	29·785			
	27.	...	...	lost	528	22	33·9	1·37	2	5	56	12·9	29·731			
	28.	...	...	,,	674·5	28·14	44·4	1·43	1	3	57·6	17·6	29·843			In the country.
	29.	...	...	,,	551·7	22·98	27·8	1·16	1	3	55·8	16·9	29·726			
	30.	...	...	,,	512·2	21·34	27·9	1·17	1	3	53	12·9	30·047			
July	3.	...	...	,,	526·2	21·92	39·8	1·66	1	4	61·7	15·7	30·178			In the country.
	4.	38	59	,,	535·3	22·3	34·9	1·45	2	...	61·7	21·1	30·019			
	5.	39·5	57·5	3·75	510·5	21·27	39·5	1·6	1	...	56·3	31·7	30·079	...	...	Milk.
	6.	40	58	4·75	472	19·4	60·5	1·48	1	5	57·2	15·8	30·163			
	7.	38	51·5	3·55	594·6	21·83	69·4	1·6	2	4	55·1	24	30·145			
Sunday.	8.	40·75	60	6·2	518·9	21·62	74	3·08	2	4	54·8	27	30·075	...	...	Milk.
	9.	39·25	59	4·6	502·6	20·94	47·6	1·98	2	4	56·8	18·4	29·984	...	...	Milk.
	10.	42·25	63	7·35	509·2	21·21	61·4	2·55	2	5	53·9	10·7	29·897	...	...	Milk.
	11.	39·5	52·75	2·1	466·8	19·45	54·2	2·24	4	5	59·5	20·2	29·866	...	...	Soirée.
	12.	33·25	52·5	3·1	465	19·37	62·2	2·6	1	...	57·9	23·8	29·836			
	13.	30	60	4·7	456	18·16	46·7	1·84	2	...	58·2	24·6	29·768			
	14.	39	57	4·6	548	22·83	50·3	2·2	2	...	60·4	21·8	29·668			
Sunday.	15.	42	60	...	617·1	25·71	55·6	2·31	2	...	62·7	18	29·745	...	...	Milk.
	16.	34·75	48	11·5	463·3	19·3	51	2·12	2	...	58·4	8·5	29·771			
	17.	37	50	2·8	482	20·13	54	2·25	2	...	62·3	19·5	29·738			
	18.	38·25	54	3·0	464	19·33	40·3	1·7	2	...	58·1	4·8	29·700			
	19.	34	58	3·6	464·7	19·34	47·2	1·96	2	...	57·4	16·5	29·552	...	...	Headache.
	20.	41·5	67	15·7	500·3	20·84	47·4	1·97	2	...	58·9	19	29·704	...	...	Well; purged.
	21.	30·5	52	...	420	15·83	50·6	2·1	2	...	55·6	16·7	29·567			
Sunday.	22.	35·75	65	2·15	520·4	21·6	52·3	2·18	2	...	58·5	18·8	29·595			
	23.	37·75	52	5·9	509·6	21·19	68·4	2·83	2	...	53·8	14·7	29·600			
	24.	34	53	4	482·4	20·13	61·4	2·55	1	...	54·4	16·7	29·674			
	25.	39	58·5	6·2	505·2	21·08	47·3	1·96	2	...	53·6	15	29·855			
	26.	34·5	56	4·75	537·1	23·32	62	2·58	2	...	54·8	22·3	29·806			
	27.	37·5	53 5	5·8	487·2	20·3	60·3	2·5	2	...	57·5	19·6	29·663			
	28.	26·25	57	3·6	499	20·8	56·4	2·35	3	...	56·4	15·2	29·601			
Sunday.	29.	36	...	...	534·4	22·26	54·2	2·25	2	...	57·5	19·5	29·776			
	30.	...	...	20·4	515	21·5	55·5	2·31	2	...	56·9	16·3	29·688	...	...	Went into country.
	Aug. 10.	...	...	...	623	27	59·4	2·47	1	4	57·5	21·7	29·716			
	11.	...	...	...	725	30·21	76·5	3·18	1	4	58·3	15·9	29·428			
Sunday.	12.	...	...	...	614	25·5	77·5	3·18	1	4	57·9	11·9	29·595			
	13.	...	...	...	604·3	25·15	56	2·33	1	4	58·3	17·1	29·605			
	14.	...	...	...	732·5	30·5	59·6	2·48	1	5	59·2	16·2	29·549			
	15.	...	...	...	534·5	22·27	49·9	2·08	1	5	59·3	16·7	29·518			
	16.	...	...	...	748·5	31·18	61	2·54	1	4	61·1	14·6	29·232			
	17.	...	...	...	575·4	24	73·1	3·04	1	4	56·2	15·4	29·394			At Scarborough.
	18.	...	...	...	...	...	...	...	1	...	52·9	11·2	29·448			
Sunday.	19.	...	...	...	717·8	25·74	63·8	2·65	1	3	58·7	11·1	29·702			
	20.	...	...	...	591	24·6	44·0	1·83	1	3	59	12·8	29·688	...	...	Walked eleven miles extra.
	21.	...	...	...	621·3	25·9	52·8	2·2	1	3	58·9	16·6	29·791			
	22.	...	...	...	659·9	27·5	37·5	1·81	1	3	56·1	11·8	29·393			
	23.	...	...	...	lost	...	lost	...	1	...	55·6	19·4	29·703			
	24.	...	...	...	714	29·77	43·6	1·81	1	3	54·4	13	29·591			

Date.	At Scarborough.		
	Temperature.		Barom. daily average.
	Daily average.	Daily range.	
1860.	° F.	° F.	inches.
Aug. 10.	57	7·7	29·700
11.	56·2	3·5	29·600
12.	56	2·0	29·795
13.	56·9	4·0	29·805
14.	55·8	5	29·775
15.	54·8	5·2	29·745
16.	57·6	2	29·450
17.	58·9	7·9	29·425
18.	54·2	6·5	29·500
19.	56·9	6·5	29·730
20.	59·4	11·5	29·725
21.	58·2	6·5	29·905
22.	56·5	5	29·637
23.	54·9	8·3	29·705
24.	53·8	13	29·650
25.	53·9	11	29·675
26.	57·9	6	29·755
27.	53·6	12	29·800
28.	52·8	11·2	29·760
29.	56·9	6	29·400

TABLE I. (continued).

Date.	Ingesta.		Egesta.					Daily number of observations and analyses.		Greenwich.			Weight −13 stones (182 lbs.).		Weather, state of health, and other observations.
				Urea.		Urine.				Temperature.		Barom. daily average.			
	Solids.	Fluids.	Fæces.	Total daily to 8 A.M.	Hourly	Total daily to 8 A.M.	Hourly.	Urea.	Urine.	Daily average.	Daily range.		Night.	Morning following.	
	oz.	fluid oz.	oz.	grs.	grs.	fluid oz.	fluid oz.			° F.	° F.	inches.	lbs. oz.	lbs. oz.	
1860. Aug. 25.	...	...	...	575	24	56·5	2·35	1	4	59·8	5·7	29·581			
Sunday. 26.	...	...	...	643	26·8	62·9	2·62	1	4	59	16·9	29·688			At Scarborough.
27.	...	...	...	579	24·12	46	1·91	1	3	56·8	17·3	29·713			Walked 14 miles extra.
28.	...	...	...	602	25·16	32·6	1·35	1	3	57·2	16·6	29·664	...	...	Dinner party.
29.	...	...	...	...	...	lost	...	...	...	59·7	16·5	29·391			
30.	...	...	...	...	...	lost	...	...	...	59·2	14	29·225			
31.	...	...	...	...	...	lost	...	...	...	57·4	17·6	29·425			
Sept. 1.	...	...	...	633·4	26·4	61·3	2·55	1	3	56·9	22·8	29·652			
Sunday. 2.	...	...	...	739·3	30·8	77·6	3·23	1	4	53·7	24·7	29·788			At Southport.
3.	...	...	...	693·8	28·9	80	3·3	1	4	55·2	18·5	29·953			
Oct. 8.	...	...	...	556·8	23·11	65	2·7	1	4	49	13·5	29·995	...	...	At home.
9.	...	...	...	434·1	18·1	49·6	2·06	2	4	45·9	10·5	29·880			
12.	...	...	...	465·5	18·98	53·4	2·22	1	...	38·6	14·6	29·747			
15.	...	...	...	529·7	22·07	61·4	2·55	1	...	49·7	17·5	29·521			
16.	...	...	...	620·2	25·84	72·8	3·03	2	...	49·5	11·7	29·468			
17.	...	...	...	546·8	22·7	65·8	2·74	2	...	50·5	11·1	29·832			
18.	...	...	...	462	19·25	40·2	1·71	1	...	54·6	12·3	29·498	...	...	Headache; deranged stomach; black draught.
19.	...	...	...	350	14·6	30·3	4·36	2	...	58·9	12·7	29·524	...	...	Still some headache.
20.	...	...	...	581·5	24·23	45·4	1·9	2	...	46	13·5	29·909			
Sunday. 21.	...	...	...	621	25·87	67·7	2·82	2	...	50	15	30·068			
22.	...	...	...	640	22·5	61·2	2·55	1	...	50·5	20	29·913			
23.	...	38	6·75	461·8	18·82	50·9	2·12	4	6	54·6	13·3	29·954	...	...	Felt as if could not take more fluid.
24.	...	61	2·5	520·4	21·7	39	1·62	4	4	56·4	10·3	29·907			
25.	...	...	4·9	523·6	21·81	50	2·15	3	...	56·5	11·7	29·907			
26.	...	...	...	608	25·32	67·3	2·80	...	...	54·7	11·1	29·806			
27.	...	...	...	608	25·12	79·2	3·3	1	...	53·4	12	29·819			
Sunday. 28.	...	...	...	...	...	...	...	1	...	57	18	29·920			
Nov. 12.	...	...	...	437	18·62	58·3	2·43	2	...	40·4	15·6	29·553			
13.	...	...	...	482	20·08	56	2·33	2	...	42·4	12·3	29·430			
14.	...	...	...	418·7	17·44	36·1	1·5	1	...	43·7	13·8	29·404			
15.	...	...	...	457	17·37	60·4	2·51	2	...	45·8	10·8	29·218			
16.	...	...	...	478	19·91	56·4	2·35	3	...	42·6	11·9	29·473			
17.	...	...	...	433	16·37	44·8	1·45	2	...	48·3	10·5	29·176	...	..	
Sunday. 18.	...	...	...	485	20·21	41·4	1·71	2	...	36·6	6·4	29·752	...	...	Constipated.
19.	...	...	...	413	15·54	49	2·04	4	...	37·8	9·5	29·967			
20.	...	...	...	428·7	17·86	55·4	2·3	4	...	41·1	13·6	29·857			
21.	...	...	...	446·3	18·6	53·4	2·22	2	...	41·7	13·8	29·550			
22.	...	...	...	489·8	20·41	62	2·58	3	...	43·5	12·1	29·364	...	...	Evening party.
23.	...	...	...	548	22·83	50·1	2·08	3	...	37·6	13·1	29·536	...	...	Colocynth; purged.
24.	...	...	...	403	17·62	48·1	2·0	2	...	39·9	5·3	29·588			A little headache and irritability.
26.	...	...	...	487·9	20·32	57·3	2·4	2	...	37·3	4·5	29·365			
27.	...	...	...			59·2	2·44			42·3	11·6	29·313			
28.	...	...	...	804·3	16·73	47·4	1·97	6	...	40·4	5·0	29·615	...	...	Milk.

	29.	...	...	...	424·7	17·7	49·7	2·04	3	...	41·7	6·5	29·606	...	...	Public dinner.
	30.	...	...	...	447·9	18·66	35·8	1·49	3	...	45·3	9·1	29·502			
Dec.	1.	...	...	6·55	539·6	22·48	49·2	2·05	3	...	45·5	12·2	29·639			
Sunday.	2.	...	...	...	425·8	16·07	45·6	1·9	4	...	45	7·3	29·473			
	3.	...	...	10·1	453	18·87	54	2·25	3	...	43·6	5·1	29·303	...	...	Purged; unwell from stomach derangement; less food.
	4.	...	...	5	426	19	37	1·54	3	...	44·6	5·6	29·315	...	...	A little better.
	5.	...	...	...	463	19·3	62·3	2·6	4	...	44·6	7·4	29·286	...	...	Headache; bad appetite; took colocynth.
	6.	...	...	25·25	449	18·71	44·1	1·84	3	...	49·8	8·5	29·108	...	...	Purged.
	7.	...	...	2·75	461	19·21	60·4	2·51	3	...	49·9	4·9	29·974	...	...	Well; good appetite; black draught.
	8.	...	...	15·75	380	15·83	46·3	1·93	2	...	44·8	6·6	29·702	...	...	Purged.
Sunday.	9.	...	...	5·0	398·6	16·61	38	1·58	3	...	42·7	6·7	29·858	...	...	Colocynth and black draught; purged.
	10.	...	...	21·2	455·7	18·98	60·2	2·5	2	...	41·4	9·5	29·356	...	...	Well.
	11.	...	...	5·2	430·3	17·51	57·7	2·4	4	...	39·6	10·2	29·528			
	12.	...	...	...	445·2	18·66	53·3	2·22	5	...	40·8	4·8	29·980	...	...	Evening party.
	13.	...	...	...	530·3	22·1	62·4	2·6	4	...	40·4	5·6	29·981			
	14.	...	...	4·0	561	23·3	51·6	2·15	3	...	38·8	5·1	30·120			
	15.	...	...	4·5	554·3	23·1	44	1·83	3	...	36·2	9·9	30·113			
Sunday.	16.	...	...	16·0	526·5	21·93	44·7	1·86	4	...	38·4	5·8	29·811	...	...	Bacon and coffee supper.
	17.	...	...	...	498·5	20·7	65	2·7	16	20	35·4	4·5	29·513			} Quarter-hourly experiments on urine.
	18.	...	...	...	473·1	19·71	65·38	2·71	15	15	30·5	7·9	29·414			
	19.	...	...	...	401	16·61	30·9	1·28	3	...	30·1	13	29·289	...	...	Frost began.
	20.	...	...	5·5	464·5	19·35	75·4	3·14	3	...	29	7·6	29·663			
	21.	...	...	...	364·9	15·2	30·3	1·26	3	...	30·6	5·9	29·724			
	22.	...	...	2·7	432	18·87	40·9	1·7	3	...	31	3·6	29·583			
Sunday.	23.	...	...	...	398	22	40·7	1·7	3	...	26	2·2	29·467			
	24.	...	...	6·8	506·3	21·1	49·0	2·46	3	...	22·4	11·7	29·313			
	25.	...	...	26·0	298	12·41	45·9	1·91	2	...	20·2	22	29·226	...	...	Headache; could not do anything; colocynth and black draught.
Sunday.	30.	...	...	...	476	19·83	30·3	1·26	3	...	38·3	12·7	29·290	...	...	Rapid thaw; hands swollen; feel full; faeces solid.
	31.	...	...	2·50	504·9	21·03	70·8	2·95	4	4	35·5	6·3	29·511			
1861. Jan.	1.	...	...	4·0	485·4	21·22	64	2·66	3	6	39·9	15·4	29·228			
	2.	...	...	6·0	470·2	19·6	61·9	2·58	2	...	29	7·2	29·918	...	...	Frost again.
	3.	...	...	12·5	559·1	23·3	55·6	2·31	2	...	28·7	9·0	30·143			
	4.	...	...	5·5	492	20·5	62·8	2·61	2	...	28	13·7	29·983			
	5.	...	...	1·5	503	21	38	1·58	2	...	27·5	10·7	29·928	...	...	Not well.
Sunday.	6.	...	...	15·6	451·5	18·4	47·1	1·86	3	...	23·6	10·3	29·932	...	...	A little headache; colocynth; purged.
	7.	...	...	5·1	437·5	17·81	68·1	2·83	4	...	26·4	12	29·966			
	8.	...	...	...	429	17·63	47·7	2·07	4	...	24·5	18·8	30·182			} Experiments on alcohol; French test.
	9.	...	...	5·5	436·6	18·2	49·6	2·07	3	...	25	18·2	30·179			
	10.	...	...	6·75	480·5	20·02	40·5	1·68	3	...	21·7	12·8	30·204			
	11.	...	...	...	495	20·62	42·5	1·77	4	...	28·7	19·7	30·184			
	12.	...	...	9·5	492·4	20·51	37·6	1·56	3	...	34·8	5·7	29·958	...	...	A little headache and stomach derangement; black draught.
Sunday.	13.	...	...	8·8	467·5	19·5	36·6	1·10	2	...	31	6·6	29·596	...	...	Bacon and coffee supper.
	14.	...	...	12	461	18·83	54	1·25	2	...	28	7·6	29·636	...	...	Well; frost again.
	15.	...	...	3·45	441	18·73	36·3	1·51	3	...	27·6	3·4	29·921	...	...	Experiments on alcohol at Society of Arts.
	16.	...	...	...	495·8	20·66	45	2·29	2	...	26·9	7·9	30·048			
	17.	...	...	8·2	552	23	49·5	2·04	1	...	33·5	6·7	30·134			} Thaw; heavy; tired; full; better at night.
	18.	...	...	8·75	441	18·5	41·4	1·89	1	...	32·9	6·1	30·129			
	19.	...	...	...	408	17	39·1	1·63	2	...	33·8	4·2	30·088			
Sunday.	20.	...	...	4·25	472·3	19·67	32·8	1·36	2	...	39·5	9·3	30·221			} Bacon and coffee supper.
	21.	...	...	15·5	418	18·25	61·5	2·56	3	...	39·9	6	30·238	...	...	Well.
	22.	...	...	1·25	408·3	17·01	60·6	2·52	4	...	35·6	7·3	30·230			
	23.	...	...	11·25	452·7	17·2	48·6	2·02	2	...	36·3	5·7	30·086	...	...	Felt as if I had taken too much tea, coffee, and bread.
	24.	...	...	...	487·9	20·32	33·9	1·83	2	...	40·6	15·9	29·896			} Milk.
	25.	...	...	...	609·4	25·4	51·1	2·13	3	...	48·5	10·4	29·790			
	26.	38	52·5	6	498	20·76	44·6	1·85	2	...	46·8	10·4	30·160	...	...	Frosty; fine; feel cheerful.
Sunday.	27.	36·25	53·5	4·5	510·1	21·25	49·4	2·05	4	...	47·1	13·3	30·124	...	...	Feel full.
	28.	36	59·5	...	586	24·41	70	2·91	3	...	41·7	14·4	30·107	...	...	Experiments on alcohol.
	29.	...	43·1	5·6	413	15·54	43·1	1·8	3	...	40·6	25·5	30·006			
	30.	...	...	3·7	500	20·83	39·6	1·65	2	...	37·8	13·5	30·016			
	31.	...	...	8·5	649	27·04	49	2·04	2	...	42·8	12·5	30·029	...	...	Dined out; strong wine; bad headache; vomiting; took aperient.

TABLE I. (continued).

Date.		Ingesta.		Egesta.					Daily number of observations and analyses.		Greenwich.			Weight —13 stones (182 lbs.).		Weather, state of health, and other observations.
				Fæces.	Urea.		Urine.				Temperature.		Baromdaily average.	Night.	Morning following*.	
		Solids.	Fluids.		Total daily to 8 A.M.	Hourly.	Total daily to 8 A.M.	Hourly.	Urea.	Urine.	Daily average.	Daily range.				
		oz.	fluid oz.	oz.	grs.	grs.	fluid oz.	fluid oz.			°F.	°F.	inches.	lbs. oz.	lbs. oz.	
1861. Feb.	1.	36·75	49	21·25	472	19·66	25·5	0·98	3	...	46·4	15·4	30·076	...	...	Feeble.
	2.	24·75	58	...	507	21·12	60	2·5	2	...	38·6	10·4	30·521			
Sunday.	3.	36	58	...	548	22·83	57·6	2·41	3	...	39·6	12·7	30·253	...	...	Irritable; drinking fine Oolong tea.
	4.	45·25	45	5·4	547·5	22·83	64·2	2·67	5	...	41·7	9·5	29·792	10 3	9 6	Well.
	5.	...	52·5	6	505	21·04	53·8	2·24	3	...	44·2	5·5	29·492	9 13	8 15	Restless.
	6.	33·75	...	10·2	532	22·16	49·8	2·07	3	...	45·7	6·9	29·263	9 14	9 2	Colocynth.
	7.	...	46·5	4·75	593	24·71	53·3	2·18	2	...	44	12·2	29·419	8 13½	8 2	Much exertion; less food.
	8.	...	51	7·7	508	21·16	40·8	2·72	2	...	43·9	8	29·384	9 2	8 8	
	9.	47·75	...	25·15	439·5	18·31	40·8	1·7	2	...	41	5·5	29·547	9 4	8 6	Colocynth and black draught; longing for bacon.
Sunday.	10.	...	...	4·5	526·6	21·94	44·4	1·85	3	...	37	10·9	30·107	10 9	10 1	Bacon.
	11.	33·5	61	2·6	584·6	24·36	67·7	2·82	3	...	31·9	11·7	29·736	8 6	7 15	Hard day's work; frosty.
	12.	...	58	3	489	20·37	49·4	2·04	3	...	30·2	13·4	29·517	9 6	8 10	
	13.	29·5	58	...	517	21·54	55·7	2·32	2	...	36·1	12·2	29·561	8 3	7 1	Hard day's work; feel full; thaw.
	14.	40·5	52	8·8	480·8	20	39·7	1·65	2	...	36·2	16·6	29·859	9 0	8 6½	Very damp.
	15.	...	...	...	528·5	22·02	61·2	2·55	2	...	44·5	9·5	29·513	9 4	8 9½	
	16.	...	58	6·25	488·4	20·35	63·9	2·66	4	...	47·2	11·3	29·584	8 2	7 6½	
Sunday.	17.	33·75	59	4	427	17·37	31·6	1·31	3	...	45·5	16·2	29·501	9 13½	9 1½	
	18.	32·5	62	...	486·4	20·27	49·2	2·05	3	...	45·5	13	29·533	11 2½	10 6	
	19.	...	52·5	6·6	462	19·2	35·5	1·48	4	...	43·6	13·3	29·578	10 6	9 13	Herring disagreed.
	20.	33·75	54	5·75	474	19·7	55·6	2·25	5	...	48·6	8·5	29·501	8 13	8 5½	Very rainy.
	21.	34·75	64·5	3·4	543	22·6	87·1	3·62	3	...	46·9	11·3	29·523	9 8	8 13½	
	22.	43	60	5·4	552·7	23	32·9	1·37	2	...	45·6	5·2	29·437	9 14½	9 3	
	23.	35·5	60	4·5	561·7	23·4	51·3	2·14	3	...	51·9	4·0	29·781	12 1½	11 5½	Very rainy.
Sunday.	24.	43·5	67·5	3·4	477·7	19·9	44·4	1·85	3	...	41	6·6	30·039	10 5½	9 13½	A little headache.
	25.	40·25	60	6·5	483·7	20·1	65·1	2·71	7	...	41·8	16·8	30·009	9 15	9 12	
	26.	42	62	10·6	418·5	17·8	62·7	2·61	4	...	40·2	19·6	29·855	11 15	10 13½	
	27.	39·5	60	2	360·8	15	40·4	1·7	3	...	40·2	19·6	29·855	11 15	10 13½	
	28.	33·75	66	4·2	445	18·5	65·1	2·71	2	...	44·5	12·9	29·684	10 15	10 9½	
Mar.	1.	...	56	3·5	456	19	55·8	2·32	16	...	44·9	14·5	29·491	9 10	9 13	Hourly experiments on the urine.
	2.	43	68	7·9	438·8	22·4	63·1	2·63	16	...	45·4	13·5	29·744	10 11	10 0	Ditto; large dinner.
Sunday.	3.	41·75	65	1·5	476·7	19·6	44·6	2·85	3	...	46·2	13	29·553	12 1½	10 9	Black draught; last quantity of fæces had peculiar animal odour.
	4.	36	60	28·3	473	19·7	55·4	2·30	3	...	41	11	29·920	10 0	9 14	
	5.	37·5	49	3·8	432·8	16·3	61·9	2·57	3	...	41·4	13·8	30·001	9 5	9 6	
	6.	...	58	...	467·3	19·4	36·3	1·5	3	...	48·6	9·8	29·636			
	7.	...	54·5	lost	631	26·2	57·3	2·4	2	...	44·9	12·9	29·967			Out of town.
	8.	...	54	...	436·4	18·1	36·8	1·53	2	...	50·3	16·8	29·890			
	9.	...	...	lost	lost	...	lost	...	...	...	43·9	16·9	30·241	10 8	9 7	
Sunday.	10.	...	59	4·5	496	20·6	31·9	1·33	3	...	46·7	19·9	29·709	12 6	11 14	
	11.	38·25	55	...	473·1	19·7	60·5	2·52	3	...	40·6	13·5	29·200	11 15	10 12	
	12.	47·25	68	5	444·1	18·4	59·6	2·48	3	...	40·8	15·1	29·240	12 4	11 10	
	13.	38·5	79	5·5	574·9	23·9	69·3	2·9	4	...	40·7	13·8	29·771	12 9½	11 8	
	14.	36	62·5	16·25	460·7	19·2	92·6	3·94	3	...	41·7	23·7	30·097	9 6	8 13½	Headache; stomach deranged; great sensibility; aperients.
	15.	22·75	70·5	26·75	411·4	17·1	26·5	1·1	2	...	45·5	16·3	29·777	9 13½	9 5	
	16.	34	59	1·5	455·6	17·3	58	2·41	2	...	40·8	15·5	30·097	10 12	10 4	Depressed.
Sunday.	17.	36·25	63	1·1	451·8	...	36·2	...	3	...	...	...	...	13 6	12 13	Well; soirée.

* As the weight in the morning is due to the conditions of the previous day, it is inserted opposite the previous day.

	18.	...	...	...	467·1	...	57·3	...	...	...	...	...	...	12 5	12 4	Dined out.
	19.	...	...	...	...	...	...	...	...	...	...	...	...			
	20.	...	...	...	...	...	...	...	...	...	...	...	...	12 10	11 1	
	21.	...	...	...	...	...	...	...	...	...	...	...	...	12 9½	11 7	
	22.	...	...	...	...	...	...	...	...	...	...	...	...	12 10	11 8	
	23.	...	...	...	...	...	...	...	...	...	...	...	...	14 2	12 10	
Sunday.	24.	...	...	...	479	20	44·3	1·84	...	...	48·9	24·4	29·651	14 8	13 10	
	25.	...	...	...	410·7	17·1	59·5	2·5	...	...	42·8	6·5	29·747	13 10½	11 8	Skin dry, hot; headache; colocynth.
	26.	...	...	...	342·8	14·28	49·7	2·07	...	...	47·5	16·9	29·496	12 10	11 10	
	27.	...	...	...	540·9	22·5	40·9	1·7	...	...	49·2	17·6	29·316	14 2	13 6	Dined out.
	28.	...	...	...	537·1	22·3	60	2·5	...	...	46·4	16·5	29·374	14 6	12 13	
	29.	...	...	...	421·6	17·6	52	2·16	...	...	45·0	19·6	29·605	13 14	12 6	
	30.	...	...	...	531·3	22·1	57·1	2·38	...	...	43·4	12·3	29·496	13 6	11 14	A little headache.
Sunday.	31.	...	...	...	535·3	22·3	49·7	2·07	...	...	43·1	17·9	29·490	15 1½	13 6½	
	April 1.	...	...	...	514	21·5	65·7	2·74	...	...	41·5	19·1	29·612	13 14	12 10	Very wet; had cold bath.
	2.	...	...	...	442·8	18·4	59·4	2·47	...	...	43·3	18·4	29·558	13 6	12 10	Supped out.
	3.	...	...	...	565·4	23·5	60·8	2·5	...	...	45·2	16·5	29·600	14 0	12 6½	
	4.	...	...	...	561·3	23·3	64·6	2·7	...	...	47·1	15·5	29·667	13 13	11 5	
	5.	...	...	...	366·7	16·1	37·5	1·56	...	...	44·0	17·7	29·785	12 10	11 14½	
	6.	...	...	...	475·2	19·8	33	1·37	...	...	40·2	21·8	30·031	14 0	12 10	
Sunday.	7.	...	...	...	458·7	18·7	69·3	2·98	...	...	42·1	14·2	30·125	14 6	12 10	
	8.	...	...	...	456·4	17·6	64·5	2·6	...	...	41·1	17·1	30·278	12 6	11 4	
	9.	...	...	...	457	19·1	44·4	1·85	...	...	41·7	20	30·396	13 6	12 11½	
	10.	...	...	...	463	19·8	47·8	2	...	...	42·7	22·7	30·364	13 13½	12 8	
	11.	...	...	...	433	18	35·2	1·47	...	...	45·6	32·6	30·297	13 10	12 8	Full; hands and feet swollen; headache.
	12.	...	...	...	486	20·2	43·8	1·82	...	...	49·4	25·2	30·222	12 7	11 4	Out of town.
	13.	...	...	...	500	20·8	38·2	1·6	...	...	46·7	13·8	30·135	12 14	11 15	
Sunday.	14.	...	...	...	489	20·6	44	1·76	...	...	44·2	6·3	30·130	15 10½	14 2½	
	15.	...	...	...	536·2	22·3	74·8	2·1	...	...	45·2	12·4	30·162	13 12	12 6	Dined out.
	16.	...	...	...	513·3	21·3	33	1·33	...	...	49·9	21·7	30·235	14 8	12 9	
	17.	...	...	...	545·3	22·7	53·1	2·2	...	...	46	17·8	30·179	14 8	13 1½	
	18.	...	...	...	461·5	19·1	62·5	2·6	...	...	46·6	15·5	30·056	13 12	11 10½	
	19.	...	...	...	456·3	19	46·1	1·92	...	...	44·9	12·9	30·067		12 5½	
	20.	...	...	...	419	17·4	29·3	1·2	...	...	41·3	21·2	30·066	13 13½	13 0	Much oppressed; weary; stiff; sperient.
Sunday.	21.	...	...	...	509·9	21·2	41·8	1·74	...	...	40·2	27·4	29·743	16 0	14 10	Well.
	22.	...	...	...	510·3	21·3	73·6	3·07	...	...	44·1	21·7	29·738	14 3	12 14	
	23.	...	...	...	388	16·1	38·6	1·61	...	...	45·1	15	29·733	14 12	13 7	
	24.	...	...	...	493·4	20·5	63·2	2·63	...	...	40·3	27·1	29·866	15 10½	13 15	
	25.	...	...	...	388·7	16·1	68·2	2·8	...	...	49·5	22·1	29·884	13 8	12 10	
	26.	...	...	...	386	16·1	30	1·25	...	...	48·9	19·5	29·957	14 14	13 13	
	27.	...	...	...	...	...	63	2·61	...	...	36·2	6·8	29·960	16 2	14 1	
Sunday.	28.	...	...	...	...	...	...	...	...	...	49·8	24·6	29·915	16 9½	15 4	
	29.	...	...	...	...	...	...	...	...	...	43·1	15·6	30·058			
	May 13.	...	...	...	525	22	57	2·37	...	...	45·1	12·6	30·106	...	...	Crab.
	14.	...	...	...	469·1	19·4	47·7	2	...	...	51·5	31·3	30·257	15 6	14 5	
	15.	...	...	...	430·3	17·9	30·4	1·25	...	...	57·7	24·6	30·176	15 7	14 6	First hot day; beer.
	16.	...	...	...	510·4	21·8	55·6	2·31	...	...	61·1	29·6	30·014	15 10	13 13	
	17.	...	...	...	484·2	20	37·5	1·57	...	...	48·7	21·2	30·009	16 8	15 6	Crab.
	18.	...	...	...	556·3	23·1	61	2·54	...	...	46·8	22·7	30·134	16 12	14 13	Milk.
Sunday.	19.	...	...	...	562	24·1	41·5	1·83	...	...	46·9	24·2	30·245	18 1	16 8	
	20.	...	...	...	576·9	22	64·1	2·66	...	...	57·5	34·3	30·218	17 8	15 2	Dined out.
	21.	...	...	...	418	17·4	27·1	1·13	...	...	62·9	29·8	30·187	15 9	14 9	Very hot.
	22.	...	...	...	lost.	...	20·66	0·86	...	...	60·4	15·1	30·042			
	23.	...	...	...	559	23·2	32·93	1·37	...	...	64·9	28·7	29·902			Out of town.
	24.	...	...	...	618·3	25·8	47·8	2	...	...	55·0	16·8	29·831			
	25.	...	...	...	586·4	24·4	55·8	2·32	...	...	54·7	21·3	29·637			
Sunday.	26.	...	...	...	536	22·3	51·5	2·15	...	...	54·3	16·1	29·745			
	27.	...	...	...	509	21·2	48·2	2	...	...	58·5	23·7	29·930	18 0½	17 1	Evening party.

TABLE I. (continued).

Date.	Ingesta.		Egesta.					Daily number of observations and analyses.		Greenwich.			Weight—13 stones (182 lbs.).		Weather, state of health, and other observations.
			Fæces.	Urea.		Urine.				Temperature.		Barom. daily average.			
	Solid.	Fluids.		Total daily.	Hourly.	Total daily.	Hourly.	Urea.	Urine.	Daily average.	Daily range.		Night.	Morning following.	
	oz.	fluid oz.	oz.	grs.	grs.	fluid oz.	fluid oz.			° F.	° F.	inches.	lbs. oz.	lbs. oz.	
1861. May 28.	...	...	...	424·6	17·6	52·16	2·17	...	...	52·2	7	29·906	17 1	15 14	Full and swollen; patients all worse; dropsy prevails.
29.	...	...	...	441·3	18·4	52·5	2·19	...	...	58·0	23·6	29·799	17 10	16 0	
30.	...	...	...	414·7	17·2	48·0	2	...	...	62·8	23·9	29·790	16 2	14 10	
31.	...	...	...	396	16·4	24	1	...	...	60·1	21·8	29·814	16 13	15 9	
June 1.	...	...	...	480	20	41·4	1·74	...	...	56·5	13·7	29·008	18 8	17 2	
Sunday. 2.	...	...	...	486·8	20·3	55·9	2·33	...	...	54·4	17·5	29·736	19 8	17 9	Very full and drowsy; flesh shakes on movement.
3.	...	...	...	423	17·6	81·7	3·4	...	...	53·4	14	29·884	17 5	16 2	Cooler; lighter; large emission of urine.
4.	...	...	...	420	17·4	38·8	1·61	...	...	53·8	11	29·888	17 10	16 12	Evening party.
5.	...	...	...	538·6	23·7	44·8	1·98	...	...	55·4	17·6	29·833	18 12	16 11	
6.	...	...	...	521·6	21·7	52·5	2·19	...	...	51·3	11·4	29·877	17 14	16 13	
7.	...	...	...	536·4	22·3	47·3	1·97	...	...	52·9	12·7	29·796	18 4	17 2	
8.	...	...	...	459	19·2	38·1	1·6	...	...	52·1	11·3	29·743	18 10	17 3	
Sunday. 9.	...	...	...	496·5	22·3	56·7	2·55	...	...	51·2	18·3	29·563	19 4	18 1	
10.	...	...	...	560·6	23·3	63·1	2·63	...	...	55·8	15·7	29·695	18 7	17 5	
11.	...	...	...	lost.	...	59·3	2·47	...	...	58	22·8	29·944	17 7	16 2	
12.	...	...	...	583·1	24·3	39·9	1·66	...	...	60·1	15·8	30·027	17 10	16 12	Soirée.
13.	...	...	...	637·7	26·5	48·5	2·02	...	...	64·3	26·5	30·034	17 15	16 7	
14.	...	...	...	468	19·5	30	1·26	...	...	67	25·8	29·906	18 13	17 6	Very hot.
15.	...	...	...	512	21·36	36	1·5	...	...	66·9	25	29·848	19 13	18 10	
Sunday. 16.	...	...	...	508·6	21·13	42·6	1·77	...	...	63·2	24·6	29·881	20 12	19 1	
17.	...	...	...	503·5	20·8	68	2·82	...	...	58·9	22·7	29·921	19 0	17 10	Cooler.
18.	...	...	...	399	16·6	34·1	1·42	...	...	60·2	28·2	29·930	18 2	17 2	
19.	...	...	...	462·4	19·6	28·6	1·19	...	...	65·1	28·8	29·867	17 4	15 12	Dined out.
20.	...	...	...	535·6	22·18	28·4	1·18	...	...	64·7	24·3	29·781	17 11	16 6	
21.	...	...	...	430·8	17·86	26·5	1·1	...	...	63·9	30·1	29·759	...	...	Beer at night.
22.	...	...	...	524·5	21·9	35·5	1·48	...	...	64·5	18	29·682	18 6	17 2	Headache all day until night.
Sunday. 23.	...	...	...	517·3	21·54	32·1	1·34	...	...	61·7	20	29·600	19 12	18 2½	Heavy; a little headache.
24.	...	...	...	485	20·48	47·9	2	...	...	62·3	24·1	29·693	18 7	17 2	
25.	...	...	...	lost.	...	...	...	...	...	56·3	7·6	29·606	19 10	17 12	Dined out of town.
26.	...	...	...	471·7	19·7	57·2	2·38	...	...	57·8	19·8	29·492	18 7	17 0	
27.	...	...	...	461·5	19·15	64·1	2·64	...	...	58·5	29·3	29·588	18 10	17 0	
28.	...	...	...	462·2	19·6	41·2	1·71	...	...	61·8	25·3	29·652	19 1½	17 6½	
29.	...	...	...	481	20·3	42·6	1·8	...	...	62·2	15·9	29·680	18 14	17 6	
Sunday. 30.	...	...	...	464·6	19·34	55·7	2·32	...	...	57·4	17	29·915	19 2	17 9	The comet appears.
July 1.	...	...	...	465·8	19·35*	52·5	2·18	...	...	61·8	27·6	29·992	18 1	16 11	
2.	...	...	...	414·8	17·15*	30·7	1·27	...	...	62·5	17·1	29·795	17 1	16 1½	
3.	...	...	...	432·1	17·8	33·1	1·38	...	...	60·7	19	29·762	19 2	17 12	Very close and peculiar weather; exceedingly oppressed until after sunset; hot tea; longing for fat at night.
4.	...	...	...	462·6	18·8	41·6	1·7	...	...	57·3	16·3	29·324	18 6	17 3	
5.	...	...	...	431·5	18	47·3	1·93	...	...	58·6	16·9	29·198	17 6	15 15	Cooler.
6.	...	...	...	487·3	20·3	39·6	1·65	...	...	59·7	21·8	29·309		16 3	
Sunday. 7.	...	...	...	587·1	24·4	52	2·17	...	...	62·6	16·2	29·449	18 14	16 8	* Very tired and stiff; very giddy; lost weight, but had lived on calves' head and not flesh meat; all patients complain of giddiness; much skin disease; alkalies required to aid the action of the skin; diseases due to the season.
8.	...	...	...	c30	26·2	67·5	2·77	...	...	61·3	22·7	29·567	15 9*	13 11	
9.	...	...	...	539·2	24	59	1·72	...	...	62·1	22·1	29·752	15 12	13 13	
10.	...	...	...	615·2	25·65	39	1·6	...	...	60·7	21·9	29·802	15 12	11 14	Headache; unable to work; dull; sperient.

	11.	...	...	...	664	26·66	34·9	1·45	...	...	59·3	23·9	29·733	13	15	12	3	Two pints of milk; no giddiness.
	12.	...	...	...	500·6	20·85	29·8	1·24	...	...	60·9	23·1	29·506	13	8	12	3	Close weather; taste not natural; well.
	13.	...	...	...	503·5	21	31·6	1·32	...	...	63·3	17	29·381	14	14	13	2	
Sunday.	14.	...	...	...	500	20·8	32·6	1·36	...	...	61·7	16·5	29·458	16	7	15	0	
	15.	...	...	...	512·8	21·3	33·6	1·48	...	...	61·1	18	29·499	14	12	13	15	
	16.	...	...	...	452	21·1	38·3	1·6	...	...	62·1	21·6	29·556	15	14	14	14	
	17.	...	...	...	452·3	18·76	33·4	1·39	...	...	60·6	24·2	29·764	15	2	13	10	Soirée.
	18.	...	...	...	490·3	24·3	45·4	1·89	...	...	61·0	13·6	29·598	14	12	13	9	
	19.	...	...	...	516·6	21·53	46·5	1·94	...	...	59·6	10·5	29·573	14	9	13	6	
	20.	...	...	...	465	19·3	39·7	1·65	...	...	63·0	14·8	29·572	15	0	13	9	Much oppressed; tongue white; sperient.
Sunday.	21.	...	...	...	553·2	23	55·8	2·32	...	...	61·9	16·3	29·626	14	12	13	4	Milk supper.
	22.	...	...	...	545	22·7	47·3	2	...	...	61·8	14·4	29·613	15	10	13	9	
	23.	...	...	...	...	...	...	...	...	...	62·4	15·6	29·518	...		...		Out of town.
	24.	...	...	...	...	...	...	...	...	...	58·2	16·2	29·649	15	9	14	2	
	25.	...	...	...	...	...	...	...	...	...	63·7	11·8	29·525	16	5	15	5	
	26.	...	...	...	472	19·6	34·5	1·43	...	...	59·9	14·7	29·464	17	10	15	2	Little headache; hands hot and full.
	27.	...	...	...	560·7	23·3	62·3	2·6	...	...	59·6	23·5	29·601	17	7	...		Cheerful; much urine; milk at night.
Sunday.	28.	...	...	...	541	22·5	71·8	2·98	...	...	59·4	22·5	29·767	15	4	14	0	
	29.	...	...	...	497	20·6	66·2	2·75	...	...	59·7	24·2	29·846	14	9	13	8	
	30.	...	...	...	409	13·76	47·8	2	...	...	61·2	17·1	29·721	15	13	14	7	
	31.	...	...	...	446	18·51	36·5	1·52	...	...	60·8	23·1	29·883	15	14	14	3	Hot day; well.
	Aug. 1.	...	...	...	527	21·3	47·4	1·92	...	...	63·6	21	29·906	15	16	14	2	
	2.	...	...	...	525	21·2	39·4	1·6	...	...	63·5	29·5	29·593	15	11	14	2	
	3.	...	...	...	499	20·75	45	1·87	...	...	59·7	13·3	29·744	16	5	14	12	
Sunday.	4.	...	...	...	463	22·07	53·2	2·23	...	...	63·3	19·5	29·834			...		Out of town.
	5.	...	...	...	...	...	...	...	...	...	66·1	26·7	29·767					
	6.	...	...	...	...	...	...	...	...	...	63·0	3·3	29·930					
	7.	...	...	...	...	...	...	...	...	...	62·8	27·6	29·813					
	8.	...	...	...	611	23·22	35·5	1·35	...	...	63	11·9	29·565					
	9.	...	...	...	640	26·04	34·4	1·4	...	...	64·7	18·8	29·776					
	10.	...	...	...	652	27·36	43·4	1·9	...	...	68·2	21·8	29·927					
Sunday.	11.	...	...	...	649	22·07	45	1·88	...	...	67·7	21·8	29·817					
	12.	...	...	...	695	29	50·4	2·1	...	...	72·9	32·3	29·610					
	13.	...	...	...	715	29·79	37·1	1·52	...	...	67·0	20·4	29·812					
	14.	...	...	...	841	34·18	63·8	2·65	...	...	64·2	26·2	29·838					
	15.	...	...	...	760	30·9	55·1	2·24	...	...	65	13·1	29·671					
	16.	...	...	...	562	22·5	43·5	1·81	...	...	59·4	8·2	29·710					
	17.	...	...	...	641	26·6	56·2	2·39	...	...	60·8	24·5	29·879					
Sunday.	18.	...	...	...	643	26·6	46·5	1·93	...	...	63·8	22·1	29·829					At Scarborough.
	19.	...	...	...	724	30	61·8	2·57	...	...	63·9	20	29·808					
	20.	...	...	...	735	30·4	62·8	2·61	...	...	59·4	23·4	29·933					
	21.	...	...	...	723	30	52·8	2·2	...	...	58·3	22·2	30·068	...		...		Whisky at night.
	22.	...	...	...	547	23·4	46·7	2	...	...	59·9	20·4	30·089					
	23.	...	...	...	839	34·9	68·9	2·87	...	...	64·1	18·6	29·937					
	24.	...	...	...	876	36·45	54	2·25	...	...	59·3	19·1	29·946					
Sunday.	25.	...	...	...	746	34·7	68·9	3·13	...	...	59	22·2	30·029					
	26.	...	...	...	681	28·35	50·4	2·1	...	...	60·7	18·1	30·066					
	27.	...	...	...	668	27·8	33·4	1·39	...	...	65·4	25·3	30·104					
	31.	...	...	...	735	30·5	50	2·08	...	...	60·8	31·1	30·109					
Sunday.	Sept. 1.	...	...	...	624	25·94	45·2	1·88	...	...	66·1	27·6	29·888	...		17	2	In town.
	15.	...	...	...	564	23·4	37	1·54	...	...	52·5	14·1	29·607	...		18	5	
	16.	...	...	...	485	20·4	57·7	2·43	...	...	55·1	14·6	29·834	...		18	7	
	17.	...	...	...	464	19·3	69·6	2·9	...	...	53	20·5	30·013	...		17	6	
	18.	...	...	...	450	18·58	45·9	1·9	...	...	54·5	30·3	30·050					
	19.	...	...	...	432	17·94	55·3	2·3	...	...	57·3	24·3	29·960	...		17	5	
	20.	...	...	...	444	18·46	54·7	2·28	...	...	54·6	21·3	29·705	...		17	14	
	21.	...	...	...	lost.	...	...	...	...	...	53·7	18·4	29·568	...		18	6	

Date.	At Scarborough.		
	Temperature.		Barom. daily average.
	Daily average.	Daily range.	
1861.	°F.	°F.	inches.
Aug. 8.	63·9	16	29·650
9.	59·8	6·5	29·800
10.	60·9	10·5	30·025
11.	60·9	9·5	29·875
12.	66·9	12·5	29·850
13.	65·9	11·5	29·950
14.	61·6	15	29·950
15.	62·9	11	29·703
16.	63·8	13	29·710
17.	57·9	10	29·950
18.	58·9	10	29·850
19.	57·9	10	29·800
20.	58·4	12·5	29·810
21.	56·9	19	29·900
22.	59·9	10·5	29·890
23.	58·9	7	29·930
24.	58·7	9·5	29·930
25.	56·9	7	30·095
26.	58·7	3	30·155
27.	54·4	4·5	30·225
28.	58·9	11·5	30·053
29.	56·7	6·5	29·850
30.	49·2	3·5	30·050

TABLE I. (continued).

Date.	Ingesta.		Egesta.					Daily number of observations and analyses.		Greenwich.			Weight – 13 stones (182 lbs.).		Weather, state of health, and other observations.
				Urea.		Urine.				Temperature.		Barom. daily average.			
	Solids.	Fluids.	Fæces.	Total daily.	Hourly.	Total daily.	Hourly.	Urea.	Urine.	Daily average.	Daily range.		Night.	Morning following.	
	oz.	fluid oz.	oz.	grs.	grs.	fluid oz.	fluid oz.			° F.	° F.	inches.	lbs. oz.	lbs. oz.	
1861. Sept. 22.	...	...	...	454	18·9	50·4	2·1	...	...	55·3	9·8	29·380	...	18 14	
23.	...	...	...	523	21·84	67·1	2·8	...	...	55·7	13·7	29·273	...	18 0	Milk.
24.	...	...	...	lost.	...	...	...	...	...	54·7	19·5	29·287	...	17 9	
25.	...	...	...	524	21·75	60·2	2·5	...	...	50·6	10	29·166			
26.	...	...	...	527	21·97	70·2	2·93	...	...	51·5	13·9	29·584	...	17 0	
27.	...	...	...	461	19·15	54·88	2·28	...	...	52·8	28·5	29·807	...	16 8	
28.	...	...	...	...	...	...	...	...	...	55·7	13	29·716	...	16 8	Out of town.
Oct. 3.	...	...	...	526	22·64	36·9	1·53	...	...	57·5	20·2	30·000	...	18 10	
4.	...	...	...	540	22·5	60	2·5	...	...	58·6	16·4	29·956	...	17 1	
5.	...	...	...	491	20·5	42·8	1·8	...	...	57·5	19·0	29·886	...	18 5	
Sunday. 6.	...	...	...	503	20·92	52·3	2·18	...	...	56	3·6	30·021	...	18 4	
7.	...	...	...	542	22·54	76·4	3·18	...	...	60·6	16	29·885	...	16 8	
8.	...	...	...	400	16·66	49·8	2·07	...	...	62·1	25·3	29·688	...	17 4	Very close weather; all patients perspiring and depressed; much diarrhœa.
9.	...	...	...	550	23·36	47	2·04	...	...	58·2	20·3	29·796	...	17 2	
10.	...	...	...	526	21·66	47·4	1·97	...	...	56·5	22·1	29·834	...	18 0	
11.	...	...	...	474	19·66	52·2	2·17	...	...	56	21	29·346	...	17 0	Out of town.
12.	...	...	...	...	...	...	...	...	...	56·4	14·4	29·843	...	19 4	
17.	...	...	...	469	19·47	53·8	2·24	...	...	50·2	21·1	30·135	...	19 7	
18.	...	...	...	523	22·23	51·3	2·18	...	...	50·2	17·5	29·972	...	18 14	
19.	...	...	...	550	22·86	55·5	2·31	...	...	50·1	22·2	29·788	...	17 15	
Sunday. 20.	...	...	...	455	18·84	36·15	1·5	...	...	51·3	14·8	29·733	...	19 14	
21.	...	...	...	534	22·26	63·4	2·65	...	...	54·7	16·6	29·608	...	18 3	
22.	...	...	...	417	17·25	35·9	1·49	...	...	53·9	13·4	29·670	...	18 0	
23.	...	...	...	543	22·57	61·5	2·56	...	...	51·9	12·2	29·901	...	18 2	
24.	...	...	...	633	26·34	68·9	2·87	...	...	56·4	14·7	29·906	...	17 11	
25.	...	...	...	547	22·7	43·4	1·76	...	...	58·6	8·5	29·970	...	17 7	
26.	...	...	...	586	24·31	50·6	2·1	...	...	52·2	14·1	30·001	...	17 14	
Sunday. 27.	...	...	...	539	22·2	48·6	2·0	...	...	48·9	18·1	29·927	...	18 5	Easterly winds prevail.
28.	...	...	...	585	24·36	67·2	2·8	...	...	47	14·6	29·967	...	18 4	
29.	...	...	...	490	20·37	68	2·83	...	...	46·8	15·2	29·843	...	18 2	Very full; purged before breakfast.
30.	...	...	...	566	23·5	67·4	2·8	...	...	47·6	11·6	29·798			
31.	...	...	...	448	19	42·6	1·81	...	...	48·3	12·4	29·615	...	17 15	
Nov. 1.	...	...	...	405	16·83	45	1·87	...	...	41·4	12·6	29·179	...	17 13	Wind changed.
2.	...	...	...	492	20·42	57·3	2·38	...	...	38·2	12·6	29·185	...	18 0	
Sunday. 3.	...	...	...	418	16·77	53·2	2·21	...	...	39·2	13·8	29·668	...	...	Very cold.
4.	...	...	...	430	17·5	51·9	2·16	...	...	40·9	19·6	29·824	...	17 15	Aperient; headache; unable to do anything.
5.	...	...	...	460	18·08	51·12	2·12	...	...	47·9	13·8	29·483	...	...	Much warmer, and better.
6.	...	...	...	408	16·98	36·7	1·53	...	...	45·8	9	29·326	...	18 14	
7.	...	...	...	446	...	67·2	2·8	...	...	42·3	16·7	29·311			Aperient.
8.	...	...	...	400	16·64	35	1·46	...	...	39·4	14·9	29·210	...	18 9	
9.	...	...	...	522	21·75	64·8	2·9	...	...	38·4	14·3	29·397			
Sunday. 10.	...	...	...	447	18·6	48·1	2	...	...	42·2	18·5	29·087	...	19 0	
11.	...	...	...	462	19·18	66·9	2·78	...	...	43·7	13·1	29·527	...	18 6	
12.	...	...	...	402	16·68	49·3	2·06	...	...	42·5	9·4	29·607	...	17 5	

	13.	...	...	...	622	21·75	36	1·97	...	...	39·1	12·2	29·284		18	4		
	14.	...	...	...	440	18·7	58·7	2·5	...	...	40·7	11·5	29·168		17	8		
	15.	...	...	...	507	25·2	46·36	2·9	...	...	37·6	13·4	29·356		17	0		
	16.	...	...	...	439	18·24	45·7	1·9	...	...	32	13·1	29·466		18	4		
Sunday.	17.	...	...	...	401	16·3	46·7	1·9	...	...	30·9	8·9	29·863	...	19	8	Dined out.	
	18.	...	...	...	568	23·76	76·9	3·22	...	...	27·1	8	30·247		17			
	19.	...	...	...	433	17·9	30·4	1·26	...	...	34·1	18·6	30·352		18	4		
	20.	...	...	...	436	17·64	43·26	1·75	...	...	41·8	10·7	30·058		18	10		
	21.	...	...	...	441	18·36	49	2·04	...	...	47·3	16·3	29·663		18	5		
	22.	...	...	...	406	17·16	47·33	2	...	...	42·9	8	29·381		18	4		
	23.	...	...	...	418	17·34	40·9	1·7	...	...	38·1	11·3	29·327	...	17	7	Frost; headache; better at night.	
Sunday.	24.	...	...	...	507	21·1	54·5	2·27	...	...	30·6	11	29·892	...	17	4	Well.	
	25.	...	...	...	345	14·35	49·94	2·08	...	...	43·2	21·5	29·697		16	5		
	26.	...	...	...	392	16·27	27·2	1·13	...	...	53·1	9	29·427		17	10		
	27.	...	...	...	393	16·35	33·65	1·41	...	...	42·1	14	29·696					
	28.	...	...	...	524	21·77	46·4	1·93	...	...	41·1	15·7	29·831		18	12		
	29.	...	...	...	491	17·6	45·2	1·9	...	...	51·9	12	29·646	...	18	4	Dined out.	
	30.	...	...	...	516·5	21·4	42·2	1·75	...	...	49·7	12·8	29·681		19	14		
Sunday.	Dec. 1.	...	...	...	614	25·2	75·8	3·11	...	...	43·4	14·3	29·911	...	19	0	Cowheel supper.	
	2.	...	...	...	485	20·25	59·8	2·5	...	...	36·5	17·4	30·279		19	0		
	3.	...	...	...	482	19·0	53·5	2·22	...	...	37·5	15·1	30·129	...	17	2	Cowheel supper.	
	4.	...	...	...	488	20·7	56	2·38	...	...	37	17·9	29·937		18	5		
	5.	...	...	...	486	20·25	64·3	2·7	...	...	39·4	9·2	29·623	...	17	0	Sausages at supper.	
	6.	...	...	...	411	17·03	40·4	1·68	...	...	41·8	13·7	29·401		18	0	A little headache; colocynth.	
	7.	...	...	...	448	18·58	71·1	2·95	...	...	49·3	10·8	29·220					
Sunday.	8.	...	...	...	379	15·84	44·9	1·9	...	...	47·6	9·6	29·503	...	18	0	Purged.	
	9.	...	...	...	347	14·4	66·4	2·76	...	...	48·7	11·1	29·748		16	15		
	10.	...	...	...	415	17·26	33·7	1·41	...	...	49·1	9·5	29·672	...			Very warm and close.	
	11.	...	...	...	424	17·68	31·1	1·3	...	...	45·6	12	29·939	...	18	7	Hot.	
	12.	...	...	...	441	18·14	63·78	2·63	...	...	50·3	4	29·720	...	17	10		
	13.	...	...	...	413	16·3	46·3	1·92	...	...	50·6	5·2	29·329		17	4		
	14.	...	...	...	406	13·89	56·4	1·93	...	...	44·7	9·6	29·887	...	17	7	Full.	
Sunday.	15.	...	...	...	432	18·43	45	1·92	...	...	46·1	6·6	30·074	...	16	12	Colocynth; purged.	
	16.	...	...	...	492	20·43	50·3	2·1	...	...	47·8	7·7	30·083	...	16	11	Dined out.	
	17.	...	...	...	477	19·83	57	1·54	...	...	43·3	7·7	30·036		18	4		
	18.	...	...	...	425	17·89	67·5	2·84	...	...	44·2	6·8	29·822					
	19.	...	...	...	423	17·5	35·2	1·46	...	...	39·3	10·5	30·119	...	17	12	Purged.	
	20.	...	...						...	...	39·2	6·4	30·278	...			At Manchester.	
	21.	...	...						...	...	38·6	4·3	30·196					
Sunday.	22.	...	...						...	...	39·2	2·7	30·147					
	23.	...	...						...	...	41·7	7·5	30·202	...			Very full.	
	24.	...	...						...	...	38·4	10·5	30·152	...			Colocynth; purged; very full.	
	25.	...	...	...	423	17·13	29·3	1·19	...	...	36·3	10·1	30·057	...	18	15	Very full.	
	26.	...	...	...	419	16·56	46·2	1·94	...	...	31·8	15·5	30·127	...	19	2		
	27.	...	...	...	390	18·22	59·6	2·48	...	...	32·2	15·6	30·400	...	18	8	Better.	
	28.	...	...	...	474	19·58	38·7	1·6	...	...	36·7	6·5	30·369		19	0		
Sunday.	29.	...	...	...	424	17·66	48·7	2·03	...	...	29·6	10	30·291		19	2		
	30.	...	...	...	440	18·27	69·8	2·9	...	...	29·9	12·6	30·283	...	17	14	Very cold.	
	31.	...	...	...	299	12·64	23·6	1	...	...	36·6	5·6	30·256		18	4		
1862. Jan.	1.	...	...	...	639	26·68	66·5	2·78	...	...	33·3	7·4	30·245		18	13		
	2.	...	...	...	442	18·38	49·7	2·07	...	...	37·1	7·2	30·249		18	10		
	3.	...	...	...	422	17·53	49·9	2·07	...	...	35·4	8	29·857		17	12		
	4.	...	...	...	443	18·46	40·7	1·7	...	...	36·5	9·5	29·702	...	18	12	Black puddings at supper.	
Sunday.	5.	...	...	...	443	18·5	42·5	1·78	...	...	39	12·5	29·659		19	6		
	6.	...	...	...	524	21·75	60·2	2·5	...	...	33	7·8	29·967	...	18	12	Frost.	
	7.	...	...	...	455	18·7	62·4	2·6	...	...	38·9	12·5	29·912		16	10		

TABLE I. (continued).

Date.	Ingesta.		Egesta.					Daily number of observations and analyses.		Greenwich.			Weight −13 stones (182 lbs.).		Weather, state of health, and other observations.
				Urea.		Urine.				Temperature.		Barom. daily average.			
	Solids.	Fluids.	Fæces.	Total daily.	Hourly.	Total daily.	Hourly.	Urea.	Urine.	Daily average.	Daily range.		Night.	Morning following.	
	oz.	fluid oz.	oz.	grs.	grs.	fluid oz.	fluid oz.			°F	°F	inches.	lbs. oz.	lbs. oz.	
1862. Jan. 8.	...	...	...	364	17·5	26	1·3	...	...	44·1	13·5	29·563	...	18 4	
9.	...	...	...	484	19·87	29·2	1·2	...	...	49·7	7·8	29·540	...		No breakfast; at the Crystal Palace; too little fluid; very full.
10.	...	...	...	556	23	48	2·08	...	...	47·8	5·5	29·630	...	17 0	Purged; drank 2½ pints of fluid without food.
11.	...	...	...	422	18·2	47·5	2·04	...	...	47·7	8·6	29·309	...	17 11	Wet; warm.
Sunday. 12.	...	...	...	401	16·6	39	1·62	...	...	43·6	11·1	29·515		18 11	
13.	...	...	...	457	18·9	50·7	2·1	...	...	39·2	10·7	29·581		17 15	
14.	...	...	...			50·9	2·28	...	...	40·2	7·1	29·497		17 1	
15.	...	...	...	390	16·2	36·1	1·5	...	...	35·9	7·5	29·843		18 3	
16.	...	...	...	417	17·2	60·4	2·5	...	...	39·1	13·0	29·935	...	1 4	Colder; frosty.
17.	...	...	...	386	16·09	43·9	1·78	...	...	27	8·0	29·925	...	17 12	} Very cold; frost.
18.	...	...	...	503	20·7	50·8	2·1	...	...	26·6	10·0	29·936	...	17 11	
Sunday. 19.	...	...	...	430	17·44	40·9	1·7	...	...	25·3	8	29·735		18 7	
20.	...	...	...	432	17·9	59·4	2·47	...	...	28	4·1	29·510		16 15	
21.	...	...	...	400	16·7	32·3	1·34	...	...	30·5	5·1	29·383	...	17 14	Long day's work; no dinner; snow; warmer.
22.	...	...	...	443	18·5	31	1·3	...	...	40·9	16·8	29·358	...	19 4	Thaw.
23.	...	...	...	485	20·2	50·4	2·09	...	...	39·6	7·5	29·434	...	19 6	Evening party; very full.
24.	...	...	...	490	17·4	55·9	2·32	...	...	48·0	10·8	29·350		18 12	
25.	...	...	...	450	18·7	49	2·04	...	...	40·7	10·7	29·627		19 2	
Sunday. 26.	...	...	...	399	16·8	47·5	2	...	...	38·1	14·5	30·132	...		Colocynth; purged.
27.	...	...	...	418	15·45	30·5	1·13	...	...	40·6	11	30·038	...	17 0	Very little food.
28.	...	...	...	472	14·35	37·8	1·15	...	...	42·7	17	29·679		18 4	
29.	...	...	...	382	16	43	1·8	...	...	43·5	9·9	29·582	...	18 6	Sore throat; not well.
30.	...	...	...	375	15·64	38·4	1·6	...	...	48	6·7	29·553	...	16 14	Better.
31.	...	...	...	485	20·12	35·2	1·48	...	...	52·1	7·3	29·628	...	18 8	Well.
															} Fever with sore throat prevailing.
Feb. 1.	...	...	...	528	21·8	62	2·58	...	...	50·9	4·5	29·796	...	17 15	Soirée.
Sunday. 2.	...	...	...	492	20·45	47·3	1·97	...	...	49	5·7	29·977		18 14	
3.	...	...	...	423	17·6	42·7	1·78	...	...	50·3	7·3	30·031	...	17 4	Very tired.
4.	...	...	...	406	17	29	1·21	...	...	51·2	8·4	30·056	...	19 0	All the patients worse with debility, diarrhœa, and perspiration.
5.	...	...	...	511	20·7	74	3	...	...	49	11	29·881	...	18 0	Cooler.
6.	...	...	...	475	19·44	58·6	2·4	...	...	43·4	13·8	29·827	...	17 6	All the patients better.
7.	...	...	...	477	19·85	56	2·33	...	...	32·4	11	30·090	...	16 15	Cooler; milk supper.
8.	...	...	...	484	20·2	78·3	3·27	...	...	29·1	10	30·406		16 1	
Sunday. 9.	...	...	...	568	23·64	40·1	1·67	...	...	32·3	14	30·425		18 14	
10.	...	...	...	490	20·36	52·7	2·19	...	...	34·4	15	30·318	...	18 6	Much fat food at night.
11.	...	...	...	453	18·86	57·6	2·4	...	...	34·6	15·2	30·079	...	19 1	Evening party; very full.
12.	...	...	...	427	18·6	73·4	3·07	...	...	40·3	7·2	29·898			
13.	...	...	...	365	15·1	31·9	1·32	...	...	36·6	4	29·938		18 2	
14.	...	...	...	520	21·6	54·8	2·28	...	...	36·6	5·9	29·954		18 3	
15.	...	...	...	523	21·7	51	2·12	...	...	38·5	9·2	29·985		18 1	
Sunday. 16.	...	...	...	507	21·8	49·4	2·13	...	...	36·6	11·2	29·313		18 10	
17.	...	...	...	397	16·2	27·1	1·11	...	...	39·1	13·1	29·200	...		
18.	...	...	...	449	18·6	33·4	1·39	...	...	47·4	15·8	29·399			
19.	...	...	...	...	...	...	...	...	...	49·6	11·5	29·399		18 4	

	20.	...	...	...	397	16·2	27·1	1·11	...	...	50	12·1	29·502	...	18	12	Very full; warm.
	21.	...	...	...	448·7	18·6	33·4	1·39	...	...	47·5	18·4	29·718	...	19	11	
	22.	...	...	...	485	20·1	50·5	2·1	...	...	47·4	15·3	29·627	...	19	5	Fog.
Sunday.	23.	...	...	...	478·1	19·9	53·9	2·24	...	...	45·2	14	29·986	...	19	12	
	24.	...	...	...	...	...	...	...	...	...	38·3	2·4	29·959	...	...		Colocynth; purged.
	25.	...	...	...	483·3	20	44·7	1·86	...	...	33·1	5·6	30·018	...	17	1	
	26.	...	...	...	397·2	16·46	35·45	1·47	...	...	35·6	3·3	30·185	...	18	9	
	27.	...	...	...	428	17·9	69·3	2·9	...	...	34·2	4·4	30·051	...	16	8	Late supper; much fat; very cold; east wind.
	28.	...	...	...	418	17·3	37·66	1·56	...	...	35·9	7·4	29·883	...	18	4	
March	1.	...	...	...	413·7	17·16	48·2	2	...	...	36·6	11·8	29·806	...	17	4	
	2.	...	...	...	477·6	19·95	45·5	1·9	...	...	31·1	11	29·354	...	18	4	
Sunday.	3.	...	...	...	431·8	18	58·6	2·44	...	...	30·8	13	29·134	...	17	4	
	4.	...	...	...	431	17·88	39·3	1·63	...	...	29·9	18·1	29·555	...	...		
	5.	...	...	...	466	19·3	42·4	1·76	...	...	35·5	19·5	29·654	...	...		Wind changed; supped out.
	6.	...	...	...	559	23·2	45·9	1·91	...	...	48·3	16	29·386	...	18	12	
	7.	...	...	...	544	22·57	40·4	1·68	...	...	51·9	10·4	29·333	...	19	14	
	8.	...	...	...	590	24·57	65·8	2·73	...	...	53·3	12·7	29·420	...	18	2	
	9.	...	...	...	503	20·9	39	1·62	...	...	47·2	8·1	29·374	...	19	7	Catarrh.
Sunday.	10.	...	...	...	457	19·11	56·5	2·36	...	...	46·4	11	29·825	...	18	3	
	11.	...	...	...	456	,19	29·5	1·23	...	...	46·1	10	29·641	...	18	14	
	12.	...	...	...	503	21	47·8	2	...	...	46·5	12·4	29·561	...	18	10	
	13.	...	...	...	477	20	69·4	2·9	...	...	44·9	12·6	29·842	...	...		Black dose.
	14.	...	...	...	430	17·8	31·6	1·31	...	...	41·8	6·2	30·009	...	19	0	
	15.	...	...	...	533	22·2	60	2·5	...	...	41·5	9·3	29·964	...	19	6	
	16.	...	...	...	443	18·5	62·5	2·62	...	...	41·5	5·9	29·767	...	19	8	
Sunday.	17.	...	...	...	443	18·44	65·3	2·72	...	...	41·7	7·9	29·639	...	17	13	

1. *Urea.*

Absolute quantity evolved.—The daily average excretion of urea, obtained from inquiries usually of a few days' duration, on persons living in their usual manner, has been most variously stated by the best observers. BECQUEREL ascertained it to be 286·1 grs.; PARKES, 371·5 grs.; BÖCKER, 444·9 grs.; BISCHOFF, 541·9 grs.; J. LEHMANN, 551·8 grs.; MOSLER, 558·9 grs.; HAMMOND, 670·6 grs., and GENTH, 688·4 grs.; but the average of all the results collected by Dr. PARKES gives 512·4 grs. per day, or 21 grs. per hour, a quantity which singularly corresponds with the amount obtained by myself on the average of all the months of the year. These inquiries were not only made upon different persons, but upon those living in different climates and at different seasons of the year; and only a very few of them were of such duration as to afford any strong evidence of the very great variations in the excretion of urea which occur in all men during lengthened periods. The amount of urea excreted to each lb. of body-weight was ascertained to be $3\frac{1}{2}$ grs. by LEHMANN and VOGEL; and Dr. PARKES ascertained, from an analysis of the returns of nine men, that it was 3·36 grs.

The average quantity of urea evolved daily in my experiments during the twelve months, from March 18, 1860, to March 19, 1861, was 519 grs., being in the proportion of 2·73 grs. to each pound avoirdupois of weight of body. The extreme quantities which were recorded are very diverse, viz. 298 grs. and 748·5 grs. daily; but the occasions on which any approach to such extreme amounts was observed were very few, as is shown in the following Table. [From March 18, 1861,

to March 18, 1862, the daily quantity was 480 grs.; so that the average daily quantity on the two years was 500 grs.]

TABLE II.—Showing the proportionate number of days on which various quantities of urea were evolved during the year.

Grains per day.	Per cent. of the whole.
200 to 300	·038
300 to 400	3·87
400 to 500	46
500 to 600	39
600 to 700	9·1
700 to 800	2·1

Thus the daily emission of urea was between 400 and 500 grs. in nearly half of the whole period of inquiry, and in only 15 per cent. of the whole were the quantities below 400 and above 600 grs.

The average weekly amount of urea was in no instance below 400 grs., and in only one instance did it exceed 700 grains per day. The quantity in this larger average was between 400 and 500 grs. per day in 49 per cent., and between 500 and 600 grs. per day in 38·3 per cent. of the whole number of weeks.

The monthly averages varied from 451 to 665 grs.; but in 47·1 per cent. the quantity was between 400 and 500 grs., and in 38·4 per cent. between 500 and 600 grs. daily.

Hence the most frequent amount of urea evolved was between 400 and 500 grs. daily, and the usual range extended from 400 to 600 grs.; but as only a few experiments were made at the period of the year when the daily evolution was commonly over 600 grains, it is probable that the true frequency of the higher rates is somewhat understated.

The following Table exhibits the average quantity of the urea, urine, and fæces which were emitted daily, and the amount of the fluid and solid ingesta, on the average of the weeks and months of the year. The thick line introduced into this and some other Tables implies that the part following has been added since the reading of the paper.

TABLE III.—Showing the daily amounts of the Ingesta and certain Egesta, with the Temperature and Barometric indications, in weekly and monthly averages.

Weekly.

Date.	No. of days.	Fluids.	Solids.	Fæces.	Urine.	Urea.	Means, Greenwich. Temperature. Daily.	Temperature. Range.	Barom.	Weight, naked, without urine, A.M., in lbs. and oz. — 182 lbs.
1860.		fluid oz.	oz.	oz.	fluid oz.	grs.	°	°	inches.	lbs. oz.
March 18 to 24.	6	52·33	44½	5·16	52·77	487	43·1		29·557	
31.	7	52·18	40¾	4·73	49·56	438	45·3		29·378	
April 7.	7	46·87	41½	4·15	46·87	447	45·7		29·740	
14.	7	53·7	40	6·37	56·15	488	40·7		29·805	
21.	7	57·25	39¾	4·27	60·17	495	42·3		29·926	
28.	7	51·12	38	3·13	54·5	428	41·2		29·855	
May 5.	7	55·65	36	8·76	42·2	574	50·7		30·090	
12.	7	54·16	36¾	3	47·16	571	52·2		29·639	
19.	7	51·7	38	5·7	43·96	517	54·4		29·600	
26.	7	52·6	39	3·44	48·8	524	59		29·850	
June 2.	4	60·5	35½	5·14	64·8	604	51·3		29·578	
9.	7	61·1	36¾	3·97	58·6	533	51·9		29·618	
16.	7	56·7	35½	3·7	56·5	560	53·9		29·533	
23.	7	59	37½	4	41·44	487·6	56·6		29·647	
30.	7	56·5	32¾	7·8	43·8	532	57·1		29·767	
July 7.	5	56·5	39	4	47·3	514	58·5		30·126	
14.	7	59·05	37¾	4·6	56·6	495	57·4		29·870	
21.	7	55·5	35½	5·2	49·4	487	59		29·682	
28.	7	56·4	35	4·6	58·3	506	55·6		29·685	
30.	2	...	...	...	54·8	524				
Aug. 10 and 11.	2	...	...	...	68	674				
18.	6	...	...	...	62·8	635	57·8		29·477	
25.	6	...	...	...	49·7	630	57·3		29·638	
September 1.	4	...	...	...	50·7	614				
5.	2	...	...	...	78·8	715				
October 6 to 13.	3	...	...	...	56	485	45·8		29·750	
20.	6	...	35¾	...	52·8	515	50		29·612	
27.	7	...	...	4·72	59·3	554	53·2		29·911	
Nov. 11 to 17.	6	...	...	...	52	451	41·7		29·416	
24.	7	...	...	...	51·3	459	39·7		29·659	
December 1.	6	...	...	...	49·8	444	41·6		29·474	
8.	7	...	...	8·4	49·9	438	46		29·152	
15.	7	...	...	5·7	52·4	481	40		29·691	
22.	7	...	...	3·6	53·7	451	32·1		29·585	
29.	3	...	...	10·9	48·4	444	25·9		29·540	
1861. Jan. 5.	7	...	...	4·57	54·8	498	32·4		29·714	
12.	7	...	...	6·1	47·8	459	26·4		30·085	
19.	7	...	...	6·14	41·2	466	30·5		29·936	
26.	7	...	33¾	5·46	47·6	475	41		30·089	
February 2.	7	52·4	40½	6·22	48·6	520	42·3		30·126	
9.	7	50·6	34¾	8·45	57·3	524	42·9		29·593	
16.	7	57·4	37½	3·6	54·6	516	37·6		29·697	
23.	7	58·5	40¾	4·23	57·6	507	45·9		29·461	
March 2.	7	62·8	38¾	5·43	56·4	454	43·4		29·800	
9.	7	56·7	36	...	48·3	485	43·9		30·241	
16.	7	63·7	...	8·5	56·9	476	40·8		29·811	

Date.	No. of days.	Fluids.	Solids.	Fæces.	Urine.	Urea.	Means, Greenwich. Temperature. Daily.	Temperature. Range.	Barom.	Weight, naked, without urine, A.M., in lbs. and oz. —182 lbs.
1861.		fluid oz.	oz.	oz.	fluid oz.	grs.	°	°	inches.	lbs. oz.
March 23 to 30.	7				51·93	466·2	46·2	16·2	29·526.	12 9½
April 6.	7				52·9	494·4	43·5	18·1	29·678	12 6½
13.	7				49·1	465	44·2	20·8	29·259	12 0½
20.	7				48·8	488·6	45·4	15·4	30·128	12 11½
27.	7				54·1	462·7	44·3	19·9	29·840	13 9½
May 13 to 18.	6				51·5	496	51·9	23·1	30·049	14 8½
25.	6				41·4	553·5	57·5	24·3	30·009	15 6½
June 1.	7				45·4	457·4	57·5	18·5	29·809	15 14
8.	7				51·32	483·6	53·3	13·6	29·822	16 14½
15.	7				47·66	543	60·5	21·4	29·859	16 15¾
22.	6				37·6	480·6	62·9	23·8	29·832	17 3
29.	6				47·53	479·8	60·1	20·3	29·609	17 6½
July 6.	7				42·9	451·2	59·7	19·4	29·614	16 12½
13.	7				42	577·1	61·5	21	29·598	13 5½
20.	7				38·7	484·1	61·3	17	29·574	13 14
27.	4				50	532·7	61·1	16·1	29·571	14 4¼
August 3.	7				50·6	493	61	21·5	29·780	14 1
10.	4				42·1	591·5	64·4	21·2	29·800	
17.	7				50·2	694·7	65·3	21·2	29·762	
24.	7				56·2	726·7	61·2	20·8	29·943	
31.	4				50·2	707·5	62·5	24·2	29·995	
Sept. 14 to 21.	7				53·4	473	54·4	20·5	29·820	17 15
28.	5				60·5	498	53·8	15·5	29·459	17 11¼
October 5.	3				46·6	519	59·3	13·5	29·816	17 10
12.	6				55	499	58·3	17·5	29·753	17 5¼
19.	3				53·5	508·4	54·6	18·3	29·952	18 14
27.	7				51·2	530·7	54·1	13·5	29·826	18 2½
November 2.	7				56·5	503·7	45·5	13·9	29·644	18 1
9.	7				51·4	436·2	42	14·6	29·460	18 5½
16.	7				50·1	474·1	39·7	13	29·356	17 15½
23.	7				47·7	441	37·5	11·7	29·842	18 3½
30.	7				42·7	442·5	44·5	13·7	29·696	17
December 7.	7				60·1	487·5	40·5	14·1	29·786	18 1
14.	7				49	403·1	48·1	8·7	29·685	17 9½
21.	5				51	450	42·6	7·1	30·087	17 5½
28.	4				43·4	426·3	36·6	9·8	30·208	18 14½
1862. Jan. 4.	7				49·8	444	34·1	8·6	30·126	18 6½
11.	7				45·5	463·8	42·9	9·7	29·654	17 15
18.	6				47·3	425·5	35·0	9·6	29·747	17 15
25.	7				47·1	435·4	36·1	9	29·484	18 14½
February 1.	7				42	437	45·8	10·1	29·772	17 13
8.	7				55·1	466·7	43·2	9·5	30·047	17 10½
15.	7				51·6	477·8	36·2	10·1	30·085	18 7
22.	5				40·1	459·4	45·4	13·8	29·507	18 14¾
March 1.	6				48·2	436·4	37·4	7·0	29·984	17 14½
8.	7				48·3	500	40·1	14·4	29·404	18 7½
15.	7				47·6	480	44·9	9·9	29·745	18 13
18.	3									

Monthly.

Date.	No. of days.	Fluids.	Solids.	Fæces.	Urine.	Urea.	Means, Greenwich. Temperature. Daily.	Temperature. Range.	Barom.	Weight.
1860.		fluid oz.	oz.	oz.	fluid oz.	grs.	°	°	inches.	lbs. oz.
March	13	52·25	40½	4·99	51·16	462	44·2		29·467	
April	28	54·02	40	4·49	54·42	489	42·9		29·796	
May	28	53·52	37½	5·22	45·53	547	53·8		29·746	
June	32	58·76	35½	4·92	53·03	541	54·8		29·613	
July	28	56·86	37	4·6	55·3	505	57·6		29·845	
August	14	.....	...	...	60·1	646	57·7		29·556	
September	6	.....	...	...	64·6	665	56		30·023	
October	16	.....	35¾	...	56	518	50		29·791	
November	13	.....	...	...	51·7	455	40·35		29·537	
December	30	.....	...	7·15	50·6	451·6	36·3		29·491	
1861.										
January	28	.....	33½	5·56	47·87	475	32·7		29·706	
February	28	54·7	38	5·62	52·9	515	41·2		29·959	
March	21	61·1	37	...	53·8	471·9	42·7		29·984	

1861.		fluid oz.	oz.	oz.	fluid oz.	grs.	°	°	inches.	lbs. oz.
March	7				51·93	466·2	46·2		29·526	12 9½
April	28				51·2	477·5	44·3		29·955	12 10½
May	18				46·1	502·3	53·4		29·955	15 4½
June	26				45·9	496·5	59·2		29·780	17 2
July	32				44·8	507·4	60·7		29·623	14 7
August	22				49·6	680·3	63·3		29·875	
September	12				57	485·5	54·1		29·639	17 13
October	26				52·5	512·2	54·3		29·789	18 0
November	28				48	448·4	40·9		29·588	17 3½
December	23				50·8	441·7	41·9		29·941	17 15½
1862.										
January	34				46·1	441·1	38·8		29·796	18 3½
February	25				48·7	460·1	40·5		29·905	18 3½
March.....	17				48	490	42·5		29·574	18 10

2. *Urinary Water.*

Absolute amount evolved.—The observers to whose names I have referred in reference to urea (p. 765), state the average daily elimination of urine to be 44 oz., 40 oz., 81 oz., 54 oz., 67 oz., 65 oz., 39½ oz., and 47 oz., in their order; so that there is much diversity in the amount recorded, and also in the relation between the amount of urea and urine in their several inquiries. PROUT recorded the amount as only 35 oz.; but the average of all the recorded observations collected by Dr. PARKES is 52½ oz. per day, which is almost identical with that evolved by myself on the average of the whole year.

There was very great diversity in the daily amount of urinary water excreted by myself, so that the extremes recorded were 23·5 and 92·67 fluid ounces in the twenty-four hours. On the average of the whole year, from March 1860 to March 1861, the daily quantity was 53·1 fluid ounces; but it exceeded 60 oz. on the average of two of the months, and fell below 40 oz. on the average of two other months. [From March 1861 to March 1862 the average was 49·2 fl. oz.; so that the average of the two years was 51·2 fl. oz.]

The following Table shows how frequently the decades of ounces of urine were emitted throughout the year.

TABLE IV.

20 to 30	fluid ounces in	3·8	per cent. of	the	whole observations.
30 to 40	"	14·4	"	"	"
40 to 50	"	26·2	"	"	"
50 to 60	"	26·5	"	"	"
60 to 70	"	22·4	"	"	"
70 to 80	"	4·8	"	"	"
80 to 90	"	1·3	"	"	"
90 to 100	"	·34	"	"	"

There was not, therefore, any decade which was recorded so frequently as half of the whole number of observations; but the two medium decades of 40 to 60 oz. somewhat exceeded half of the whole. The larger quantity of 60 to 70 oz. was present in no fewer than one-fifth of the whole, and the extremes of below 30 and above 70 oz. were found in 10 per cent. of the whole.

There was a close proximity in time in the emission of the extreme quantities; so that an unusually large quantity immediately succeeded to or was immediately succeeded by a small one, and *vice versâ*, as is illustrated by the following observations:—

1860. Mar. 19.	70	ounces were	followed	by	50 and 41	ounces on succeeding days.
26.	63	"	"		49 and 28	" "
April 14.	77	"	"		42	" "
19.	88	"	"		42	" "
25.	76	"	"		47, 41, and 29	" "
30.	64	"	"		35 and 25	" "
June 11.	69	"	"		35	" "
26.	67	"	"		32, 44, 27, and 27	" "
Dec. 20.	75	"	"		30	" "
1861. Jan. 28.	70	"	"		43 and 39	" "
Feb. 2.	60	"	preceded	by	23	" "
21.	82	"	followed	by	32	" "
Mar. 9.	61	"	"		36	" "
14.	92	"	"		26	" "
[Apr. 15.	74	"	"		33	" "
May 20.	64	"	"		27	" "
June 4.	81	"	"		38	" "
Nov. 18.	76	"	"		30	" "
Dec. 1.	75	"	preceded	by	42	" "
12.	63	"	"		31	" "
30.	69	"	followed	by	23	" "
1862. Jan. 7.	62	"	"		29	" "
Feb. 8.	78	"	"		40	" "
12.	73	"	"		31	" "]

In numerous instances there was a marked alternation in the quantities evolved on succeeding days, as was noticed by Dr. PARKES, and is well shown in the following instances:—

	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	
1860. Jan. 10 to 15.	57	37	62	44	60	46			} Plate XXXII.
1861. Dec. 3 to 8.	44	67	49	55	39	61			} fig. 1.
[May 18 to 21.	61	41	64	27					
Nov. 5 to 12.	51	36	67	35	64	48	66	49	
Dec. 5 to 10.	64	40	71	44	66	33]			

A wave extending over a period of several days was of common occurrence. The following are examples:—

		oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	
1860.	March 19 to 26.	73	50	41	41	57	52	54	63	} Plate XXXII. fig. 2.
	April 25 to 30.	76	47	41	29	42	64			
	Apr. 30 to May 6.	64	35	25	37	39	50	52		
	May 21 to 25.	60	43	36	49	61				
	June 18 to 21.	59	31	25	34	59				
	Aug. 10 to 14.	59	76	77	56	59				
	16 to 21.	72	65	41	30	45	67			
[1861.	April 16 to 20.	33	53	62	46	29				
	May 13 to 18.	57	47	30	55	57	61			
	20 to 25.	64	27	20	32	47	55			
	29 to June 3.	52	48	24	41	55	81			
	June 14 to 19.	30	36	42	68	34	28			
	Oct. 5 to 9.	42	52	76	49	47]				

In a few instances there was a progressive decline through many days. Thus—

	oz.	oz.	oz.	oz.	oz.	oz.	oz.	
1860. Jan. 7 to 13.	68	49	49	40	42	37	26	

On several occasions there was a progressive and marked increase. Thus—

	oz.	oz.	oz.	oz.	oz.	
1860. Feb. 19 to 21.	35	55	87			
Oct. 15 to 27.	50	67	79			
Mar. 10 to 14.	31	60	59	69	92	
July 4 to 8.	34	38	60	62	74	

In a few instances the quantity remained very high for some days. Thus—

	oz.	oz.	oz.
1860. May 31 to June 2.	84	65	72

Owing to the complexity of the attendant circumstances, it is not possible to assign the precise cause to each variation; but the relation of temperature and barometric indications will be considered further on.

[*Relation of Urinary Water to Weight of Body.*

A reference to Plate XXXVI. will show that there is a very marked and an inverse relation between the variations in the quantity of urine evolved and those of the weight of the body on the same day. There is not an exact correspondence between the two, since the weight of the body must be influenced by many other circumstances than that of the loss of urine; but a selection of the instances in which there was a large and sudden variation in the quantity of urine will suffice to prove the close relationship referred to, and shown in the following Table.

TABLE V.

Date.	Increase of urine.	Decrease of weight.	Date.	Decrease of urine.	Increase of weight.
	fl. oz.	oz.		fl. oz.	oz.
1861. April 1.	16	12½	1861. April 23.	35	14½
15.	30	28½	26.	38	19
22.	32	28	May 31.	24	15
June 3.	26	23	June 3.	43	10
17.	26	23	28.	23	6½
July 6 to 8.	28	30	Oct. 5.	18	20
26 to 28.	37	21	8.	27	12
Sept. 15 to 17.	32	15	Nov. 19.	46	13
October 3.	23	25	26.	22	21
6.	24	28	Dec. 31.	46	6
21.	27	27	1862. Jan. 8.	33	23
November 18.	30	33	21.	27	15
December 1.	33	14	Feb. 2.	15	15
12.	32	13	4.	13	28
1862. February 5.	45	16	9.	38	45]
8.	22	14			

3. *Relation of the quantities of Urea and Urinary Water.*

The relation of urea to urinary water may be ascertained by arranging the daily emissions of urine in decades of fluid-ounces, and determining the quantity of urea which was contained in each decade. The following Table is constructed in this manner; and, in addition, the whole year is divided into monthly periods.

TABLE VI.—Showing the amount of Urea, ascending with each decade of ounces of urine emitted daily, during every month of the year.

Urea emitted daily (omitting decimals).

Urine.	1860.										1861.		
	Mar.	April.	May.	June.	July.	Aug.	Sept.	October.	Nov.	Dec.	January.	Feb.	March.
fluid oz.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.
90 to 100	...	.....	.....	.....	.....	...	.....	.....	.....	.....	.....	.....	460
80 to 90	...	534	655	.....	.....	...	693	.....	.....	.....	.....	543	
70 to 80	452	509 535	.....	642	518	575 614 725	739	603 620		464 504	586		
60 to 70	375 471	389 513 419 518 460 483	520 531 594	489 520 497 541 507 583 519 601	465 509 473 509 482 524 487 537	617 643 748	633	529 608 540 621 546 556	489 557	455 489 461 530 463 473	408 470 418 492 427 485	418 507 445 528 483 547 488 584	432 473 538 574
50 to 60	384 480 499 502 580	355 473 511 503 538	509 646 560 574 576 595	487 555 498 563 517 580 547 590 553 633	420 515 463 520 466 534 482 548 499 617	604 621 623 732	.....	461 468 523	428 487 437 548 446 478 482	420 445 453 506 561	461 559 609	474 561 505 517 548 593	441 455 456 473 631
40 to 50	404 431 488 499	376 544 410 565 419 589 429 604 463 713 484	486 595 504 599 516 532 553 562	416 674	456 464 464 500 502 505	534 591 579 714	.....	434 462 581	403 413 424 433 485	298 528 380 539 425 554 433 449 526	413 495 429 495 436 498 444 510 451 552 452 649 480	360 532 439 477 486 489 508 526	470
30 to 40	421	511	485 528 517	446 551 512	.....	...	.....	.....	.....	.....	467	472	411
30 to 40	...	449	432 539 454 545 473 592 505	404 547 475 596 522 528	510 526 535	602 659	.....	350 520	418 447	364 476 398 401 436	408 492 441 500 472 503 487	427 462 480 552	436 451 467 496

There is so great a variation in the amounts of urea recorded, in every part of the Table, that at first sight it appears to be impossible to eliminate any general law. In every decade of ounces of urine the variation in the amount of urea, in even the same month, extends to from 50 to 200 grains; and this is equally true whether we regard the medium or the extreme quantities. When, however, we make an average of the whole observations in each month and under each decade, we find that they arrange themselves on a general plan. The following Table contains the average of all the observations made during each month, arranged in the order of the ascending decades of ounces of urine.

TABLE VII.—Showing the average relation of the amounts of Urea and Urinary Water at each month of the year, and in every decade of ounces of urine (omitting decimals).

	1860.										1861.		
	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.
fluid oz.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.
90 to 100	...	...	...	...	...	...	...	...	...	...	...	...	460
80 to 90	...	534	655	...	...	...	693	...	...	...	...	543	
70 to 80	452	522	...	642	518	638	739	611	...	484	586	...	
60 to 70	423	465	546	532	473	669	633	566	473	468	450	500	504
50 to 60	489	476	576	552	505	645	...	463	472	496	543	533	491
40 to 50	455	545	543	545	479	...	...	469	431	463	525	489	470
30 to 40	...	449	505	512	527	635	...	485	432	415	471	480	460
20 to 30	421	511	510	503	...	...	...	...	...	...	467	472	411

Omitting any reference to the unique instance of the emission of upwards of 90 fluid ounces of urine in one day, we find that the following unbroken order occurs as we ascend in each decade, commencing with the second. Thus—

TABLE VIII. (Plate XXXII. fig. 3.)

Urine (fluid oz.)...	20 to 30	30 to 40	40 to 50	50 to 60	60 to 70	70 to 80	80 to 90
Urea (grs.).....	470	490	492	520	531	577	606

The decade from 50 to 60 fluid ounces represents the average quantity of urea and urinary water evolved daily throughout the year, whilst the superior decades represent the larger, and the inferior the lesser quantities of urea.

But although the lines of urinary water and urea run parallel to each other, there is not a precise correspondence in the daily excretion of these two substances, as the following Table shows:—

TABLE IX.—Showing the weight of Urea in each fluid ounce of Urine, arranged in the ascending order of the middle of each decade of ounces of the latter (Plate XXXII. fig. 4).

Urinary water (fluid oz.)...	25	35	45	55	65	75	85
Urea (grs.)	18·8	14	10·9	9·4	8·1	7·7	7·1

There was thus a progressive decrease in the amount of urea found in each ounce of urine as the quantity of the latter evolved daily increased; but the quantity of urea in the lowest was proportionately greater than in the higher decades, and the decrease was more rapid in the ascent from the two lowest decades. If the decade between 50 and 60 fluid ounces be accepted as the central point of this variation, it will be observed that the degree of divergence differs greatly at the two extremes, although equidistant; so that it is the greatest in the upper and the least in the lower decades. In reference to the total daily quantity of urea evolved, the difference was an increase of 16½ per cent. with the highest, and a decrease of 9·6 per cent. with the lowest decades; but in reference to the quantity of urea in each ounce, the increase with the lowest decade was 100 per cent., whilst the decrease with the highest decade was only 24 per cent.

Thus with increase of urinary water there was a greater increase in the urea than there was a decrease of urea with decrease of urinary water, and the least difference in the amount of urea in each ounce of urine. The average proportion of urea and urinary water throughout the year was 9·77 grains of the former in each fluid ounce of the latter; and assuming the average specific gravity of the urine to be 1020, the proportion of urea to urinary water was 2·19 per cent.

4. *Period of the day.*

Parkes states that the quantities of the ingredients of the urine are the highest in the afternoon and evening, from the effect of the dinner and the exertion of the day. Draper found the quantity of urea to have lessened in the night, but the urinary water was equal in the day and night. Kaupp found that when there was a large excretion in one period there was a small one in the other; and Parkes noticed a certain oscillation extending to two or three days. Kaupp also ascertained that there was a diminution in the excretion of urine, urea, and chloride of sodium during the night. In none of these inquiries were the hours of the day and night defined, neither were the conditions as to the ingesta and the hours of the meals alike.

In stating the results of my own inquiries, I shall arrange them under two heads, viz. with ordinary food, and with fasting or variation from ordinary food.

A. *With ordinary food.*

This part of the inquiry has been investigated by several series of experiments. The amount of urea and urinary water evolved per hour was determined thrice a day throughout nearly the whole year, viz. the average excretion of the whole day, the average of the night, and the quantity secreted in the morning, after the night urine had been voided, and before any fluid or solid food had been taken. The two former embraced lengthened periods, whilst the latter extended to from twenty minutes to one hour only. The object in determining the latter quantity was to ascertain if, at the period of the day when the system was the least influenced by food, there was so uniform a production of urea as to constitute a standard with which to compare the quantities produced

at other periods, and to determine the influence of meteorological phenomena over the daily production of this excretion. At that period, moreover, the only disturbing cause in operation was the slight exertion required whilst dressing. This was called the "basis quantity," and corresponds to a similar inquiry recorded in my paper on the evolution of carbonic acid, published in the Philosophical Transactions, 1859. But beyond these three periods of inquiry there was commonly a fourth, viz. that which intervened between the emission of the basis quantity and midday; and further, an analysis of the urine was made at frequent intervals in the afternoon during the months of January, February, and March 1860.

Another series of inquiries was expressly instituted to determine the hourly emission with the utmost exactitude; and on three days the urine was passed at every hour, and on two other days at every quarter of an hour during the day.

The food was taken at the hours indicated in the earlier part of this paper. I shall now proceed to describe the result of the first series of inquiries.

Relations of Urea.

Plate XXXIII. exhibits the hourly quantities of urea and urinary water at the four periods just mentioned, viz. the whole day, the night, the basis quantity, and the period until midday, on each day of the inquiry throughout the year, and the following Table contains the weekly and monthly averages of the daily quantities.

TABLE X.—Showing the quantity of Urea and Urinary Water excreted per hour, at different periods of the day, on the average of the weeks and months of the year.

Weekly Averages.

Date.	Urea.				Greenwich Means.		Urine.			
	24 hours.	Night.	Basis.	To midday.	Temp.	Barom.	24 hours.	Night.	Basis.	To midday.
1860.	grs.	grs.	grs.	grs.	°	in.	fluid oz.	fluid oz.	fluid oz.	fluid oz.
March 18 to 24.	20·3	17·6	21·36		43·1	29·557	2·2	1·13	1·81	
31.	18·2	14	18·31		45·3	29·378	2·06	1·07	1·79	
April 7.	18·5	13·1			45·7	29·740	1·95	1·14		
14.	20·3	16·5	19·01	25·4	40·7	29·805	2·34	1·4	1·98	5·79
21.	20·6	16·7	22	27	42·3	29·926	2·67	1·24	2·24	6·8
28.	22	17·3	20·14	27·72	41·2	29·855	2·27	1·36	1·93	6·1
May 5.	23·9	20·2	18·4	23·8	50·7	30·090	1·75	1·15	1·15	1·98
12.	23·7	21·2	20·3	27·89	52·2	29·639	1·97	1·33	1·36	3·44
19.	21·5	20·2	19·9	24·81	54·4	29·600	1·83	1·22	1·49	2·55
26.	21·8	18·9	20·7	21·9	59	29·850	2·03	1·53	1·79	2·1
June 2.	25·1	19·5	30·5	27·13	51·3	29·578	2·71	2·2	3·36	6·7
9.	22·2	20	22·7		51·9	29·618	2·44	1·6	2·3	
16.	23·3	19	22·9		53·9	29·533	2·35	1·44	2·37	
23.	20·3	17·1	15·9		56·6	29·647	1·71	1·4	1·87	
30.	22·1	15·5	21		57·1	29·767	1·82	1·3	2·2	
July 7.	21·4		18·5		58·5	30·126	1·97		1·45	
14.	20·6	20·2	17·3		57·4	29·870	2·35		1·79	
21.	20·2	17·8	18		59	29·682	2·05		1·87	
28.	21	18·2	18		55·6	29·685	2·43		1·96	
30.	21·8		23				2·28		2·74	
August 10 & 11.	28·1						2·83			
18.	26·4				57·8	29·477	2·61			
25.	26·1				57·3	29·636	2·07			
September 1.	25·5						2·11			
5.	29						3·28			
October 6 to 13.	20·2		20·9		45·8	29·750	2·33		2·2	
20.	21·8		21·7		50	29·612	2·2		2·2	
27.	23	15·4	20·3		53·2	29·911	2·47	0·99	1·95	
November 17.	23	12·07	17·6		41·7	29·416	2·16		2·28	
24.	23·3	13·7	19·6		39·7	29·659	2·14	0·93	1·98	
December 1.	17	17·2	21·6		41·6	29·474	2·07	1·38	2	
8.	18·2	15	21·9		46	29·152	2·07	1·38	2·11	
15.	20	15·2	17·9		40	29·691	2·18	1·02	1·72	
22.	16·1	13·6	18·3		32·1	29·585	2·1	0·92	1·45	
29.	18·3	17·5	25·97		25·9	29·540	2·01	1·1	2·18	
1861. Jan. 5.	20·7	16·5	21·8		32·4	29·714	2·28	0·92	3·08	
12.	18·5	15·4	21	24·13	26·4	30·085	1·99	0·97	2·09	3·9
19.	19·4	15·6	16·42		30·5	29·936	1·71	1·09	1·02	
26.	19·8	16·7	19·15	21·8	41	30·089	1·98	0·99	1·74	4·35
February 2.	21·6	16·7	22·04	27·15	42·3	30·126	1·85	1·2	2·69	4·5
9.	21·4	17·2	19·7	27·5	42·9	29·593	2·14	1·06	2·22	6·43
16.	21·5	17·6	19·5		37·6	29·697	2·27	1·43	2·06	
23.	20·8	16·65	22·64	29·43	45·9	29·461	2·4	1·43		4·85
March 2.	17·1	16·4	20·4		43·4	29·800	2·35	1·33	2·41	
9.	20·2	16·9	19		43·9	30·241	2·01	1·8	2·02	
16.	19·8	16·03	22·7		40·8	29·811	2·37	1·29	2·76	

TABLE X. (continued.)

Monthly Averages.

	Urea.				Greenwich Means.		Urine.			
	24 hours.	Night.	Basis.	To midday.	Temp.	Barom.	24 hours.	Night.	Basis.	To midday.
	grs.	grs.	grs.	grs.	°	in.	fluid oz.	fluid oz.	fluid oz.	fluid oz.
1860, March.....	19·25	15·8	20·8		44·2	29·467	2·17	1·1	1·83	
April	20·3	15·9	20·38	26·7	42·9	29·796	2·27	1·28	2·03	6·23
May	22·7	20	19·8	24·6	53·8	29·746	1·89	1·41	1·45	2·5
June	22·5	18·2	22·6		54·8	29·613	2·21	1·59	2·46	
July	21	18·7	18·9		57·6	29·845	2·3		1·96	
August	26·8				57·7	29·556	2·5			
September	27·2				56	30·023	2·7			
October ...	21·6	15·4	20·9		50	29·791	2·33	·99		
November	23·1	12·9	18·6		40·35	29·537	2·15	·93	2·11	
December	17·6	15·7	21·13		36·3	29·491	2·18	1·16	1·89	
1861, January ...	19·6	16	19·6	23	32·7	29·706	1·99	·99	1·97	4·12
February...	21·3	16·5	20·97	28	41·2	29·959	2·2	1·28	2·32	5·29
March	19	16·4	20·7		42·7	29·984	2·24	1·7	2·39	

Both the Plate and the Table prove that the least hourly elimination of urea occurred in the night, and that the "basis quantity" was higher than that of the night. The average of the whole day was greater still, and the highest elimination took place in the morning hours, up to, or a little beyond midday. On the average of the whole yearly returns combined, the difference in the amount eliminated at the different periods of the day is sufficiently great; for if the average excretion of the whole day be accepted as the standard, that of the night was 24, and the "basis quantity" was 6·4 per cent. less, whilst the increase to midday was 17·5 per cent. greater than that standard. The following are the average quantities excreted at these periods:—

	Whole day.	Night.	Basal quantity.	To midday.
	grs.	grs.	grs.	grs.
Urea	21·7	16·5	20·3	25·5

The relative diminution of the excretion in the night is greater than this mode of comparison indicates; for the standard of the day has been lowered by adding the diminished quantity of the night, in order to obtain the average of the whole day. Nothing, however, would be gained by separating the day from the night hours, so as to use the rate of excretion in the former as a standard of comparison with the latter, since no two observers agree as to the hours which constitute the day and the night respectively. The Table also imperfectly exhibits the amount of excretion before midday, since that part of the investigation was not pursued at the period of the year when the largest elimination of urea was proceeding; but the second series of inquiries remedy this defect.

The *monthly averages* in Table X. vary the order above given in but one instance, when the basis quantity was ·2 gr. per hour less than that of the night. There is also an unimportant exception in the *weekly averages*, in reference to the rate of the night excretion, in which the night exceeded the day excretion by ·2 gr. per hour. The former occurred in May, and the latter in December.

There are, however, no fewer than seventeen exceptions in reference to the relative position of the basis quantities and the average of the day. In several instances the excess of the basis quantity was very small, but during the occurrence of the frost in December the basis exceeded the day average, on four out of five weeks, by 4·6, 3·7, 2·1, and 7·6 grs. per hour. In five weekly averages the basal rate fell below the night rate.

Plate XXXIII. shows that, in reference to the relative position of the night rate of excretion, on only thirteen occasions throughout the whole year did it exceed the day rate, and then only to the small extent stated in the following Table:—

TABLE XI.—Showing the exceptional instances in which the night rate exceeded the day rate of excretion of urea, and the amount in excess.

1860.	Grains per hour.		Grains per hour.
April 10.	·4	June 22.	·45
12.	·83	23.	·15
16.	·18	25.	·15
May 11.	3·85	January 25.	1·17
16.	1·3	December 4.	·17
18.	·5	8.	1·74
24.	·6		

The most marked exception occurred on May 11, in which the excess was 20 per cent. of the day rate, and was occasioned by an unusually large supper, which was eaten on that night. The same kind of explanation is also applicable to the exception on April 12; but in several other instances, as on December 4 and 8, the exception was due to an unusually low day rate of excretion of urea and urine. In a future part of this paper I shall consider more fully the influence of food in varying the day and night rate of elimination of urea.

The investigation of the true value of the basis quantities is beset with much difficulty; for whilst on a large average they are less than the day rate and greater than the night rate, there are not fewer than seventy-two exceptions to the former and twenty-four to the latter law; and whilst many of them are insignificant, there are others in which the variation is very considerable. I think it of great physiological importance to determine if these quantities will at all represent the average of the day, or be any fixed standard with which to compare other returns of a varying nature; and with a view to the more complete investigation of this period, I have abstracted and tabulated these exceptions, and have placed by their side the actual quantities of urine emitted on the same and on the previous day. It is very probable that, if any relation exists between the urea in the basis quantity and the total quantity of urine of the day, it will have reference to the day preceding the "basis quantity."

TABLE XII.—Showing the *exceptions* in the "basis quantities" as compared with the day and the night rate of elimination of urea, and the quantity of urine emitted on the same and on the preceding day.

I. Basis quantity in excess of the Day Rate.

Date.	Day.	Urine.		Urea variation.	
		Day before.	Same day.	Same day.	Day before.
1860.		fl. oz.	fl. oz.	grs.	grs.
March 19.	Monday	58	50	5·4	
21.	Wednesday	50	41	·2	1
22.	Thursday.....	41	41	2·2	1·8
26.	Monday	54	63	·45	
27.	Tuesday	63	49	2·65	1
29.	Thursday.....	28	40	5·3	4·7
April 1.	Sunday	50	60	·98	
2.	Monday	60	50	1·1	1·1
3.	.....	...	...	...	1·5
11.	Wednesday	46	61	1·76	1·2
12.	Thursday.....	61	46	·1	
13.	Friday	46	57	2·5	6·3
15.	Sunday	77	42	1·9	
16.	Monday	42	61	11·11	8·6
17.	.....	...	...	...	2·2
18.	Wednesday	64	64	3·7	4
20.	Friday	88	42	4·7	2·6
21.	.....	...	...	...	·5
23.	Monday	54	67	3·7	5·6
24.	Tuesday	67	64	3·9	2·4
May 3.	Thursday.....	25	37	1·3	·3
10.	Thursday.....	48	57	1·5	1·5
12.	.....	...	...	...	4·3
20.	.....	...	...	...	2
21.	Monday	55	60	3·0	·7
31.	Thursday.....				
June 1.	Friday	72	72	9·5	
6.	Wednesday	68	57	2·5	4·4
7.	Thursday.....	58	58	6·14	5
8.	Friday	63	63	6·3	4·3
9.	.....	...	...	...	·9
10.	Sunday	67	65	4·9	8·3
11.	Monday	55	55	6·4	·1
22.	Friday	59	68	·39	
23.	Saturday	57	57	2·1	1·4
26.	.....	...	...	·45	·32
July 16.	Monday	56	51	2·9	
17.	Tuesday	51	54	2·67	3·5
21.	Saturday	47	50	·42	
23.	Monday	52	68	1·8	1·4
26.	.....	...	...	...	·12
29.	.....	...	...	...	·3
30.	Monday	54	55	3·3	
October 9.	Tuesday	65	69	1·6	
15.	Monday	...	61	2·33	
16.	.....	...	...	...	3·2
19.	Friday	41	30	4·7	
20.	.....	...	...	...	5·9
23.	Tuesday	61	50	9·2	5·5
24.	.....	...	...	...	1·1

TABLE XII. (continued.)

Date.	Day.	Urine.		Urea variation.	
		Day before.	Same day.	Same day.	Day before.
1860.		fl. oz.	fl. oz.	grs.	grs.
November 12.	Monday	...	58	10·6	
13.	.....	...	...	...	·6
15.	Thursday.....	36	60	·75	·7
16.	Friday	60	56	·3	2·8
17.	Saturday	56	44	·27	
18.	Sunday	44	41	·29	6·8
20.	Tuesday	49	55	9·2	11·5
22.	.....	...	...	...	·1
27.	.....	...	...	...	1·0
29.	Thursday.....	47	49	4·7	
December 1.	Saturday	35	49	·7	4·6
3.	Monday	45	54	·53	2·6
4.	Tuesday	54	37	·6	·7
9.	Sunday	46	38	·19	1
11.	Tuesday	60	57	·7	
13.	.....	...	...	...	3·4
19.	Wednesday	65	30	2	
20.	Thursday.....	30	75	·45	4·2
23.	Sunday	40	40	4·6	7·8
24.	Monday	40	59	4·2	3·3
31.	Monday	30	70	4·8	
1861. Jan. 3.	.....	...	...	...	1·1
7.	Monday	47	68	1·2	·6
10.	Thursday.....	49	40	8·5	10·2
11.	Friday	40	42	2·1	2·7
25.	.....	...	...	...	3·3
27.	Sunday	44	49	4·6	5·1
29.	Tuesday	70	43	5	
February 4.	Sunday	57	64	1·5	1·5
12.	Monday	67	58	·63	
20.	Tuesday	35	55	4·8	5·3
21.	.....	...	...	...	·7
22.	.....	...	...	...	·2
24.	Sunday	51	44	1·7	
25.	Monday	44	65	8·4	8·6
26.	Tuesday	65	62	2·6	·8
27.	Wednesday	62	40	5·38	2·5
March 3.	Sunday	63	44	1·6	
5.	Tuesday	55	49	1·1	
6.	.....	...	...	...	2·2
10.	Sunday	...	31	·4	
11.	Monday	31	60	2·9	2·0
12.	Tuesday	60	59	4·4	3·2
13.	.....	...	...	...	1·6
14.	Thursday	69	92	6·9	2·2

TABLE XII. (continued.)

II. Basis quantity less than the Night Rate of Urea.

Date	Day.	Urine.		Urea variation.	
		Day before.	Same day.	Night following.	Night before.
1860.		fl. oz.	fl. oz.	grs.	grs.
March 25.	.....	...	...	...	·47
28.	Wednesday	49	28	3·9	
April 3.	Tuesday	50	45	·11	
12.	Thursday.....	61	46	·73	
May 4.	Friday	37	39	3·5	5·0
5.	Saturday	39	50	4·2	1·7
9.	Wednesday	40	48	5·5	·2
11.	Friday	57	49	7·8	5·5
12.	Saturday	49	44	1·2	
13.	Sunday	44	60	·68	5·5
14.	Monday	60	55	·58	3·3
16.	Wednesday	43	53	4·6	·1
17.	.....	...	...	...	1·5
18.	Friday	25	45	2·2	
22.	.....	...	...	...	3
24.	Thursday.....	36	54	6·1	
June 3.	Sunday	72	54	3·6	2·4
5.	Tuesday	69	35	5·6	1·5
12.	.....	...	...	...	4·0
20.	Wednesday	25	34	3·56	3·0
21.	Thursday.....	34	59	2·5	2·9
22.	.....	...	...	...	·5
July 9.	.....	...	...	...	4·4
28.	Saturday	60	57	·4	
November 23.	Friday	62	50	·5	
December 12.	Wednesday	57	53	·9	
1861. Jan. 25.	Friday	33	51	1·9	
February 3.	Sunday	60	57	4·0	
10.	Sunday	40	44	3·14	
March 2.	Saturday	55	63	9·9	5·0

On comparing the exceptions with the daily excretion of urine, it is seen that they are almost equally numerous when compared with the urine of the same or the previous day; so that it might be inferred that there is no essential connexion with either; but on regarding the total average of the quantity of urine emitted on the day before these exceptional occurrences, it will be observed that the quantity is so high as 67·3 oz., and that the list embraces not fewer than twenty-three occasions on which the quantity exceeded 60 ounces daily. Thus there is a connexion established between unusually large excretion of urinary water and unusually high basis quantity of urea emitted on the following morning, and this corresponds with observations recorded when making the

analyses throughout the year. It should also be noticed, in reference to the amount of urine emitted on the same day, that whilst on the average it was scarcely higher than the average of the year, the list comprehends twenty-one occasions on which the quantity exceeded 60 ounces.

On turning to the exceptions in the relation of the basis quantity of urea with the night urine, it is seen that the defect in the quantity of urine emitted, both on the preceding and on the same day on which the defect in the basal quantity of urea occurred, is quite as marked as is the excess of urine in relation to excess of the basal quantity of urea; for the average was below that of the whole year, and the list comprehends a large proportion of the days of unusually small emission of urine.

Hence it is shown that, although the "basis quantity" of urea is on the whole average greater than the night and less than the day quantities, it has a marked tendency to become in excess whenever the quantity of urine is increased, and to be in defect when the quantity of urine is diminished. After a laborious analysis of the returns, and a careful consideration of the subject, I fear that there is not any period of the day at which a standard excretion of urea may be established for short periods, and that the determination of this basis quantity will aid but little as a measure of the vital actions proceeding in the body.

The *monthly* and *weekly* averages of the rate of excretion of urea before midday, with but one exception, conformed to the rule established by the yearly average. The excess in this quantity over the daily average varied in different weeks from 0 to 8·6 grs. per hour. In the instance which forms the exception, this morning rate was equal to that of the whole day, and was caused by a reduction of the rate on one day from an attack of diarrhœa, which occurred in the morning hours. There were also three exceptions in the weekly averages; but two of these involved only the second place in decimals, and the last was deficient to the extent of 2 grs. per hour. With all these exceptional conditions, there was associated a remarkable diminution in the quantity of urine voided during the morning hours.

The relative position of the excretion of urea in the morning hours, in reference to the other periods now referred to, is thus well established.

I determined the rate of the excretion of urea during the afternoon at intervals of two or three hours in the month of February 1860, and have stated the results in a few cases in Table XVI. There are variations on the different days; but they do not affect the general direction of the curves, so that all agree in showing that there is a large and rapid increase in the elimination of urea in the morning hours, followed by a gradual decline as the afternoon advances. The hour of maximum elimination was commonly from midday to 2 P.M.; but sometimes it was so early as 10 A.M., and so late as 4 P.M. There was frequently seen an attempt at increase in the middle of the afternoon, and this will be further elucidated when we describe the results of the next series of inquiries; but in the instance of February 17 this tendency was so exaggerated as to cause the maximum elimination to occur at that period, a circumstance which was due

to the ingestion of an unusually small quantity of fluid at the breakfast, and to my having taken 18 oz. of strong beef-tea without solid food at 1¾ P.M. The first cause lessened the morning excretion, whilst the second increased the emission at the hours indicated; and on both of these grounds, and from my being unwell in the afternoon, the day was exceptional.

I now proceed to describe the results of the observations made at each hour of the day, and at every quarter of an hour during limited periods.

Most observers have found that the emission of urinary water is increased nearly one-third in the second or third hours after a meal; but others, as CHAMBERT, found no increase, and BENEKE met with great diversity in his results.

All persons have, however, noticed an increase in the elimination of urea after a meal; but there is a diversity of statement as to the commencement and the duration of the increase. LEHMANN and others found it to commence during the first hour, and to attain its maximum in the third hour; but VOIT, in his experiments upon dogs, sometimes observed the maximum so late as the seventh, and traces of the increase remained throughout nearly a whole day.

On March 1 and 2, 1861, I made hourly observations from 8 or 9 A.M. until midnight. The only variation from the ordinary food was, that the dinner on March 2 was unusually large, and black pudding and coffee were taken so late as 10½ P.M. The following Table contains the results of the inquiries (Plate XXXIV. fig. 1).

TABLE XIII.—Showing the Hourly Excretion of Urea and Urinary Water, and the periods of Meals.

1861.	A.M.				P.M.												
	Breakfast.				Dinner.			Tea.		Supper.							
	9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	12	Night.
	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.
Urea, Mar. 1...	...	23	17·7	20·3	39·7	23	15·4	16·8	13·8	20·4	22·8	32·7	35	16·7	19·1	30·6	15·68
Urea, Mar. 2...	10·6	20·6	31·4	25·8	28	26	24·3	17·8	...	20·7	17·6	25	28·4	29·2	23·9	20·2	19·5
	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.
Water, Mar. 1	...	4·5	3·94	3·2	9	5·12	1·8	1·6	1·28	1·97	1·55	2	2·76	1·82	1·6	1·47	1
Water, Mar. 2	1·03	2·05	7·5	4·1	11·1	5·05	3·35	1·75	...	1·45	1·15	1·95	2·6	2·87	2·23	1·33	1·44

On December 17 and 18, 1860, I determined the rate of excretion at each quarter of an hour, from 9½ A.M. to 1.45 P.M. on the former, and from 1 P.M. to 3½ P.M. on the latter day. I did not venture to pass urine so frequently as each quarter of an hour throughout the whole day without intermission, on account of the influence which I believe it to have of increasing the secretion of urine and of causing an unusual state of excitement at the neck of the bladder. Breakfast was taken on the first day from 9.20 to 9.37 A.M., and dinner commenced at 1.52 P.M.; whilst on the second day coffee, with bread and butter, was taken from 1.20 to 1.30 P.M. The following Table exhibits the results of these inquiries.

TABLE XIV.—Showing the Hourly Rate of Elimination of Urea and Urinary Water at each quarter of an hour.

1860.	A.M.							P.M.													
	10¼	10½	11	11¼	11½	11¾	12	12¼	12½	12¾	1	1¼	1½	1¾	2	2¼	2½	2¾	3	3¼	3½
December 17. Urea	grs. 29·6	grs. 34·8	grs. 37·1	grs. 36·6	grs. 27·8	grs. 35·2	grs. 33·6	grs. 36	grs. 40	grs. 54·6	grs. 26	grs. 24	grs. 34	grs. 25							
Water	fl. oz. 7·7	fl. oz. 7·37	fl. oz. 8.34	fl. oz. 8·2	fl. oz. 9·6	fl. oz. 12·8	fl. oz. 11·6	fl. oz. 12	fl. oz. 9·9	fl. oz. 13·5	fl. oz. 6·5	fl. oz. 4·6	fl. oz. 6	fl. oz. 4·6							
December 18. Urea	...	...	...	...	...	...	...	...	...	...	...	grs. 23·7	grs. 29	grs. 25·1	grs. 30·6	grs. 30·5	grs. 34·2	grs. 32·8	grs. 35·2	grs. 28·8	grs. 25
Water	...	...	...	...	...	...	...	...	...	...	...	fl. oz. 4·4	fl. oz. 3·8	fl. oz. 4·5	fl. oz. 4·2	fl. oz. 3·13	fl. oz. 3·37	fl. oz. 3	fl. oz. 3	fl. oz. 2·6	fl. oz. 2·4

The changes described in Tables XIII. and XIV. are also delineated on Plate XXXIV. figs. 1 & 2.

The hourly inquiries show that there was a rapid increase of from 10 grs. to 30 grs. per hour in the elimination of urea during the morning hours, followed by a diminution to 15 or 17 grs. per hour at the middle hours of the day. There was a second elevation to 25 and 29 grs. per hour in the afternoon with its maximum at 9 or 10 P.M.; and after that period the final fall occurred, which reduced the rate to the lowest amount of the twenty-four hours. The former was the greater and the more enduring elevation. The smallest emission during the day occurred after the early dinner hour, and the second elevation followed the tea meal. On March 2, when the greatest evening elevation occurred, there was an unusually large ingestion of food, viz. 40¾ oz. of solid food (including 9 oz. of meat and bacon), and 68 oz. of fluid, and there was a sense of oppression or of excess in the evening. On comparison of these observations with those made at longer intervals, there appears some disagreement, since the latter (Table XVI.) did not usually show any very distinct evening increase; but it may be stated in explanation, that the longer interval between the inquiries prevented the discovery of the full fall after midday, and by taking the average of a part of the morning increase with a part of the subsequent fall, and again an average of a part of the fall with a part of the subsequent increase, the effect of both seemed to be nearly lost.

The more frequent inquiry at each quarter of an hour, shows yet more clearly the rapid changes which occur during the morning hours. Table XIV. shows that a maximum rate of nearly 55 grs. of urea per hour occurred in 3¼ hours after the breakfast, and was followed by a decline to less than half that amount in the following quarter of an hour. The second part of this inquiry shows the comparatively low and uniform ate of emission after the midday hours, and also the commencement of the evening elevation.

Hence it is proved that when meals are taken at the hours adopted by the mass of the people, there is a rapid and maximum excretion of urea in the hours following the breakfast, with a subsidence at about the early dinner hour. There is no increase immediately following the dinner, but after the tea meal and in the early evening hours there is a second increase, which is finally lost in the night hours.

Relations of Urinary Water.

Table XVI. shows that the relations of urinary water to period of the day are very similar to those now described in reference to urea. The average rate at all hours combined was 2·21 fluid ounces, whilst that of the night was reduced to 1·19, and the basis quantity to 2·05, but the quantity evolved to midday was increased to 4·41 fluid ounces. The decrease in the night and at the hour of the basis quantity, as compared to the whole day rate, was 46 and 7·2 per cent., and the increase at midday was 100 per cent.

The night rate varied in the different months from ·93 to 1·59 fluid ounce per hour; but its position in reference to the rates at the other periods of the day was uniformly maintained in every month, and also in every week, during the year, except on two occasions (May and June), when it was equal to the basis quantity. Plate XXXIII. shows that in only six instances throughout the year did the night rate equal or exceed the whole day rate; and the increase was only to the following amount:—

May 20.	May 31.	June 21.	December 6.	December 30.	February 23.
·1 fl. oz.	·43 fl. oz.	·43 fl. oz.	·26 fl. oz.	·07 fl. oz.	·18 fl. oz.

There were, however, seventeen instances, as is shown in the following Table, in which the relative positions of the night and basis rate were reversed, and the former exceeded the latter.

TABLE XV.—Showing the exceptional instances in which the night rate of elimination of urinary water exceeded the "basis quantity," and the amount of increase. The seven instances marked with an * correspond with the exceptional instances in reference to urea in Table XII.

1860.		Fluid ounce.			Fluid ounce.
April	14	·9	June	3*	·34
	21	·24		21*	1·31
	25	·72		23	·1
May	5*	·61	December	16	·12
	9*	·47	February	2	·1
	12*	·75		21	·49
	20	·83		23	·47
	24*	·68	May	2*	·41
	31	·13			

If we omit reference to the variation on June 21, the exceptions are not important; and as they form so small a proportion of the whole, they do not invalidate the general law. Hence it is established that the night rate of the emission of urinary water is less than the whole day rate, and than the "basis quantity."

The "*basis quantity*" exceeded the whole day rate in two months, viz. June and March, to the extent of ·25 and ·12 fluid ounces; and in the various weeks the exceptions to the rule were so numerous as 41 per cent. of the whole, and varied from 0 to ·8 fluid ounce. There were sixty-two exceptions in the various days of the year, varying from

an increase of 0 to 2·45 fluid ounces over the whole day rate of the same day. Of these, forty-seven were the days on which the basis quantity of urea also exceeded the whole day rate, as is observed in Table XII. Many of the exceptions were quite insignificant, so that in ten instances the excess only involved the second place of decimals; but in ten others it exceeded 1 oz. per hour. The large number of instances in which the basis quantity, both of urea and urinary water, was excessive, proves the relationship which exists between them, and their mutual dependence upon a common cause; but in 34 per cent. of the days on which the basis quantity of urea was exceptional, there was no exception in the rate of excretion of urinary water at the same period. Hence the estimate already given of the value of the basis determination of urea, is nearly equally applicable to the excretion of urinary water. Both vary greatly, and chiefly in relation to the amount of urinary water evolved on the preceding day.

The relative position of the rate of urinary excretion before midday was unbroken in every month and week of the year. On three days in the year the rate was less than the whole day rate by the second place in decimals only. There were two exceptions only in reference to the basis quantity, viz. a defect of ·15 and ·57 fluid ounce in May and June. The rate of excretion varied greatly, and on one occasion exceeded an average of 13 fluid ounces per hour.

Table XVI. exhibits the variations of the hourly rate in the excretion of urine during the hours of the afternoon. All the figures show that there was a progressive increase in the quantity through the morning hours, and until two to six o'clock in the evening, and afterwards a progressive decrease until the night period. There were, however, variations in the quantity which are deserving of notice.

TABLE XVI.—Showing the Hourly Rate of Excretion of Urine at various periods of the day, in fluid ounces.

1860.	A.M.				P.M.												
Hours.	9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	12	Night.
	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.		fl. oz.	fl. oz.	fl. oz.	fl. oz.
February 14.	·6	...	...	4·1	...	...	...	6·17	...	6·25	...	...	...	1·85	1·2	...	·97
15.	·9	1·36	...	6·16	...	...	...	...	...	...	1·85	...	...	...	1·68	...	·88
16.	1·3	...	...	3	12·6	...	...	...	...	3·08	...	...	...	...	...	2·48	1·09
17.	2·48	...	...	2·72	...	...	2·66	...	...	...	2·55	...	...	...	1·71	...	1·01
18.	1·8	...	...	4·3	...	...	6·23	...	...	...	1·75	...	...	...	1·62	...	1·14
23.	1·1	·8	...	7·73	...	...	5	...	1·75	...	1·33	...	...	...	...	3·3	·88
24.	1·36	1·3	...	1·24	...	5·1	...	...	3·84	...	...	...	...	...	1·91	...	1·24
25.	1·12	...	6·4	3·9	...	...	...	2·47	...	...	...	...	...	...	1·6	...	·86
27.	2·5	5·76	10·4	11·4	8·4	...	...	...	2·4	...	...	2·01	...	...	...	3·05	1·33

The circumstances especially worthy of note in connexion with this Table are the variations in the maximum rate, the different hours in the middle part of the day when it occurred, the great diversity of elimination in successive hours, and the large rate of excretion at midnight on three occasions, compared with the preceding night rate on the same day, or with hours immediately preceding on other days. February 17 was an exceptional day, since I was not well, and beef-tea only was taken at dinner. On the

occasions on which the midnight rate was great, fluid food was taken between 10 and 11 o'clock, the two former with wine, at evening parties, and the latter with coffee only.

On referring to the hourly excretion of urine, as shown in Table XIII. (Plate XXXIV.), we find that the rate increased after the breakfast until 1 A.M., when a maximum of eleven times the basis quantity was emitted. It then fell, and remained low until 8 P.M., when it rose to a second maximum at 9 or 10 o'clock, and thenceforward fell to the night rate. The morning maximum was nearly four times as great as the evening one. The morning emission did not increase in equal proportions during each hour until the maximum was attained, but, after slowly rising, it suddenly ascended to the maximum in one hour, and as suddenly fell at the next hour. I had frequently noticed this occurrence throughout the year; and it was usually found in about four hours after the breakfast meal. On March 2, 1861, there were two maxima in the morning, separated by a decrease at 12 o'clock, the former being at the rate of 7·5, and the latter of 11·1 fluid ounces per hour.

There was a general correspondence between the rate of emission of urea and urine, but not such a one that the proportionate rate of the two remained the same at each hour, as is shown in the following Table.

TABLE XVII.—Showing the number of grains of Urea in each fluid ounce of urine at each hour of the day, on the average of two days.

A.M.				P.M.											Night.
Breakfast.				Dinner.	Tea.								Supper.		
9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	12
grs. 10	grs. 6	grs. 4·3	grs. 6·3	grs. 2·88	grs. 4·8	grs. 7·8	grs. 10·3	grs. 10·8	grs. 12	grs. 15	grs. 11·7	grs. 10	grs. 10·0	grs. 11·3	grs. 14·2

There was, therefore, the smallest amount of urea in each ounce of urine at the periods when the largest excretion of urine occurred, and *vice versâ*. The contrast between the quantity of urea eliminated per hour, and the quantity contained in each ounce of urine at the various hours of the day, is delineated in Plate XXXIV. fig. 1, and proves that the proportion is the least in the morning hours, advances with the afternoon, and attains its maximum in the night hours. Table XIV. shows that, during the investigation made at every quarter of an hour, the quantity of urine increased from 7·7 fluid ounces per hour at $10\frac{1}{2}$ A.M. to 13·5 fluid ounces per hour at $12\frac{3}{4}$ P.M., and was the greatest at the two periods when the rate of excretion of urea was the greatest, viz. $12\frac{3}{4}$, when 54·6 grs. of urea and 13·5 fluid ounces of water were excreted per hour. There was an increase of urine with the increase of urea per hour, but it was not proportionate to the quantity of urea. In the afternoon the rate slowly fell from 4·4 to 3 fluid ounces per hour at $2\frac{3}{4}$ and 3 P.M., and then rose to 5·4 ounces at the termination of the inquiry.

B. *With variations in the ordinary dietary.*

Fasting.—Mosler, Becher, Böcker, Falck, and Ferber found that when water was administered during fasting for short periods, the evolution of urea and urinary water was considerably increased, but afterwards the former fell below the normal amount. The increase in the elimination of water occurred in the second or third hour, and the increase in the quantity of urinary water was greater than the increase in the quantity of water supplied.

In October 1860 I fasted during 29½ hours from supper-time, and took 30 ounces of water only at about the ordinary meal hours, viz. 8½ A.M., 12½, 5½, and 9 P.M., and determined the hourly excretion of urea and urinary water during the day. The results are given in the following Table, and delineated in Plate XXXIV. fig. 3.

Table XVIII.—Showing the Hourly Rate of Elimination of Urea and Urinary Water during fasting, but with water.

	A.M.					P.M.						
	Water.					Water.					Water.	Water.
	8	9	10	11	12	1	2	3	4	5	8½	2½
Urea ...	grs. 7·79	grs. 20·48	grs. 34·5	grs. 20·8	grs. 17·85	grs. 18·72	grs. 15·6	grs. 14	grs. 13·5	grs. 5·3	grs. 21·6	grs. 7·49
Urine ...	fl. oz. ·812	fl. oz. 3·2	fl. oz. 11·5	fl. oz. 6·94	fl. oz. 7·44	fl. oz. 5·2	fl. oz. 10·4	fl. oz. 2·93	fl. oz. 2·5	fl. oz. 1·36	fl. oz. 2·57	fl. oz. 1·39

The progression in the rate of elimination of urea and water was precisely the same as when ordinary food was taken, and the only evident variation was the rapidity with which the increase and decrease was effected. After the last meal, the night rate of emission of urea was less than 8 grs. per hour; but after taking ten ounces of water it rapidly increased to 34·5 grs. per hour, and subsequently rose after every administration of water, and fell before the period arrived for the subsequent supply. The lowest rate of the day occurred at 5 P.M.; and the two highest followed the breakfast and the tea hours. It is singular to notice that there was very little increase following the administration of water at the dinner-hour; and in this respect also it corresponds with the amount evolved after an ordinary dinner.

The urinary water was also largely increased after every dose of water, so that the maximum rate was fourteen times greater than the basis quantity; and a decrease always preceded the following supply. Hence the simple administration of water at different periods of the day causes a series of defined curves of increase and decrease of the urinary secretion and of urea. The average hourly rate of elimination of urea from 8½ A.M. until 2½ A.M. without any food having been taken was 15·1 grs.

Water only.—On various occasions I drank water or other fluids in the morning, and abstained from food until midday, with a view to determine their influence at the period of the day when the urinary secretion is the most abundant.

The following Table and Plate XXXIV. fig. 6 contains the results of drinking ten

ounces of water at 8½, 9½, and 11 A.M., the effect being determined every quarter of an hour until midday.

TABLE XIX.—Showing the effect of drinking Water alone, upon the Urea and Urinary Water.

	A.M.													P.M.
	Water.				Water.				Water.					
	8½	9	¼	½	¾	10	¼	½	11	¼	½	¾	12	3
	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.
Urea ...	20·7	15·28	30·7	35·14	27·65	29·2	35·22	30·86	24·8	26	29·5	32	29	23
	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.
Urine ...	2·8	1·24	2·86	9·6	21·6	16·6	21·4	22·2	14·6	11·6	15	19·4	17·6	4·13

There was an increase in the quantity of urea and urine evolved throughout the whole inquiry; and after each dose of water there was a progressive increase to the maximum rate in half or three-quarters of an hour, and then a decrease until the next supply of water. The maximum rate after the first and second dose was upwards of 35 grs., and after the third dose it was 32 grs. per hour.

The urinary water increased from the basal rate of 2·8 oz. per hour to the rate of 21·6 oz. per hour in one hour; and although it fell at the period of observation following the second dose of water, it was renewed, and increased to the maximum rate of 22·2 oz. in half an hour after the second dose. It again fell, and subsequently rose after the third dose of water, but the rate was not then so high as at the previous maximum periods. The period of maximum emission of water was a quarter of an hour later than that of urea, except after the third dose, when both coincided. The maximum emission was eight times the basis quantity.

Various fluids only.—Numerous other experiments were made of a similar character but with different fluids, and the examinations were made hourly. These are collected in the following Table.

TABLE XX.—Showing the Hourly Rate of Excretion of Urea and Urinary Water before midday under the influence of various fluids, but (with two exceptions) without food.

Urea.

1860.	January.							February.	
	23.	24.	25.	26.	28.	30.	27.	20.	22.
Hour.	Water 8 oz., at 9 A.M.	Water 8 oz., bread 3 oz., at 9 A.M.	Coffee (½ oz.), 8 oz., at 8½ A.M.	Tea (100 grs.), 8 oz., at 9 A.M.	Black dose 2 oz., water 6 oz., at 8½ A.M.	Water 8 oz., at 9 A.M.	Water 8 oz., at 8½	Water 8 oz., at 8¼, 9¼, 10¼ A.M.	Gluten bread 2 oz., water 8 oz., at 9, 10, 11 A.M.
	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.
8½	.....	.....	12·2	.....	12·4	.....	.....	21	
9	17·66	14·7	25·3	15·2	19·8	13	11·7	.....	18·5
9½	.....	.....	.....	.....	.....	.....	15·8	28·64	
10	30·5	17·6	19·3	24·6	21·8	22·7	.....	.....	19·5
10½	.....	.....	.....	.....	.....	.....	23·7	28·1	
11	24·9	26·6	19·2	24·24	23·7	20·16	.....	.....	25·4
11½	.....	.....	.....	.....	.....	.....	.....	28·8	
12	24	30	15·3	17	15·8	20·4	18·7	28	36·9

Urinary Water.

	fluid oz.	fluid oz.	fluid oz.	fluid oz.	fluid oz.	fluid oz.	fluid oz.	fluid oz.	fluid oz.
8½	.....	.....	·76	.....	·9	.....	.....	2·5	
9	2	1·4	·3	·8	1·6	3·1	·9	.....	1·7
9½	.....	.....	.....	.....	.....	.....	1·9	12·25	
10	9·25	2	2·35	3·5	2·35	5·2	.....	.....	1·9
10½	.....	.....	.....	.....	.....	.....	5·4	21·5	
11	8·3	6·65	1·8	3·6	2·7	11·2	.....	.....	3·1
11½	.....	.....	.....	.....	.....	.....	.....	16·5	
12	8·5	10·13	1·65	2·25	1·8	8·5	3·26	7	12·6

Maximum increase per cent. over the basis quantity.

Urea	76	104	58	59	75	65	102	35	100
Water	341	621	200	350	200	260	500	760	640

The maximum emission of urea in these several experiments varied from 22·7 to 36·9 grs. per hour; but no just comparison can be made, unless we accept the basis quantity as the standard in each experiment. If this be accepted, we find a striking difference in the effects of the several substances.

The greatest increase always occurred when water was the fluid employed. In these experiments with water the increase in the rate of elimination of urea exceeded 100 per cent., whilst in two others with water it was 65 and 76 per cent., and in one was so low as 35 per cent. The effect of tea and coffee was precisely the same, and but little more than half that of water, whilst with black dose there was still an increase of 75 per cent. The addition of a little common bread and of gluten bread did not lessen the effect of the water, but it deferred the period of maximum elimination to the end of the inquiry; and in this respect these experiments stand alone. The largest elimination of urea followed the use of the gluten bread. The maximum effect of coffee is stated in

the Table to be almost immediate; and hence it is possible that it might be due to some unexplained agency, and the subsequent emission should be regarded as the maximum.

The effect upon the urinary water was not precisely parallel to that upon urea; but in the instances in which water caused the largest emission of urea it also caused the largest emission of urinary water. The greatest increase, viz. 760 per cent., occurred in an experiment with water; but in that experiment the basis quantity was unusually high, and the increase in the rate of elimination of urea was proportionately small. The least increase occurred with coffee and black dose; and tea caused a greater increase of urinary water than coffee, although they eliminated urea at precisely the same rate.

All these experiments agree with the foregoing in demonstrating the great activity of the urinary function in the morning hours.

On several occasions I drank 1½ oz. of alcohol with 4½ oz. of water between 8 and 9 A.M., and deferred the breakfast for two hours. The effect in different experiments was to cause within 1½ hour the following increase in the elimination of urea and urinary water.

TABLE XXI.—Showing the early effect of Alcohol and Water without food over the rate of emission of Urea and Urinary Water.

	1860.	May 14.	May 16.	May 19.	May 21.	May 24.
Urea, grs.	Basis	16·3	20·1	21	24·7	17·2
	Maximum	33	27·7	33·6	31·9	24·5
Water, fl. oz.	Basis	1·58	1·57	1·06	2·6	1·15
	Maximum	5·47	9·27	6·92	17	7·3
Increase per cent.						
Urea, grs.		102	38	108	30	43
Urinary water, oz.		246	554	553	553	534

The increase in the rate of elimination of urea varied much, but in two-fifths it was more than equal to a much larger quantity of water taken alone. In one instance, in which the increase was small, the absolute quantity and the basis quantity were both high, whilst in another experiment the absolute quantity was low.

The uniform effect upon the rate of elimination of water is most striking; for, if we except the first experiment, the increase was almost uniformly 550 per cent. This is too remarkable and too often repeated to be merely a coincidence, and it is probable that it may represent the true effect of alcohol when taken in the morning and alone, as above indicated.

Bread and fluids.—Another series of experiments demonstrate the effect of a diet of only bread and water and tea and coffee over the hourly elimination of urea and urinary water. They were made on consecutive days in February 1860, and comprehend one day on bread and water, one on bread and tea, one on bread and coffee, and a fourth

day in which bread and water only were taken until 3 P.M., and then a suitable supply of food. The urine was not emitted at any prescribed hours, but at short intervals. The following Table contains the results of these inquiries.

TABLE XXII.—Showing the Hourly Rate of Excretion of Urea and Urinary Water, with a diet of bread, water, tea or coffee on three days, and additional food on the fourth (Plate XXXIV. fig. 5).

1860.	February 6.		February 7.		February 8.		February 9.	
	Bread 8 oz., water 12 oz., at 8½, 12½, 4½, and 7½, and 2 oz. of bread at midnight.		Bread 8 oz., water 12 oz., tea (black) 75 grs., at 8½, 12½, 3½, 7½ P.M., and 2 oz. of bread at 11.		Bread 8 oz., water 12 oz., coffee ½ oz., at 8½, 12½, 4½, 8½.		Bread 8 oz., water 12 oz., at 8½, 12½; dinner at 3; tea at 6; coffee 8½; supper 11.	
Hour.	Urea.	Urine.	Urea.	Urine.	Urea.	Urine.	Urea.	Urine.
	grs.	oz.	grs.	oz.	grs.	oz.	grs.	oz.
8 to 9	14·5	1·51	10·3	·7	13·5	1	13·6	·92
9 to 10			17	1·33	22·4	1·87	17·9	1·24
10 to 11	27·9	4·35	20·5	1·3	19·8	1·65	25·2	2
11 to 12			21·3	2	25·4	3·37	19·8	1·8
3.30	29·3	6·82	24·6	3·0	22·3	3·3	23·7	3·47
4.30			.....	.....				
6 to 7	.....	.....	21·6	2·25	20·4	3	26·4	1·6
8	17·3	1·97	.....	.....				
10½ to 11	15·6	1·37	19·3	1·51	25·2	2	29·6	4
12								
Night.	14·2	1·03	19·7	1·46	13·59	·98	15·6	1

I attended to my ordinary duties during this inquiry. There was a marked sense of want experienced, and particularly on the first day; but the two following circumstances were the most intolerable, viz. the sensation of cold following the drinking of the cold water, and the absence of sufficient saliva to enable me to masticate the bread without taking water with every mouthful. The first was partially relieved by taking a small quantity of bread at night, the second by drinking warm water; but as the quantity of water was insufficient to enable me to well masticate the bread, I found no relief for the third. The experiment was relinquished on the fourth day, from the sense of depression and the want of courage in looking to the prospective periods of the inquiry; and after its relinquishment I ate heartily and with enjoyment.

On the three first days the hourly progression in the rate of elimination of urea and urine was precisely the same as that which occurs with ordinary food; but the maximum was perhaps depressed a little, on account of the small quantity of fluid which was taken at the breakfast. The maximum hourly increase of urea was 14·8 grs., 14·3 grs., and 11·9 grs. in their order; and when compared with the basis quantity, the increase per cent. was 100, 141, and 88 in their order. The first day succeeded to the Sunday, when there was commonly a small excess of food, and the progress through the day was so regular that the rate of excretion during the night was the same as it had been in the early morning. On the days with tea and coffee added to the dietary, the maximum amount

of urea was not so high as it had been on the previous day, but the excretion was very large relatively at midnight on both days. On the fourth day, before additional food was given, the maximum was equal to that on the preceding day, and occurred so early as 11 A.M.; but after the additional food the elimination of urea again increased, and the maximum of the whole day occurred between 7 and 12 P.M. On this day the maximum increase with the bread and water was 10·1 grs. per hour, or 75 per cent. of the basis quantity; but after the additional food and late in the evening it rose to 16 grs. per hour and 117 per cent. of the basis quantity.

It is to be remarked that, after three days insufficient dietary and the addition of abundant food in the afternoon of the fourth day, the rate of elimination on the following night was scarcely, if at all, increased—a circumstance due doubtless to the fixation of the nitrogen.

The rate of the elimination of urine on the first day followed the usual course in health, and attained a maximum of 6·82 oz. per hour. On the second and third day the rate sensibly declined, and on the day with coffee it was relatively high in the evening. On the fourth day, before additional food had been taken, the rate was the least which had occurred, and was so low at midday as 1·8 oz. per hour.

Hence this series of experiments showed that a full dietary of bread and water, or tea or coffee, does not vary the hourly progression of the urinary secretion from that which is found with mixed food, but it fails to yield comfort to the system, and also that with 48 oz. of fluid per day the rate of urinary secretion fell considerably below that which ordinarily occurs. When water is given with bread alone it should be of the temperature of the body.

5. *Cycle of the Week.*

There are no experiments on record in reference to this subject. Sunday was a day of strong contrast with other days of the week; for I took almost perfect rest on that day, and also ate a little additional solid food, as follows:—

1860.					1861.	
March.	April.	May.	June.	July.	February.	March.
oz.	oz.	oz.	oz.	oz.	oz.	oz.
2·0	6·3	·3	·1	2·7	2·6	1·5

The comparison of the urinary excretion on Sunday and on week days may be made in two ways. As compared with the average returns of all the days of the year, the excretion of urea was on Sundays 533·4 grs. against 519 grs. on all days combined; but as many influential causes of variation occurred during each week which did not occur on Sundays, a more just comparison may be made with the average of those days on which no known cause of variation occurred. I have collected all the unexceptional days, and have taken the average of them in each month, as is shown in the following Table:—

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TABLE XXIII.—Showing the average excretion of Urea on 191 selected days in the year.

	Average all days.	Sunday.	Monday.	Tuesday.	Wednesday.	Thursday.	Friday.	Saturday.
1860.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.
March	462	580·7	462	455	460	404	375	384
April	489	521	467	458	463	...	493	509
May	546·5	589·4	562	534	528	505	473	558
June	541	555·4	487	543	539	527	467	405
July	505	547·7	496	499	505	494	479	497
August	646	628						
September ...	665	739·3						
October	518	621	534	546	533	492	471	592
November ...	455	485	444	415	421	457	513	418
December ...	451·6	471	476	454	432	456	412	444
1861.								
January	475	475	488	433	479	519	510	475
February ...	515·2	492	525	468	470	515	515	499
March	471·9	471·8	473	437	467	560	441	497

Thus the average daily emission of urea on all these selected days combined was 475·5 grs., giving an increase on the Sundays of 57·9 grs. daily. The increase chiefly occurred before January 1861; and during the winter months of 1861 there was but little variation on Sundays and other days. The causes of exceptional conditions were very few in January, February, and March 1861; and nearly all the days in those months have been included in the selection.

The largest daily emissions of urea which occurred during the year fell relatively more frequently on Sundays than on the whole year, to the following extent:—

Daily excretion, 700 to 800 grs.	600 to 700 grs.	500 to 600 grs.
3·4 per cent.	13·1 per cent.	Equal.

I have sought to ascertain if this periodical day of increase, or the causes which occasioned it, were influential in any defined order in the other days of the week; and the following Table shows that there is a progressive decline in the rate of emission of urea as the week advances to the Saturday, on the average of the 191 selected days and the Sundays just referred to.

Table XXIII., and Plate XXXII. fig. 7, show the average amount of urea excreted on each day in the week, and on the average of the year; the daily excretion was as follows to the end of December 1860:—

	Sunday.	Monday.	Tuesday.	Wednesday.	Thursday.	Friday.	Saturday.
Urea, grs. ...	574	491	488	485	476	460	475

The loss from the Monday to the Saturday was 31 grs., and it was greater proportionately on the Thursday and Friday than on the earlier days in the week. There was an increase on the Saturday; and, in explanation, I may state that Friday is my last Hospital day, when I take only coffee and bread and butter for lunch, and dine at a later hour, and on Saturday I have commonly more rest than on other working days.

Hence during a week of regular labour and food, the daily rate of excretion of urea diminishes as the week advances.

I have also found that, on the average, the weight of the body increases greatly on the Sunday, and progressively lessens throughout the week, with greater or less uniformity, when a tolerably uniform amount of exertion is made; but with the ordinary variations of exertion the weight is the least on the Saturday, and the greatest on the Sunday and Monday mornings. The body was weighed naked on retiring to rest, and also on rising, directly after having passed urine, and before defæcation. The hour of weighing was not fixed, but varied at night from 11 on Sundays to 12 or 1 o'clock on other days, and from 8 to 9½ in the morning; and therefore there would be cause for a slight variation in the results. The scales were good ones, of ordinary construction; and I did not attempt to weigh nearer than to half an ounce. The average of the weights at night and on the following morning were entered as the weight for that day.

The following Table XXIV., and Plate XXXIV. fig. 4, represent the weight on each day from February 4 to March 3, 1861.

TABLE XXIV.—Showing the Weight of the Body naked, and after emission of Urine, on each day of the week, diminished by 13 stones (182 lbs.).

1861.	February 4 to 10.	February 10 to 17.	February 17 to 24.	February 24 to Mar. 3.
	lbs. oz.	lbs. oz.	lbs. oz.	lbs. oz.
Sunday	.....	9 14	9 3	11 1½
Monday	9 6½	7 10	9 2	9 13
Tuesday	9 7¼	8 7½	10 7½	9 9
Wednesday.....	9 2	7 12½	9 12½	11 2
Thursday	8 2¼	8 6½	8 2½	10 4¼
Friday.....	8 8	8 8¼	8 14	
Saturday.....	8 8	7 8	8 14¼	9 15

On the average of this period the weight on the Sunday was 1 lb. 12 oz. greater than on the Saturday.

There was considerable variation in the progression from day to day in the different weeks; but there was much uniformity in the general fact of a decrease in weight towards the end of the week, and the increase at the beginning of the week was without exception. During the first week I made considerable physical exertion daily, but in the others the amount varied; and the average weight of the body increased weekly, as follows: 8 lbs. 4 oz., 9 lbs. 3¼ oz., and 10 lbs. 5 oz. + 13 stones. The difference in the average weights on the two first and the two last days of the week was 1 lb. 5¾ oz. in favour of the former; but that in the extremes of single observations was 2 lbs. 3¼ oz.

There was not a very marked variation in the fluid and solid ingesta during the week, except so far as related to the Sunday; but the following Table offers some points of interest.

TABLE XXV.—Showing the Weight, in ounces avoirdupois, of the Fluid and Solid Ingesta on the various days of the week in March, May, June, and July 1860.

Week. 1860.	Sunday.		Monday.		Tuesday.		Wednesday.		Thursday.		Friday.		Saturday.	
	Solids.	Fluids.	Solids.	Fluids.	Solids.	Fluids.	Solids.	Fluids.	Solids.	Fluids.	Solids.	Fluids.	Solids.	Fluids.
	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.	oz.
Feb. to 25.	55½	61	41½	50	37½	53½	44	51	42½	53½		54	44½	53
March 1.	44½	50	40½	44½	44	50½	42	55½	38½	54	39½	56½	36	49½
8.	41½	48	45½	48	39	42	40½	51½						
15.							40½	57	41	49½	38½	51		
22.	44½	50	39½	58	41½	57½	41	65½	39		27	54	44½	39
29.	52	53	36½	61	38½	47	34½	57	28	45½	37	43		
Average ...	45½	50½	40½	52½	40½	49½	39½	57½	36½	49½	35½	51	40	44½
May 6.					35½	58½	35	57½	36½	58	37	49½	36½	55
13.	35½	49	32	58	37	48½	39½	51½	31½	59½	41	49½	37	63
20.	39½	46	35½		41	50			32½	41½	42½	46	36	
27.	37½	49					40	56	37½	54½	41½	46		
Average ...	37½	48	33½		37½	52½	38½	55	34½	53½	40½	47½	36½	59
June 2.											33½	57	30½	54
9.	35½	52½	34½		29½	68½	38	58½	39	71	36½	51	43½	65½
16.	37	55½	40½	68	28½	51½	35½	59½	34½	49½	37½	53		
23.							29½	68½	40½	53	37	57	35	61
30.	35½	53½			37	44								
Average ...	36	53½	37½		31½	54½	34½	62	38	57½	36	56	36½	60
July 8.							38	59	39½	57½	40	58	38	51½
15.	40½	69	29½	59	42½	63	39½	59½	33½	52½	30	60	39	57
22.	49	65	34½	52	37	53	38½	58½	34	56	41½	53½	30½	57
Average ...	41½	67	33	30½	39½	58	38½	56½	35½	55½	37	57	35½	55
October 28.					38½	38½	31½	59½	37½					
Total average...	41½	54	37½	55½	37½	51½	37½	57½	36½	53½	37½	52½	37½	55

The total average of these observations shows that, with the exception of the increase on the Sunday, there was singular uniformity in the weight of the solid ingesta taken on the various days of the week, there being only one day on which the quantity was below 37 oz. When, however, the fluid and solid ingesta are added together, there is more variation, and the two lowest amounts occurred on my two Hospital days, when there was a lunch of coffee and bread and butter, instead of an early dinner, and dinner was eaten at a later hour, and then there was an increase on the following day. The combined quantities of the fluid and solid ingesta on the different days were as follows, in ounces:—

Sunday.	Monday.	Tuesday.	Wednesday.	Thursday.	Friday.	Saturday.
95¾	92½	89¼	95	90	89½	92½

6. *Cycle of the Year.*

There are not any recorded observations upon the influence of season over the urinary excretion except those of VOGEL, who found that the urinary water was increased in the cold season.

It is not possible to isolate the effects of season from other influences, as, for example,

by adopting a standard dietary for the whole period, for then the effect of season upon the requirement for food could not be determined; and hence the only practicable inquiry was to allow such food as the system required, and ascertain the absolute amount of urinary excretion during the whole period.

The year 1860 was remarkable for the coldness of its summer, and for the long-continued frost during the winter, so that scarcely any opportunities were afforded of determining the effect of great heat over the elimination of the urinary secretion. The general effect of season was, however, well established by the results which were obtained, and it was shown that the urinary products increased in quantity as the summer advanced, and decreased in the winter season.

Urea.

Table III. shows that on the monthly averages the daily quantity of urea progressively increased from 462 grs. in March, through 489, 546, 541, and 505, to the maximum of 646 and 665 grs. per day in August and September, and thenceforward fell through 518, 455, 451·6, 475, and 515 to 417 grs. in the last month of the inquiry. The progression in the rate of increase was well sustained through the spring and summer months, whilst the fall was rapid in autumn; and the rate continued much below the average of the year throughout the winter. The months of distinct increase were April, May, June, and July; the maximum months were August and September; the month of decrease was October, and the stationary minimum months were all those at the end of autumn and during the winter. [The monthly averages in 1861 and 1862 differed somewhat from the above in actual quantities, but followed almost precisely the same course. Those recorded in September were much lower than those of the corresponding month of 1860, because I was not then staying at the sea-side.]

Thus the year may be conveniently divided into two seasons, one extending from May to October inclusive, and the other from November to April inclusive,—the former being the season of heat and of maximum production of urea, and the latter of cold and the minimum production of urea. The average elimination of urea in the former period was 570·1 grs., and in the latter 480·5 grs. daily [in 1861 and 1862 the quantities were 530 grs. and 460 grs.], a difference so marked as to show a natural division of the year into the two seasons. In this arrangement the month of April would be that of a change towards increase, and October that towards decrease.

The increase from the daily rate of elimination in March to the maximum quantity was 36 per cent. of the former, and the decrease from the maximum to the rate in the following March was 29 per cent. of the maximum, whilst the extreme difference beween the minimum and maximum quantities on the monthly average was 46·1 per cent. of the former.

Urinary water.

The rate of excretion of urinary water followed the course just described in reference to urea, viz. increasing with the summer and decreasing in the winter, but with less

uniformity than was found in reference to the urea. The quantity eliminated in March 1860 was 51·16 oz. daily, and this progressed through 54·42, 45·53, 53·03, 55·3, and 60·1 to the maximum of 64·6 oz. daily in September, and then fell through 56, 51·7, 50·17 to 47·87 in January, and rose to 52·9 and 53·8 in February and March 1861. The progression was thus broken by the diminished rate in May and January, and the increase began so early as February; but the maximum quantities were found at the period of maximum elimination of urea. On dividing the year into two parts, as above indicated, when speaking of urea, we find that the elimination of urinary water was 55·7 oz. daily in the warm, and 51·9 oz. daily in the cold season. The excess of the maximum over the minimum monthly rate was 40·2 per cent. of the latter. [In 1861 and 1862 the maximum quantity of 57 oz. occurred in the month of September, and in the months of June, July, and August the quantities were less than those recorded in 1860. On the division of the year into two parts the quantity was precisely equal in each, viz. 49·3 fluid ounces.]

Causes of Seasonal variation.

Temperature.—KAUPP, in numerous experiments made at various temperatures, found that the quantity of urinary water, urea, chloride of sodium, and, indeed, both solids and liquids diminished as the temperature increased, and he attributed the diminution of the solid excretions to the lessening of the urinary water. LEHMANN found that moderate cold increased, but great cold decreased the urinary excretion.

I have not made any experiments with artificial temperatures, or for short periods, or with any regulated or standard dietary to be used under varying conditions, but have ascertained the effect of seasonal variations of temperature with the ordinary variations of food. I was not able to determine the amount of food taken in August and September; but, being then at the sea-side, it is more than probable that it was increased. Hence it is highly probable that the observations hitherto recorded and those now to be mentioned have not been made under parallel conditions; and the results may be due to different causes, although associated with temperature.

It is also necessary to bear in mind that the mean external temperature very imperfectly represents the true temperature in which we live during the greater part of the twenty-four hours, and particularly in the winter, and also that other causes exert influences which may correspond with or be opposed to those of temperature. Hence the inquiry is a most difficult one.

The general influence of temperature is evident from the foregoing statement; for the lines of temperature and urea were parallel throughout the year. The average temperatures of the two periods of the year were 55° and 44°, corresponding with 570 grs. and 480 grs. of urea daily, which gives the very uniform proportion of 10·36 grs. and 10·9 grs. of urea to each degree of temperature on the average of the two opposite seasons. Moreover, the month (May) of marked increase in the urea (56·5 grs.) was that of marked increase of temperature (10°·9); whilst that (November) of marked decrease (63 grs.) was also that of great decrease of temperature (9°·65). As the pro-

duction of urea is due to many causes, we may not expect to find precise uniformity in its relations to temperature; and hence we find that, after the temperature had attained its maximum, the urea continued to increase for a short period; but when the proportionate amount of urea to temperature is tested in the two highest and two lowest months respectively, it is found to be almost absolutely the same, viz. 11·5 grs. and 11·8 grs. to each degree.

The foregoing refers to the production of urea, since it comprehends lengthened periods; and the influence of temperature is very clear; but on considering the influence of that agent during short intervals, as in sudden changes of temperature, the acts of production and elimination are mixed together and cannot be easily separated. Hence the effect of the changes of temperature seems to vary at different periods; and this occurs, doubtless, because in some instances the urea is retained longer than in others. I have sought to ascertain the effect of sudden changes, and have found great diversity; but commonly the influence was felt on the urinary water on the first, and on the urea on the second day. The following are instances of rapid changes of temperature associated with the effect upon the urinary secretion (Plate XXXII. fig. 6):—

TABLE XXVI.

1860, November	15.	16.	17.	18.	19.	20.	21.	22.	23.	
Temperature...	45°·8	42°·6	48°·3	36°·6	37°·8	41°·1	41°·7	43°·5	37°·6	
Urea (same day)	grs. 457	grs. 478	grs. 433	grs. 485	grs. 413	grs. 428	grs. 446	grs. 489	grs. 548	403
Urea (next day)	478	433	485	413	428	446	489	548	403	

If the temperature and urea on the same day be compared, it will be found that the action of temperature is inverse, and in that sense the various parts would correspond both in the ascending and the descending series, except at the point at which the change in the direction of the lines occurred, as on November 20, when an increase of 3°·3 of temperature would be accompanied by an increase of 15 grs. of urea and thus break the rule; but if the effect of the temperature be sought for on the day following the variation, the action will be found to be direct and to correspond in every part, as is shown in the Table by the lowest line, in which the quantities of urea have been placed a day earlier than they actually occurred.

The following illustration shows a deviation from the rule, in consequence of a very large emission of urine on the day of a frost, compensating the greatly diminished quantity of the preceding day. The urea varied with the temperature of the previous day. The urine increased as the temperature fell; but when the temperature became stationary, it offered the compensating alternations before referred to.

TABLE XXVII.

1860, Dec. 16 to 21.	Frost.									
Temperature...	38°·4	35°·4	30°·5	30°·1	29°	30°·6	31°	26°	22°·4	Ill.
Urea	grs. 526	grs. 498	grs. 473	grs. 401	grs. 464	grs. 365	grs. 443	grs. 528	grs. 506	grs. 298
Urine.....	oz. 44·7	oz. 65	oz. 65	oz. 31	oz. 75	oz. 30	oz. 41	oz. 40·7	oz. 59	oz. 46

In December and January there were various occasions on which frost and thaw rapidly alternated; and the resulting effect upon the urea was one of confusion, as is seen in the following Table:—

TABLE XXVIII.

1861, Feb. 12 to 16.	Frost.			Thaw.				
Temperature ...	37°	31°·9	30°·2	36°·1	36°·2	44°·5	47°·2	
Urea (grs.) ...	526	584	489	517	481	528	488	
Urine (oz.) ...	44	67	49	55	39	61	64	31

Increase in the urea evolved occurred with increase of urine, and the latter alternated in quantity from the varying influences then existing. At the same time constipation of the bowels occurred, and the system became disordered.

Dec. 30.—With a rapid thaw and an increase of 12°·4 of temperature the urea increased 28·9 grs. on the following day. The urine was so low as 30·3 oz., but rose on the following days to 70·8 oz. and 64 oz.

Jan. 11.—With thaw and increase of 6°·1 of temperature the urea remained stationary and high, but fell on the following days with frost and falling temperature. The quantity of urine was very low, and yet fell from 42·5 oz. to 37·6 oz. on the same day to 26·6 oz. on the following day, and then rose to 54 oz. with decreasing temperature.

On April 24, with 7°·8 increase of temperature, the urea rose greatly on the same and on the following day. The excretion of urine was very high, and it increased to 76·7 oz. on the same, and fell to 45·7 oz. on the following day.

On July 11, with 5°·6 increase of temperature, the urea was lessened on the same and on the following days until the fourth day, when it rose 92 grs. with increasing temperature.

Whilst in a majority of instances the influence of temperature acts on the day following any important change, it sometimes acts on the same day, and sometimes the effect is contrary to the law. The urine is commonly increased with a sudden fall and decreased with a sudden rise of temperature on the same day, and the opposite condition occurs on the following day.

There are great oscillations, due to the necessity of maintaining a certain equilibrium in the fluids of the body; and these are set in motion by any act which materially varies the emission of fluid, and they continue in a degree and mask the influence of agents which in their absence could be traced in the elimination of the urinary products.

Atmospheric pressure.—The relation of the production and elimination of urea with the barometric elevation is a direct one, so that both rise and fall together. Table III. shows this relation in reference to the monthly averages; so that the month of the highest elimination of urea was that of the greatest atmospheric pressure, and that of the least elimination was that of the least pressure. There is also an unbroken parallelism in the lines as they increase from March to May, decrease in June, increase in September, decrease in October, November, and December, and increase in January and February; but there is a want of correspondence in July and August. When the year is divided into the two seasons already mentioned, the relations of the barometric elevation, temperature, urea, and urine are as follows:—

	Barometer. inches.	Temperature. °	Urea. grs.	Urine. oz.
Summer	29·762	55	570	55·7
Winter.....	29·658	44	480	51·9

The barometric relation is not so evident as it would have been had there not been the exceptional conditions in July and August; but it is sufficient to show that it corresponds with all the other subjects which we have discussed, and that in summer we had high atmospheric pressure and temperature, and great elimination of urea and urinary water, with the contrary conditions during the winter.

The relation of sudden changes of the barometric pressure to the elimination of urea is well seen in the following sequence of days (Plate XXXII. fig. 8):—

TABLE XXIX.

December ...	11.	12.	13.	14.	15.	16.	17.	18.	19.
	inches.	inches.	inches.	inches.	inches.	inches.	inches.	inches.	inches.
Barometer	29·528	29·880	29·981	30·120	30·113	29·811	29·513	29·414	29·289
	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.	grs.
Urea	420·3	445·2	530·3	561	554·3	526·5	498·5	473·1	401

The two lines run parallel throughout the whole series. The temperature was nearly stationary until the 17th, when it fell, and the diminution in the excretion of urea became greater.

When the lines of temperature and barometric pressure run parallel, their influence over the elimination of urea is increased; but when they are opposed, their influence is lessened. This is shown in the instance on January 2, when, with great diminution of temperature, there was but slight diminution of urea on the same day, and an actual increase on the following day; for the barometer rose from 29·228 inches on the day before to 29·918 inches on the same day, and to 30·143 inches on the following day.

An instance of the relation of high barometric indication and elimination of urea is given in the following sequence:—

TABLE XXX.

	April 24.	April 25.	April 26.	April 27.	April 28.	April 29.	April 30.	May 1.	May 2.
Barometer (in.)	29·519	29·812	30·026	30·120	30·160	30·204	30·273	30·125	29·951
Urea (grs.)	483·5	535·2	611	565·6	511·9	713·7	684·6	545·7	528·4
Urine (oz.)	64	76	47	41	29	42	64	35	25

The quantity of urea evolved on the 26th and the 29th was somewhat unduly increased by food.

Hence, on summing up the results now obtained, I venture to state that the action, both of temperature and atmospheric pressure, is direct, and that the urea varies as they vary; but the results are oftentimes conflicting. The following are some of the causes of variation from the rule:—

1. When the temperature increases and the pressure decreases, and *vice versâ*.

2. When the same cause acts for a lengthened period, the specific effects will be found only in the early days, since the necessity which exists for the maintenance of an equilibrium in the statics of the body induces oscillations, as the two influences are respectively powerful. Hence high atmospheric pressure will at first cause increased elimination of urinary water; but at length a point is reached below which the fluids of the body cannot be reduced consistently with health, and the quantity of urine falls, as is seen in the last Table.

3. Whenever, and from whatever cause, there is an increased elimination of urine, there will usually be an increase in the urea, notwithstanding the influence of adverse atmospheric influences.

4. The effects of meteorological and other influences are frequently carried on to the following day; so that in such a case there will be a modified influence acting on both days, and the distinction will be less perceptible; but sudden increase of pressure commonly caused an immediate increase in the quantity of urine evolved.

5. The actual temperature in which we live is not that of the external atmosphere, and cannot be rigidly determined.

With sudden increase of temperature and a low barometer, there is a sense of faintness and fullness, and the elimination of fluid and urea is retarded; whilst with increased atmospheric pressure and decreased temperature, the bulk of the body is lessened with the increased emission of fluids, and there is according to the degree a sense of lightness or of oppression.

[*Contrast of the daily inquiries during two consecutive years, viz. from March* 1860 *to March* 1862. (Plate XXXVI.)

The long interval which has elapsed since the preparation of this paper has permitted me to continue the daily inquiries through another year, and has afforded me the singular advantage of being able to compare the daily quantities through the several seasons of two successive years, and I have been permitted to add the figures to the Tables (p. 759 *et seq.*), and to represent the oppositions and agreements in the quantities recorded in Plate XXXVI. I now append a short Summary of the points in which the latter year has agreed or contrasted with the former.

Conditions of the inquiry.—During the last year the health has been as uniformly maintained as before; but there has been somewhat less of bodily exertion, and the weight of the body has increased about ten pounds. The daily inquiries have been made with

somewhat fewer intermissions; and there have been fewer experiments, which might interfere somewhat with the regular nutrition of the system. The summer holiday was again taken, and a month was again spent at the sea-side. The only important condition of contrast refers to the frequency of the analyses of urea; for, whilst in the former year there were always three, and often more analyses daily, with a view to determine the excretion at the different periods of the day, in the latter there was but one daily. Hence in the past year it has not been possible to restrict the day to uniform hours, viz. from 8 A.M. as before, since, without knowing the rate of excretion proceeding in the night, no correct addition or subtraction of quantities could be made for defect or excess of time. The twenty-four hours has therefore terminated at periods from 8 to 9½ A.M., but commonly it ended at about 8½ A.M.; and the addition and subtraction has been made by the average rate of emission of the twenty-four hours. The former was doubtless the more correct method; but the latter, by not reducing the day to a uniform hour, has not introduced any material error. It must also be recorded that, as there are errors of inquiry connected with LIEBIG'S analysis for urea (as indeed with all other inquiries), they have been varied by the reduction of the number of daily analyses. I am not able to state whether this has rendered the total computation of the daily excretion more or less correct, since it is not known in what direction the errors in the analyses lie, or whether the direction is the same with different inquirers; but, considering that it lies in the appreciation of the exact degree of colour in the test solution, and knowing the degree of care which I have taken, I am inclined to believe that the tendency in my hands would be to lessen the quantity as the experiments were fewer.

Meteorological conditions.—The former year was remarkable for its cold summer and the long-continued frost of winter, whilst the latter, without being a hot year, was warmer in both summer and winter. Each year comprehends a part of two cold seasons, and the summer season, since it begins at the middle of March. The average temperature of the latter year was 2°·2 higher than that of the former. With the exception of May and September, February and March, the monthly temperature was higher; and the difference in May was only a defect 0°·4. The increase of temperature was particularly found in June, July, August, and October, in which the average monthly increase was 4°·4, 3°·1, 5°·6, and 4°·3 respectively. The highest mean daily temperature was 9°·8 higher, and the lowest mean daily temperature was 6°·8 higher, in the latter than in the former year on the days of inquiry. The pressure of the atmosphere was also somewhat higher in the latter than the former year, the excess being ·033 inch. The monthly averages on the days of inquiry were higher, except in July, September, October, February, and March; and the increase was particularly found in April, May, August, and December, in the last of which it was ·450 inch. The most marked months of defect were September and March. As we have shown that the opposition in the movements of these two elements of season have an important influence, it may be remarked that the two years corresponded in April and July, but differed in

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September and January, with opposition in the former year, and in July and March in the latter year.

Elimination of Urea.—The total yearly average was less in the latter than in the former year; but the general course throughout the months of the year was the same. The average daily elimination, however, was greater in March (both), July, and August, but particularly in August, whilst the month of the greatest defect was September. In reference to this difference, it must be remarked that the months of greatest divergence were those of difference in the opposition of temperature and atmospheric pressure, and also that the conditions recorded in the month of September were very different in the two years. It has been remarked that in both years August and part of September were spent at Scarborough, and that there was doubtless some increase of nitrogenous food and exertion; and in reference to food, the increase was certainly greater in the latter than in the former year, and may account for the excess of urea in August. In reference to the divergence in September, it is to be remarked that in the former year only six days were included, and those were spent at Scarborough; whilst in the latter year twelve days were referred to, and all of them were spent in London; and hence with difference of conditions there were differences of results. The highest daily elimination of urea occurred in the latter and the least in the former year.

Elimination of Urinary Water.—The quantity of urinary water evolved was less in the latter than in the former year, and the difference occurred in every month of the year except March and May, but chiefly in the summer season. The greatest defect was 10 ounces daily in August. The defect in the winter half of the year was not considerable.

Weight of Body.—The foregoing differences render it very desirable to ascertain in what degree the weight of the body had increased with the diminution of excretion of fluid, and to compare the weight in the two years. The latter information cannot be given, since the inquiries in reference to weight were commenced only at the end of the preceding year; but the former may be abundantly supplied. The general expression is that the weight increased rapidly and greatly from April to October, with a decrease in May and November, and a second increase to the end of the year. The increase was about 10 lbs. from February to October; and thenceforward the monthly average varied in both directions within limits of 12 ounces. Hence, with the conditions of the year, the habits during the year, and the lessened elimination of fluid by the kidneys, there was a marked increase in the weight of the body.

It may be added as a general expression, that whilst the special conditions of each year caused and permitted variations in the vital and physical actions of the body, there was such a general influence exerted that the course of the changes remained the same.]

7. *Relation of Urea to exertion.*

The effect of exertion has, until very recently, been determined only in an indefinite

manner and during short periods. Draper, Speck, and J. Lehmann found no increase in the urea, whilst C. J. Lehmann, Hammond, Benecke, and Beigel found a variable increase not exceeding 25 per cent. When much sweating occurred, as it commonly did with severe exertion, it was assumed that urea had been lost by the perspiration, in accordance with the results obtained by Funke and Meissner. The excretion of urinary water varied in the different experiments. Voit has, I believe, recently ascertained that with prolonged exertion a dog did not emit any materially increased quantity of urea—a result which will now be shown to correspond with my own preceding and contemporaneous experiments.

The relation of urea to exertion was determined by the second series of inquiries, viz. those made on four prisoners from March 1 to March 26, 1860.

The treadwheel is a revolving drum, with steps placed at distances of 8 inches upon the outside of the cylinder; and the prisoners are required to turn the wheel downwards by stepping upwards. The rapidity of revolution is regulated partly by the weight of the prisoners, and partly by a governor, and therefore is not absolutely uniform. The prisoners grasp a crossbar, and partly hang by it, and the body is held behind its centre of gravity. They were engaged in this labour in alternate quarters of an hour, the intervening periods being occupied in perfect rest in the sitting posture. The duration of this mingled labour and rest was from 7.15 to 8.25 A.M., 10.10 A.M. to 1.50 P.M., and 3.10 to 5.20 P.M.; so that the total period of actual labour daily was 3½ hours. The total ascent per hour of continuous labour was 2160 feet, and per day 1·432 mile. The average weight of each man at the end of the inquiry was 105·125 lbs., 108·125 lbs., 120·5 lbs., and 122·625 lbs. avoirdupois, and of the whole 113·75 lbs.; and therefore the number of tons which they lifted 1 foot per day was as follows, upon the data used by Professor Haughton:—

Table XXXI.

No. of prisoner	858.	948.	1040.	1041.	Average of all.
Weight in lbs.	105·125	108·125	120·5	122·625	113·75
Tons, 1 foot daily	354·81	356·76	406·56	413·89	388·9

Hence the average labour of each man was represented by lifting 384 tons through 1 foot per day, and this was exacted on alternate days only, on Tuesday, Thursday, and Saturday, whilst Sunday was a day of almost unbroken rest. There were during the inquiry ten days of treadwheel labour, ten of very light labour, and four of perfect rest. Their daily dietary was as follows:—

20 oz. of excellent brown bread, one pint of cocoa, one pint of oatmeal gruel, 6 oz. of cooked meat without bone (4⅓ oz. lean and 1⅓ oz. fat), 8 oz. of boiled potatoes, about 1 oz. of salt, 10 oz. of water at midday, at 2 P.M. with dinner, and at 4 P.M. No. 1040 had 6⅔ oz. of bread daily. All the food was eaten, and the quantity of salt was regulated to three-quarters of an ounce daily on March 7. Thus the average solid food was 34 oz. (plus 6⅔ oz. bread for 1040, and the ingredients in the gruel and cocoa in all the

cases), and fluid 70 oz. daily. They were well inured to prison discipline and dietary; and all gained weight during the inquiry, except 1041, who lost a few ounces. They were in fair health, and willingly lent themselves to the inquiry. Their age, height, and occupation were as follows:—

TABLE XXXII.

No. of prisoner ...	858.	948.	1040.	1041.
Age	31	32	43	22
Height	5 ft. 2¼ inches.	5 ft. 2½ inches.	5 ft. 5½ inches.	5 ft. 7 inches.
Occupation ...	Plasterer.	Grocer.	Butcher.	Labourer.

Hence their average age, height, and weight were 32 years, 5 ft. 4¼ inches, and 113·75 lbs.

The object of the inquiry was to ascertain the weight of urea, chloride of sodium, urinary water and fæces, and to determine by final analysis the amount of nitrogen and ash in the food and in the urinary and fæcal excretions. There were also some variations in the food supplied during the inquiry. Thus, no salt was allowed from the dinner on March 10 to the dinner on March 14, except that added to the gruel; 3½ oz. of extra fat were given daily from March 14 to the end of March 17; half an ounce of tea was given daily with the water from midday on March 18 to the end of March 24; 1½ oz. of coffee was given daily from 1 P.M. on March 22 to the end of March 23; and lastly, 2 oz. of alcohol were given daily from the afternoon of March 24 to the end of March 26,—the duration of these latter inquiries being three days each.

The urine was collected from March 2 to March 17, from 6¼ to 7¼ A.M. whilst the prisoners were preparing for their duties and were more or less exposed to the open air; and this was regarded as the basis quantity, or that excreted in the absence of food and labour. On the treadwheel days a further quantity was collected from 7.15 to 8.25 A.M., during the treadwheel labour and before fluid or solid food had been taken. After these periods the whole of the urine was collected until 5½ P.M., when the prisoners were locked up for the night; and lastly, the urine passed from 5½ P.M. until 6¼ A.M. was collected. On Sundays (and also on weekdays after March 17) there were but two collections, viz. at 6¼ A.M. and 5½ P.M. The urine of each man was collected separately, and care taken that not the smallest quantity was lost; and whilst the fæces of each man were weighed separately, the whole was passed into one vessel, and a fair sample submitted to final analysis.

Urea.

The following Table contains the basal quantities of urea passed from 7¼ to 8.25 A.M. on each morning (except Sundays) from March 2 to 17, when that part of the inquiry terminated. It will be recollected that the quantity of salt allowed was reduced to three-quarters of an ounce on March 7, and that no salt was given (except that contained in the gruel) from March 10 at dinner time to March 14 at dinner time.

TABLE XXXIII.—Showing the basal quantities of Urea of each of four Prisoners on days of very light and of Treadwheel Labour.

1860.	No. 858.		No. 948.		No. 1040.		No. 1041.	
	Basal.	Treadwheel.	Basal.	Treadwheel.	Basal.	Treadwheel.	Basal.	Treadwheel.
March 3.	11·07	10·27	10·1	12·7	13·8	14·7	17·7	15·9
4.								
5.	10·78	.....	16	.....	16	.....	17·4	
6.	11·6	10·83	18·3	17·2	12	16·3	24	14·8
7.	21·34	.....	14·6	.....	16·2	.....		
8.	12·6	13	14	14·3	13·6	16·3	14·8	12·2
9.	11·9	.....	12·1	.....	17	.....	24·2	
10.	15·0	10·5	15·1	13·0	19·5	14·3		
11.								
12.	23·8	.....	12·6	.....	15	.....	22·5	
13.	.....	.....	13·6	11·5	16	15·4	16·1	14·8
14.	7·3	.....	9·1	.....	14	.....	25·8	
15.	16·8	14·4	12·8	11·3	16·5	23·3	26·3	13·1
16.	25	.....	11·6	.....	20·8	.....	41·3	
17.	6·24	13·03	16·5	14·6	16·5	24	34	18

The Table shows that the quantities of urea evolved by No. 1041 were notably larger than those of the other prisoners. On the average of the three others the quantity evolved per hour during the treadwheel labour was ·2 gr. less than that evolved at rest, viz. 14·4 grs. and 14·6 grs. per hour, but when the returns of No. 1041 are added, the defect with the treadwheel labour is no less than 2·4 grs. per hour. There was some diversity in the returns of each of the three cases; so that in one the quantity of urea was the same under both conditions, in another it was 2·5 grs. in excess with rest, and in the third there was 1·9 gr. in excess with labour. The numbers of times in which any excess was found with labour over rest in the three cases were 28, 33, and 71 per cent. in the order above given. The greatest excess with labour was 7·5 grs., and the greatest defect with labour was 5·3 grs. per hour, and both occurred in the same person. In one-third of all these exceptions the excess was less than 1 gr. per hour. Hence it is shown that violent exertion has no definite influence over the *excretion* of urea in the absence of food.

The following Table contains the results of the daily average of urea, chloride of sodium, and water, in each of the cases during the whole period of inquiry, distinguishing the days of treadwheel labour from those of comparative rest, and indicating the periods when certain additional foods were administered (Plate XXXV.).

TABLE XXXIV.—Showing the Daily Rate of Emission of Urea, Chloride of Sodium, and Urinary Water by four Prisoners, with and without Treadwheel Labour (Plate XXXV.).

1860.	Prisoner No. 858.						No. 948.						No. 1040.						No. 1041.							
	Light work and Sundays.			Treadwheel.			Light work and Sundays.			Treadwheel.			Light work and Sundays.			Treadwheel.			Light work and Sundays.			Treadwheel.				
	Urea.	Ch. Sod.	Urine.	Urea.	Ch. Sod.	Urine.	Urea.	Ch. Sod.	Urine.	Urea.	Ch. Sod.	Urine.	Urea.	Ch. Sod.	Urine.	Urea.	Ch. Sod.	Urine.	Urea.	Ch. Sod.	Urine.	Urea.	Ch. Sod.	Urine.		
	grs.	grs.	fl. oz.	grs.	grs.	fl. oz.	grs.	grs.	fl. oz.	grs.	grs.	fl. oz.	grs.	grs.	fl. oz.	grs.	grs.	fl. oz.	grs.	grs.	fl. oz.	grs.	grs.	fl. oz.		
March 2.	572	594	85·1	...	...	...	...	...	76·2	...	...	...	579	437	71·5	...	...	...	641	329	78·3	...	...	...	March 2.	
3.	...	...	...	591	664	...	...	...	...	547	411	70·1	...	...	...	659*	491	67·4	...	...	...	546	261	51·9	3.	
4.	555	699	80·5	...	...	70·6	516	438	81·5	...	...	...	597	594	61·4	...	...	...	568	374	66·5	...	...	...	4.	Salt 1 oz. daily.
5.	447	591	67·6	...	...	...	525	415	66·9	...	...	...	511	566	64·3	...	...	...	483	529	61	...	...	...	5.	
6.	...	...	...	590	556	...	...	...	...	551	373	85·3	...	...	...	511	415	58	...	...	...	604	544	74·1	6.	
7.	488	533	71·8	...	...	83·4	510	318	65·9	...	...	...	620	456	63·4	...	...	...	523	547	64·3	...	...	...	7.	
8.	...	...	...	570	773	...	...	...	...	546	428	78·5	...	...	...	570*	652	61·3	...	...	...	548	545	68·7	8.	
9.	471	526	72·7	...	...	81·4	712	363	68·9	...	...	...	616	622	66	...	...	...	513	571	59·9	...	...	...	9.	Salt ½ oz. daily.
10.	...	...	...	501	418	81·8	...	...	...	566*	355	67·5	...	...	...	554*	468	84·7	...	...	...	553	577	80·7	10.	
11.	441	163	76·4	...	...	...	485	149	77	...	...	...	666	110	74·5	...	...	...	476	242	88·5	...	...	...	11.	
12.	528	169	80·4	...	...	...	482	182	69·2	...	...	...	574	193	79·5	...	...	...	577	219	86·5	...	...	...	12.	No salt.
13.	...	...	...	544	84	69·2	...	...	...	512	200	84·1	...	...	...	616	223	71·2	...	...	...	510*	203	76·4	13.	
14.	514	288	48	...	...	...	470	178	51·1	...	...	...	498	202	38·3	...	...	...	570	155	54·6	...	...	...	14.	Salt ½ oz. daily.
15.	...	...	...	414*	265	47	...	...	...	548	246	68·9	...	...	...	579	382	58·3	...	...	...	487*	141	49·2	15.	Fat 3½ oz. extra.
16.	582	273	84·8	...	...	...	492	251	63·9	...	...	...	550	534	65·4	...	...	...	575	514	59·2	...	...	...	16.	
17.	...	...	...	493	525	62·9	...	...	...	489*	410	88·1	...	...	...	577	536	63·5	...	...	...	567*	514	61·4	17.	Fat ended.
18.	386	562	61·8	...	...	...	405	393	82·5	...	...	...	486	595	64·8	...	...	...	606	757	84·2	...	...	...	18.	Tea ½ oz. daily.
19.	400	542	65·5	...	...	...	462	462	70	...	...	...	454	595	62	...	...	...	451	530	49·1	...	...	...	19.	
20.	...	...	...	544	615	81	...	...	...	498	476	75·5	...	...	...	541	592	70·5	...	...	...	451*	405	52	20.	Tea ended.
21.	430	552	68·2	...	...	...	469	483	58	...	...	...	531	724	73·2	...	...	...	524	583	68·2	...	...	...	21.	Coffee 1 oz. daily.
22.	...	...	...	533	552	74·0	...	...	66·5	567	348	83	...	...	...	...	...	69·6	...	...	...	...	...	69·5	22.	Coffee 1½ oz. daily.
23.	527	492	74·5	...	...	...	527	320		...	...	...	473	473	60·7	...	...	...	509	415	62·8	...	...	...	23.	Coffee ended.
24.	...	...	...	439*	316	58·5	...	...	...	461*	238	41·7	...	...	...	443*	495	59	...	...	...	523	482	53·5	24.	Alcohol 2 oz. daily.
25.	456	380	52·7	...	...	...	475	237	44	...	...	...	418	382	49	...	...	...	436	286	41·5	...	...	...	25.	
26.	574	410	52·5	...	...	...	472	226	47·2	...	...	...	545	424	50·5	...	...	...	543	347	46·2	...	...	...	26.	Alcohol 1½ oz. daily.

The average of all the observations gives the following results. On Sundays the elimination was to the extent of 494 grs., on days of comparative rest 512 grs., and on treadwheel days 528 grs. The increase on the days of treadwheel labour over that of merely routine labour was 16 grs. per day. The prisoner No. 1041, to whose exceptional returns I have before alluded, had no average increase with treadwheel labour, but, on the contrary, there was an average decrease of 51 grs., and the increase was carried on to the following day. The three other cases exhibited an increase of 37 grs., 59 grs., and 21 grs. daily, or an average of the three of 42 grs. daily with labour.

None of the cases were without an exception in the relative influence of treadwheel and mere routine labour; but one had two, another had three, and two had four exceptional days on which there was no increase on the treadwheel days. These instances are marked in the Table with an asterisk. When there was no increase on the treadwheel days, there was an increase on the following day of rest; and this irregularity alternated several times in succession.

The cause of these exceptions is not clear; but I am fully convinced that it is not due to any error, nor to any change in the dietary or the habits of the men. There were, however, two noticeable meteorological conditions at the periods when the marked variations occurred. Thus, from March 6 to March 11 the temperature fell from 40° to 30°·9, when the exceptions in No. 1040 occurred, and rose considerably on the day on which the exceptions ended, and at the same period the barometer rose very greatly, and remained at above 30 inches until the day before the exceptions terminated. These would exert a mutually opposing influence. The contrary conditions occurred on the exceptional days in case No. 1041; for the temperature rose from 30°·9 to 46°·5 from March 11 to March 20, when the exceptions ended; and the barometer having fallen to 29·570 inches on the day when the exceptions began, reached 30·012 inches on the day on which they terminated. There is thus at least a singular coincidence in the existence of these disturbing causes and a succession of exceptional results; and on each occasion the usual effect of the treadwheel labour over the *elimination* of urea was retarded one day.

The largest increase from the treadwheel labour was 144 grains, and the largest decrease was 100 grs. of urea per day.

The following Table contains the percentage results of this inquiry.

TABLE XXXV.

	The whole period.	To March 17, before extra food was given.
Average increase from treadwheel over routine labour, in all cases combined	per cent. 3	per cent. 3·4
Average increase from treadwheel in the three most regular cases	7·4	4·7
Average increase from treadwheel over Sundays (all combined)	6·4	
Average increase from treadwheel over Sundays, three most regular cases	10	
Average increase in the cases Nos. 943, 838, and 1040	7, 11, and 3·7	7·2, 3, 2
Average increase on days of routine labour over the treadwheel days, exceptional case No. 1041	10	
Average increase on treadwheel days, No. 1041	.....	1·5
Maximum increase from treadwheel over routine labour	26	20
Maximum decrease from treadwheel over routine labour	19	19

In order to ascertain how far the addition of alcohol, tea, and coffee may have modified these results, I have in the last column of the preceding Table separated the average returns before those substances were administered. The exceptional case, No. 1041, was regular up to that period, and on the whole average yielded a small increase on treadwheel days; but the general results in the other cases were varied only so far as that the effect of the treadwheel was less on the short than on the long average.

The average amount of urea excreted daily to each pound weight of the body is as follows; and for comparison I have added the proportion in myself.

TABLE XXXVI.

No. of prisoner.....	858.	948.	1040.	1041.	Average of the whole.	Myself.
Urea, grs. to each lb.....	4·61	4·74	4·58	4·39	4·58	2·73

The foregoing facts prove that, whilst there is no average increase in the *elimination* of urea during the period of actual treadwheel labour without food, there is a small increase in the production on the whole day when ordinary food is taken; but on some occasions this increase is not eliminated on the day of labour, but on the following one of rest. The proportion of urea to the weight of the body on the whole period of labour and rest combined, is 59 per cent. greater than is observed in myself with much greater weight, less food, and less labour.

RUDOLPH found that during fasting there was an increase in the solids in the urine emitted, on the increasing weights of the men; but BENEKE and others have shown that, whilst there may be this general relation, there is much diversity in the results. Moreover, since in one person the bones, and in another the fat, may be relatively heavier than the muscle in a third, and since in relation to body weight the amount of food and the activity of the vital functions vary much, it is impossible that there should be any strict relation. In the case of these prisoners there was but little fat, and the bones were not unusually large, and the muscular system was over worked and under fed. They did not lose weight during this inquiry; but they had been long imprisoned, and the weight with which the final weighing was compared was not that on their entrance into the prison, but the reduced one at the period of this inquiry. It is probable that the knowledge of the relation of urea to body weight is of very little value.

Urinary Water.

The excretion of urinary water was greater on treadwheel days than on days of comparative rest. On the whole cases combined the daily quantity was 69·6 oz. on the former and 64·6 oz. on the latter days, or an increase of nearly 8 per cent. on the days of severe labour. The relative amounts evolved during the first ten days, before any variation of the food occurred, were 74·7 and 67·7 oz., giving an increase with labour of 10·4 per cent.

The rule thus established was not maintained without variation; but on every short

average, and in all the cases, it was observed. The following Table contains the average quantities of urinary water emitted in each of the cases from March 2 to March 10, with the treadwheel and with light labour.

TABLE XXXVII.

No. of prisoner / 1860.	858.		948.		1040.		1041.		Total average.	
	Light work.	Tread-wheel.	Light work.	Tread-wheel.	Light work.	Tread-wheel.	Light work.	Tread-wheel.	Light work.	Tread-wheel.
	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.	fl. oz.
Urine ...	73·15	79·4	76·8	82·87	63·8	67·9	62·9	68·9	67·7	74·7

Chloride of Sodium.

The elimination of chloride of sodium was commonly less on treadwheel than on the other days, but the difference was not very considerable. On the average of all the observations during the first ten days, the quantity of chloride of sodium evolved daily was 509 grs. with treadwheel, and 520 grs. with light labour. After that period the chloride of sodium was temporarily withheld; but upon the average of all the observations throughout the inquiry, the amount of that salt emitted was slightly less on treadwheel than on other days, the actual quantities being 432 grs. and 437 grs. There was much variation in the results both on the same and on different prisoners.

Fæces.

The analyses of the fæces were made by Mr. MANNING, from March 2 to March 16 inclusive, including two Sundays, six treadwheel, and six light-labour days. There was no instance in this part of the inquiry of the evacuation being of a purged character. Nos. 858 and 1040 had only one day without an evacuation, and No. 948 had an evacuation daily; but No. 1041 in but one instance had more than one evacuation every second day. The analysis on each day was made from the evacuation of that day; and therefore on every alternate day there was no fæcal analysis in the case of No. 1041. In determining the amount of the nitrogen emitted by the fæces daily, I have been compelled to admit a small error by dividing the quantity of fæces evacuated by No. 1041, on each occasion, equally between that and the preceding day, and to adopt a similar plan on the two exceptional occasions just mentioned in reference to the other prisoners. This prevents the accurate determination of the amount of nitrogen evolved by No. 1041 on any particular day, as, for example, on the treadwheel days, and so far lessens the characteristic results of each day, on the average of the whole cases. I have also referred the fæces to the *preceding* day, as they are clearly connected with the food of that day.

The following Table gives the total daily weight of the fresh fæces, with the total day emissions of nitrogen, and the amount of water, nitrogen, and ash in each ounce of the fresh fæces.

TABLE XXXVIII.—Showing the amount of Nitrogen and Mineral Matter evolved daily in the Fæces of four Prisoners, and placed under the preceding day.

		Fæces.				Total fæces, average.	Total nitrogen.	In 1 oz. of fresh fæces.	
		No. 856.	No. 948.	No. 1040.	No. 1041.			Water.	Nitrogen.
	March.	oz.	oz.	oz.	oz.	oz.	grs.	per cent.	grs.
	1.	9·75	4·25	6	2·5	5·62			
	2.	7	11·9	6	14·75	9·91	39·7	73·8	4·363
Wheel	3.	5·5	4·25	10·75	9·08	7·4	35·87	72·6	4·848
Sunday	4.	17	12	24·9	9·08	15·74	69·25	73·2	4·4
	5.	19	5·66	12·5	6·4	10·89	49	77·6	4·5
Wheel	6.	4·15	6·5	17	6·4	8·51	42·2	73·7	4·96
	7.	7·75	5·15	9·25	13·3	8·86	43·23	73	4·88
Wheel	8.	11·41	4	12	13·3	10·1	45·75	72·8	4·52
	9.	8·12	7·75	2·4	·87	4·8	21·22	74·3	4·42
Wheel	10.	8·12	1·75	2·4	·87	3·78			
Sunday	11.	9·75	12	10·4	11·5	10·91	50·59	73	4·637
	12.	4·75	4·25	8·5	5·45	5·71	27·17	71·8	4·76
Wheel	13.	8·66	11·34	9·5	5·45	8·74	42·86	72	4·904
	14.	8·5	10·25	10·5	7·9	9·28	41·46	74·1	4·468
Wheel	15.	9·5	5·75	9·25	7·9	8·1	36	73·3	4·45

The weight of fæces evolved per day was on the whole average 8·55 oz., and in the different cases 9·26, 7·1, 10·1, and 7·64 in their order; but it varied on different days from 24·9 oz. with daily evacuations, and 26·59 oz. with evacuations on alternate days to 1·75 oz. The uniform weight of solid food taken daily was 38 oz., and consequently the weight of the fæces on the average was 22½ per cent. of that of solid food.

The largest emission of fæces was on the Sunday, when it was 44·3, 70, and 74 per cent. higher than the average of all the days in the three first-mentioned cases.

There was a smaller evacuation on the wheel days than on the average of all days, in two of the cases to the extent of 14·8 and 21·1 per cent.; but in the third case both averages were the same. The average diminution, as compared with the quantities evacuated on Sundays, was 41, 53·3, and 42·6 per cent. The least evacuation occurred on the Saturday, which was also a treadwheel day; and in the three cases the diminution below the whole average was 26·1, 57·6, and 34·6 per cent.; but it was less than that of the Sunday by no less than 48, 75, and 62 per cent.

The amount of water contained in the fæces was very uniform, viz. 73·5 per cent. on the average, and varied only from 71·8 to 77·6 per cent. on different days. It was not above the average on Sundays, and it was a little below the average on the treadwheel days. The quantity of nitrogen in each ounce of fæces varied from 4·36 to 4·9 grs., and on the average it was 4·646 grs. The total quantity contained in the daily evacuation of fæces was an average amount of 41·8 grs. There was a considerable increase on the Sunday, and a marked decrease on Saturday, and it was below the average on the treadwheel days,—the actual amount on the three days being 59·9, 35·8, and 40·53 grs., or an increase of 43·3 per cent., and a decrease of 14·3 and 3 per cent.

Hence on Sundays there was a diminution in the amount of urea evolved from March 2 to the 16th, of 13 grs. and 18 grs. in the first two cases, but there was increase

in the nitrogen evolved in the fæces by the three cases, which when reckoned as urea amounted to 17·33 grs. There was no decrease in the quantity of urea on Sundays at this period by the third case, but, on the contrary, there was an increase of 52 grs. above the average of all days. The loss of nitrogen in the urine was thus found in the fæces. The case which was allowed 6⅔ oz. of extra bread per day had the largest amount of fæces on the average, and the largest increase of fæces on Sundays. The total average excretion of fæces was 73 per cent. greater than in myself, with an equal amount of food and much more exertion.

The foregoing investigation has elicited the following facts.

1. The prisoners emitted much more urea and fæces than occurs in health under ordinary circumstances.

2. On Sundays the amount of urea was commonly lessened, but the nitrogen in the fæces was increased in the same proportion. The whole weight of fæces was increased.

3. With treadwheel labour there was a small increase in the amount of urea and of urine evolved, whilst there was a small decrease in the evolution of chloride of sodium by the urine, the weight of the fæces, and the nitrogen contained in the fæces. On Saturdays, with treadwheel labour the diminution in the fæces and the contained nitrogen was considerable.

4. With increase in the allowance of bread there was a considerable increase in the weight of the fæces, and particularly with rest.

[The following are the results of the experiments which were made upon four prisoners in Wakefield Prison in June 1861, and to which reference has been made at pp. 749–751. I have arranged them in the order observed in reference to the experiments at Coldbath Fields Prison; and, for further convenience of comparison, the pages at which the latter have been recorded in this paper are annexed to each subject of inquiry. The details of the results are given at length in Table XXXIX.

TABLE XXXIX.—The following Table contains the daily quantities of

Two

Date.	Daily Ingesta.									Fæces.			
	Bread.			Chloride of sodium, besides that in the bread.			Water (not in food).			Quantity.		Nitrogen.	
	No. 182.	No. 184.	Average.	No. 182.	No. 184.	Average.	No. 182.	No. 184.	Average.	No. 182.	No. 184.	Per cent., both.	Total daily quantity.
1861.	grs.	grs.	grs.	grs.	grs.	grs.	fl. oz.	fl. oz.	fl. oz.	oz.	oz.	grs.	grs.
June 28	10,913	10,898	10,906	194	49	121·5	18	10½	14½	6·88	9·66	...	...
„ 29	10,849	10,338	10,594	191½	75½	108·5	16	17	16½	4·33	7·19	...	...
„ 30, Sunday	10,791	11,192	10,992	201½	45½	123·5	15	17	16	5·6	10·85	·789	28·5
July 1	10,908	10,748	10,828	187½	87½	137·5	4	10	7	7·53	11·41	·71	29·43
„ 2	10,988	10,727	10,858	190	86	138	29	31	30	5·55	5·35	1·03	24·51
„ 3	12,612	10,500	11,556	192½	92	142·2	10	5	7½	4·83	9·64	·88	27·59
„ 4	9,552	10,145	9,899	178	102½	140·2	9	3½	5½	7·09	10·28	·77	29·27
„ 5	10,430	10,446	10,438	224	115	169·5	8	7½	7½	5·55	5·06	1·08	25·1
„ 6	11,275	10,095	10,685	202	143½	172·7	17½	8	12½	6·21	10·82	·79	29·45
„ 7, Sunday	10,854	10,343	10,599	190½	42½	166·5	9½	11	10½	5·66	5·9	·98	24·77
„ 8	10,206	9,187	9,697	192	39	116	5	9	7	4·83	10·1	·93	30·48
„ 9	9,830	11,331	10,581	231	41½	136·2	15	7	11	5·09	4·53	1·16	24·42
„ 10	9,893	11,256	10,575	217	40	128·5	11½	9½	11½	5·53	5·78	1·14	28·23

Two COCOA-

Date.	Daily Ingesta.									Fæces.			
	Bread.			Chloride of sodium, besides that in the bread.			Water (not in food).			Quantity.		Nitrogen.	
	No. 7.	No. 39.	Average.	No. 7.	No. 39.	Average.	No. 7.	No. 39.	Average.	No. 7.	No. 39.	Per cent., both.	Total daily quantity.
1861.	grs.	grs.	grs.	grs.	grs.	grs.	fl. oz.	fl. oz.	fl. oz.	oz.	oz.	grs.	grs.
June 28	12,988	12,848	12,918	86½	38½	62·5	31½	60	45½	9·04	8·48	...	...
„ 29	14,543	14,664	14,604	88	39½	63·7	40	61	50½	13·51	5·96	...	...
„ 30	11,528	14,305	12,917	78½	None	...	14	16	15	6·63	5·88	1·03	26·41
July 1	13,607	13,475	13,541	69	37½	53·2	40	17½	28½	2·03	10·33	1·24	34·16
„ 2	13,127	12,407	12,767	73	37	55	40	29	34½	10·11	8·52	1·15	46·26
„ 3	13,629	13,758	13,694	88½	42	65·2	29	40	34½	8	13	1·10	50·71
„ 4	13,314	13,412	13,363	105	40½	72·7	40	40	40	5·35	9·23	1·04	34·66
„ 5	12,829	14,588	13,709	86	41	63·5	40	40	40	4·32	7·94	0·97	26·01
„ 6	12,260	14,050	13,155	93	50	71·5	40	29	34½	10·94	8·64	1·35	57·8
„ 7	13,254	13,340	13,297	82	25½	58·7	30½	14	22½	1·72	14·4	1·00	35·28
„ 8	12,613	12,687	12,650	85	54	74·5	33	40	36½	10·7	5·28	1·31	45·81
„ 9	12,943	13,753	13,348	88	76½	82·2	50	40	45	6·74	12·84	1·05	45·03
„ 10	13,232	13,010	13,121	80	66	73	40	29	34½	10·48	10·45	1·05	48·1

the Ingesta and Egesta in Two Tailors and Two Cocoa-Matting Weavers.

TAILORS.

Daily Egesta.									Weight of body, in lbs. and oz. avoirdupois.			Urea to 1 lb. of body-weight.
Urine. Quantity.			Urea.		Nitrogen, daily.		Chloride of sodium.					
No. 182.	No. 184.	Average.	Per fl. oz. of urine.	Total daily.	Urine.	Total in urine and fæces.	In each fl. oz. of urine.	Total daily in urine.	No. 182.	No. 184.	Average.	
fl. oz.	fl. oz.	fl. oz.	grs.	grs.	grs.	grs.	grs.	grs.	lbs. oz.	lbs. oz.	lbs.	grs.
									124 13	117 9¼	121·35	
44·4	56·6	50·5	14·4	707	330·4	.....	3·0	151·5	125 2¾	117 8	121·35	5·82
59·17	39·04	49·1	14·76	725	338·8	.....	3·6	176·7	125 2	118	121·28	5·91
35·4	43·45	39·45	14·7	580	271	299·5	3	118·3	125 11½	118 11½	122·21	4·74
40·0	50·67	45·38	12·30	569	265·8	295·23	2·7	122·4	125 10¾	118 5½	122	4·66
31·7	43·3	37·5	13·36	501	234·1	258·61	3	112·5	125 8½	119	122·26	4·09
32·4	38·5	35·45	13·71	456	213	240·59	2·7	95·7	125 13½	118 13¾	122·36	3·72
34·9	41	37·95	14·44	547	255·6	284·87	3·6	136·6	126 3½	118 10¾	122·43	4·46
35·7	40	37·85	15·64	592	278·5	303·6	3·6	135·0	126 2½	119 7	122·8	4·82
49	47·5	48·25	13·83	668	312·1	341·55	2·7	130·27	125 15½	118 9½	122·27	5·46
33·3	42·5	37·9	14·4	546	255·5	280·27	4	151·6	126 4½	119 3½	122·75	4·44
38·4	39	38·7	17·04	660	307·4	337·88	4	154·8	126	119 1½	122·53	5·38
33·8	41·5	37·65	18·8	708	376·6	401·02	4	150·6	125 11½	118 9	122·14	5·79
39·3	40	39·65	16·4	650	303·7	331·93	4	158·6	125 13½	119 5½	122·6	5·3

MATTING WEAVERS.

Daily Egesta.									Weight of body, in lbs. and oz. avoirdupois.			Urea to 1 lb. of body-weight.
Urine. Quantity.			Urea.		Nitrogen, daily.		Chloride of sodium.					
No. 7.	No. 39.	Average.	Per fl. oz. of urine.	Total daily.	Urine.	Total in urine and fæces.	In each fl. oz. of urine.	Total daily in urine.	No. 7.	No. 39.	Average.	
fl. oz.	fl. oz.	fl. oz.	grs.	grs.	grs.	grs.	grs.	grs.	lbs. oz.	lbs. oz.	lbs.	grs.
									146 8½	147 3½	146·87	
36·8	62·3	49·55	13·74	671	313·4	.....	2·4	118·9	146 10	146 6½	146·51	4·55
42·7	61·15	51·92	13·8	717	335	.....	2·7	140·1	146 3½	146 4½	146·25	4·9
41·06	48·9	44·98	15·36	691	322·9	349·31	2·4	107·9	147 6½	146 11	147·1	4·7
53·8	40	46·9	14·76	692	323·3	357·46	3·3	154·7	147 9½	146 11	147·15	4·7
53·9	35·7	45·8	13·89	637	297·3	343·56	3·9	178·6	146 10½	146 13½	146·75	4·34
29·4	42·9	46·15	15·42	558	262·6	313·31	3·6	166·1	147 0½	146 11½	146·9	3·62
45·5	45	45·25	15·9	710	331·8	366·46	3·6	162·9	147 5½	146 14	147·11	4·83
55	44	49·5	15·45	765	357·3	383·31	3·3	163·3	147 2	146 0½	146·58	5·21
74	40·5	57·25	13·53	775	362·1	519·9	2·7	154·5	146 4	146 10	146·43	5·29
48·5	37·5	43	15·87	683	319·1	354·38	2·7	116·1	148 3½	147 12½	147·98	4·61
49	44	46·5	15·2	707	330·3	376·11	3·6	167·4	146 14½	147 12	147·32	4·8
46	46	46	16	736	343·9	388·93	3·6	165·6	144 15½	147 11½	146·34	5·02
42	46·5	44·25	17·84	790	369·1	417·2	3·6	159·3	144 9¼	147 2¼	146·36	5·39

Urea (p. 809).

The analysis for urea was made by LIEBIG'S method in the manner already described in reference to the experiments at Coldbath Fields.

The total average daily quantity of urea evolved was 655·65 grs., of which 608·4 grs. were emitted by the Tailors, and 702·9 grs. by the Weavers. The maximum and minimum amounts were 790 grs. and 456 grs.,—the former in the Weavers, and the latter in the Tailors. In the Weavers the quantity exceeded 700 grains on seven of thirteen days, whilst it occurred only three times during that period in the Tailors; and in only one instance during the inquiry was it below 500 grs. daily.

The quantity of urea to each pound of body-weight (p. 810) was 4·812 grs. in the Tailors, and 4·675 grs. in the Weavers; but it varied in the former from 3·72 grs. to 5·82 grs., and in the latter from 3·62 grs. to 5·39 grs. on different days. The quantity of urea eliminated was always lessened on the Sunday (pp. 809, 812). The diminution in the Tailors from the Saturday to the Sunday was 145 grs. and 122 grs., and in the Weavers 26 grs. and 92 grs., yielding an average diminution of 96·25 grs.

The quantity of urea in each ounce of urine was on the average 14·9 grs. in the Tailors, and 15·25 grs. in the Weavers, giving a total average of 15·075 grs. The maximum and minimum quantities were 18·8 grs. and 12·3 grs. in the Tailors, and 17·84 grs. and 13·53 grs. in the Weavers.

Weight of Body (p. 810).

The average weight of three of the prisoners during the inquiry was greater than that recorded on the day preceding the commencement of the inquiry; but there was a loss of weight of the fourth. The average gain of the Tailors was $15\frac{1}{2}$ oz. and $17\frac{3}{4}$ oz., and of one of the Weavers $3\frac{1}{4}$ oz., but the other Weaver lost $3\frac{3}{4}$ oz. The greatest gain in the different cases was 1 lb. $13\frac{1}{4}$ oz. and 1 lb. $7\frac{1}{2}$ oz. in the Tailors, and $8\frac{3}{4}$ oz. and 1 lb. 11 oz. in the Weavers; and the greatest loss was $1\frac{1}{4}$ oz. in one Tailor, and 1 lb. $2\frac{1}{4}$ oz. and $4\frac{1}{2}$ oz. in the Weavers. There was not any unvarying progression in the weight during the week; but in every case there was an increase from the Saturday to the Sunday, and the amount was as follows: $11\frac{1}{4}$ oz. and $10\frac{1}{4}$ oz., $9\frac{1}{2}$ oz. and 5 oz. in the Tailors; $6\frac{1}{4}$ oz. and $18\frac{1}{4}$ oz., $19\frac{1}{4}$ oz. and $31\frac{1}{2}$ oz. in the Weavers, or an average increase on the Sunday of 13·62 oz.

Urine (p. 810).

1. *Quantity.*—The largest quantity of urine evolved in one day was 25,321 grs. (56·6 oz.) and 26,624 grs. (59·17 oz.) in the Tailors, 27,791 grs. (62·3 oz.) and 32,924 grs. (74 oz.) in the Weavers. The average daily quantity was 41·2 oz. in the Tailors, and 47·51 oz. in the Weavers, giving a total daily average of 44·35 oz. There was a large increase on the Saturday, and a marked decrease on the Sunday, as the following quantities prove.

TABLE XL.—Showing the average quantity of Urine evolved on Friday, Saturday, and Sunday.

	Friday.	Saturday.	Sunday.
	oz.	oz.	oz.
Two Tailors	.....	49·1	39·45
	37·85	48·25	37·9
Two Weavers.....	.....	51·92	44·98
	49·5	57·25	43

The average decrease from the Saturday to the Sunday was 10·29 oz.

2. *Specific Gravity.*—The specific gravity of the urine varied from 1016 to 1027·5, but there was singular uniformity in the general results. In the Tailors it was 1023·7 and 1025, and in the Weavers 1024·37 and 1024·6, giving a total average of 1024·35 in the Tailors and 1024·45 in the Weavers.

Chloride of Sodium (p. 811).

The average quantity of chloride of sodium evolved was 3·37 grs. per oz. in the Tailors, and 3·18 grs. per oz. in the Weavers, giving a daily emission of 138·8 grs. in the former, and 148·5 grs. in the latter.

Fæces (p. 811).

The general character of the fæces was homogeneous and moderately cohesive; but on a few occasions there was variety in the consistence. In the 52 observations, 32 exhibited fæces formed but soon subsiding, 7 well-formed, 1 scybalous, 2 soft, and 9 of mixed characters; and no one prisoner offered any very marked difference in those conditions. The trace of the bran of the bread was easily seen in the fæces. The average daily evacuation was 6·98 oz. in the Tailors, and 8·52 oz. in the Weavers, giving a total daily average of 7·75 oz. There were somewhat considerable daily variations; so that the maximum and minimum quantities were in the Tailors 11·41 oz. and 4·32 oz., and in the Weavers 14·42 oz. and 1·72 oz.; but in no instance was there the omission of a daily evacuation.

The quantity of nitrogen per cent. found by Mr. MANNING by the volumetric method, varied from ·71 gr. to 1·16 gr. in the Tailors, and from ·97 gr. to 1·35 gr. in the Weavers; but the total average in the two classes was ·93 gr. in the Tailors and 1·12 gr. in the Weavers, giving 1·025 gr. on the whole.

The total daily elimination of nitrogen by the fæces were found to be 27·43 grs. in the Tailors, and 40·93 grs. in the Weavers. The variation in the amount of fæces on Sunday from that of other days was not uniform in quantity, since it was less on Sunday than on week days in the Weavers, and was equal in the Tailors.

Such is a summary statement of the results obtained in the inquiry on two classes of prisoners, one of which followed a sedentary and non-laborious occupation, whilst the

other really performed hard labour; and it will be observed that there were many differences in the results obtained from them. Both classes had a similar and substantial dietary, but there were certain personal differences. Thus, the weavers of cocoa-matting in the wide loom, when compared with the Tailors, were older, taller, heavier, and broader. They ate more bread, milk, and water. They lost weight, whilst the Tailors gained weight. They emitted more urine, urea, chloride of sodium, and fæces with their contained nitrogen. They exhibited much less diminution in the amount of urea evolved on the Sunday, and a little less urea to body-weight.

It is not possible to compare the results of this inquiry very closely with those already described in reference to Coldbath Fields, since the conditions in each set of experiments were not identical. At Coldbath Fields the quantity of bread and water was rigidly fixed, whilst at Wakefield there were daily variations according to the desires of the prisoners. The quantity of bread eaten was greater at Wakefield than at Coldbath Fields, and would so far increase the amount of urea *produced*, whilst the variable quantity of water taken from day to day would at the same gaol vary the *elimination* of that product. Yet these causes of variation have only a certain value; and upon the whole it will be seen that there is a very close correspondence between the excretions of the Weavers at Wakefield and those who worked the treadwheel at Coldbath Fields.

The weight of the men at Wakefield was greater than that of the prisoners at Coldbath Fields; the quantity of urine and of fluid drank was less, and that of urea was greater; but the proportion of urea to body-weight was very nearly the same at the two prisons. In both there was more urea evolved on days of hard labour, and less on Sunday. There was less chloride of sodium evolved, as there was less supplied in the food. The weight of the fæces and the contained nitrogen was the same at both places.]

8. *Certain relations of Urea to Food.*

All observers have found that the amount of urea varied with the food taken, and was the greatest with that food which yielded the most nitrogen—as, for example, albumen and gelatine. This has been well established by Lehmann, Bidder and Schmidt, and Lawes and Gilbert, and many others of the best repute; and there was usually a correspondence between the increase in the nitrogen supplied and excreted, if the body-weight remained the same.

I have in former parts of this paper referred to the influence which food exerts upon the excretion of urea, and particularly in the effect of absence of food over the hourly rate of excretion, and the effect of increased food on Sundays. I propose now to show the effect of food taken at a late hour, and of unusual kind and quantity, as occurred on various occasions at dinner and evening parties during the year, and then to refer to a few special experiments on foods.

Instances of excess of Urea after Dinner and Evening Parties, when no excess in food or wine, in the ordinary acceptation of the term, was committed.

Dinner.—Feb. 25. The urea increased from 453·4 grs. on the preceding day, and an average of 438·8 grs. on the three preceding days, to 594·3 and 582 grs. on the same and the succeeding day; also to an average of 648 grs. on the two following days, and 517·2 grs. on the fourth day, and afterwards it fell to 488 and 483 grs. The quantity of urea emitted on the first night was 60 per cent. greater than on the preceding night.

Supper.—Feb. 9. There was no increase in the urea emitted during the night, but on the following day the increase was 144 grs.

Supper.—Feb. 16. The urea increased from 470·6 grs. on the previous day to 539·6, 543·7, 534·4, and 522·1 grs. on succeeding days, and fell to 397 grs. on the fourth day. There was an increase on the first night, and the basis quantity on the following morning was increased 49 per cent.

Dinner, a bottle of Moselle.—March 8. There was an increase of 125 grs. of urea during the night, and a maximum increase of 230 grs. on the following day.

Dinner, pint of mixed wine.—March 23. There was no increase in the quantity of urea until the second day.

Supper.—April 12. There was an increase during the night and on the two following days to the extent of 100 grs. on each day.

Supper.—April 19. After merely taking cream and wine there was an increase during the night, and the basis quantity on the following morning was increased 33 per cent.

Soirée.—April 21. There was an increase of 54 grs. during the night, and the basis quantity was elevated.

Supper.—April 26. There was an increase during the night of 76 grs. of urea, and it continued through the next day.

Supper, meat and ale.—May 11. There was an increase of 42 grs. of urea on the day following. The supper was followed by a sensation of excess.

Supper.—Nov. 22. The urea increased from 490 grs. to 548 grs., and fell on the administration of medicine.

Supper.—Dec. 12. The increase in the urea during the night was 7 grs. per hour, and on the following three days it was 100, 130, and 134 grs.

Dinner.—Dec. 30. The urea was increased 63 per cent. during the night, and on the next day the increase was 92 grs.

Dinner.—Jan. 31. After taking three or four glasses of strong wine and dining heartily I was ill, and vomited very acid matter. There was an increase of 3·4 grs. of urea during the day, but on the next day the quantity was lessened.

[Examples of a similar kind are recorded in the Tables, p. 759 *et seq.*, on March 27, April 15, May 18 and 20, June 12, November 18 and 30, 1861, and February 1, 1862.]

Hence in every instance, and at all periods of the year, when food of an unusual kind and in unusual quantity was taken and retained, an increase in the elimination of urea immediately followed, and continued during periods varying from one to four days. In reference to the influence of wine, I have remarked that on the occasion on which the wine was not good, and any stomach derangement followed, there was a much larger emission of urea than occurred with a greater quantity of wine of fine quality.

[Milk supper was also followed by an increased elimination of urea on May 18, July 11 and 21, September 23; and the same result followed the eating of crab and cow-heel on May 13 and December 1, but the effect was not uniform.]

The influence of some special kinds of food.

As many of the experiments in reference to the action of foods did not extend beyond a part of a day, they cannot show any influence over the daily excretion of urea, and I shall not here refer to them.

On the occasion on which I lived on bread with water, tea, and coffee during three days, the total amount of urea, the quantities of urea and urinary water, were as follows:—

TABLE XLI.

34 oz. bread, 48 oz. water.		34 oz. bread, 48 oz. water, 350 grs. tea.		34 oz. bread, 48 oz. water, 2 oz. coffee.	
Urea.	Urine.	Urea.	Urine.	Urea.	Urine.
grs. 442	fl. oz. 67·85	grs. 467·28	fl. oz. 41·55	grs. 493·16	fl. oz. 52·04

The amounts evolved on the preceding day were 550 grs. of urea and 57·73 oz. of urine; but as that was Sunday, there would have been, with ordinary food, a considerable decrease on the Monday, and hence we lack a good basis for comparison. There can be no doubt, however, that there was a considerable diminution of urea on the day on which bread and water alone were taken; but there was a progressive increase on the two following days, when tea and coffee were administered. On the following day, when bread and water only were taken until 2½ P.M., and then plenty of additional food, the urea increased to 507·2 grs.

The diminution in the excretion of urine with the tea was in part the result of the large elimination on the preceding day. The quantity evolved with the coffee was normal.

The appetite was lessened under the influence of coffee, and there was an odour of coffee in the urine.

If we accept HAUGHTON'S analysis of bread, and consider that 1 oz. of fine bread is equivalent to 12·25 grs. of urea, we find that on the first day there were 25 grs. of urea eliminated by the urine more than could have been contained in the bread.

On the occasion when extra foods were given to the prisoners, the following were the average results obtained:—

TABLE XLII.

3½ oz. extra fat.		½ oz. tea.		1½ oz. coffee.		2 oz. alcohol.	
Urea.	Urine.	Urea.	Urine.	Urea.	Urine.	Urea.	Urine.
grs. 529	fl. oz. 69·17	grs. 474	fl. oz. 68·37	grs. 515	fl. oz. 69	grs. 489	fl. oz. 49·96

The urea and urine, on the average of the four preceding days, were 525 grs. and 70·3 oz.; but the quantity of urea had fallen from the commencement of the inquiry.

Hence the extra fat produced no average change in the excretion of urea and urine; but in No. 858 both were diminished, and in No. 1041 the urea was increased.

BÖCKER and BISCHOFF have shown that, in the ordinary conditions of the system, an increase in the quantity of fat supplied does not vary the amount of urea evolved.

The urea was greatly lessened under the influence of the tea, but that occurred chiefly on the first and second day; for, whilst it fell to 442 grs. on the second day, it rose to 508 grs. on the third, which was a treadwheel day. There is not, however, an unexceptionable basis of comparison, since it is very probable that the average of the three preceding days was unduly increased by two of the three having been treadwheel days; and hence it is probable that the diminution in the excretion of urea under the influence of tea is less than is now represented. The average excretion of urine was unchanged.

The urea rose under the action of the coffee 42 grs. daily, and nearly reached the point from whence it fell before the tea had been administered. The quantity of urine remained unchanged.

Professor LEHMANN found that theine increased the elimination of urea; but BÖCKER and HAMMOND state the contrary to be the action of tea, whilst at the same time they affirm that the urinary water is unaffected. The urinary water was observed by LEHMANN, BÖCKER, and HAMMOND to be increased, and the urea to be decreased, in various degrees by the action of coffee.

The alcohol caused a further diminution in the amount of urea to the extent of 26 grs. per day; but it yet remained 14 grs. per day higher than the point to which it first fell with the tea. In each of three regular cases the alcohol prevented the increased elimination which usually occurred from them on the treadwheel days, and rendered the amount on that day 43 grs. less than it had been with rest on the preceding day; but the urea rose on the third day from 466 grs. with the treadwheel labour, and 446 on the Sunday, to 557 grs. on the Monday.

The amount of urine was reduced nearly 20 oz. per day on the average of the whole of the prisoners, and in each of them.

The barometer fell so low as 28·904 in. on the first day of the administration of the alcohol, and would therefore tend to lessen the elimination of urea.

BÖCKER and HAMMOND found that the addition of moderate quantities of alcohol to the ordinary diet lessened the excretion both of urea and urinary water.

Tea, coffee, and alcohol, but particularly the latter, had, in my experiments, the power

of retarding the elimination of urea for one or two days; but on the third day this power ceased. The same was observed of other changes of diet, as, for example, the omission of salt and the addition of fat,—the former causing a diminution of 26 grs. from the amount on the treadwheel day (when only 10 grs. had been added by the treadwheel day), and the latter a loss of 6 grs., although it was a treadwheel day. The increase with the salt, fat, and coffee occurred on the second day; but with tea and alcohol it was deferred until the third day, and even then the normal quantity of urine was not restored with the alcohol.

There was no disturbance of the health during these investigations, except that No. 1040 was once purged with the coffee; and with the alcohol all complained of being lazy at their work and thirsty, and they noticed that they passed less urine and slept more profoundly.

Chloride of Sodium.

Abstinence from the use of chloride of sodium was found by WUNDT to diminish the quantity of urinary water, and to lessen the amount of chloride of sodium excreted with it progressively through a period of five days.

The rate of excretion of chloride of sodium, when three-quarters of an ounce was allowed daily besides that which was contained in the bread and gruel, was, on the average of all the cases, 506 grs. daily. When only that was allowed which was contained in the bread and gruel, the daily emission was reduced to 184 grs. On renewing the full quantity, and adding 3½ oz. of extra fat, the rate increased to 419 grs. daily, which was a less quantity than that which was recorded when the supply of salt had long been unlimited. The quantity was increased 123 grs. daily under the use of tea, and then fell 48 grs. daily with coffee; and a further loss of 142 grs. daily occurred with the alcohol.

The average elimination under these different conditions was as follows:—

TABLE XLIII.

	Unlimited supply.	¾ oz. less supplied.	Full supply, extra fat.	Full supply, tea.	Full supply, coffee.	Full supply, alcohol.
	grs. daily.	grs. daily.	grs. daily.	grs. daily.	grs. daily.	grs. daily.
Chloride of Sodium ...	506	184	419	542	494	352

The very large supply and elimination of chloride of sodium in prisoners is remarkable, the elimination under ordinary conditions being nearly equal to that of urea. The diminution in the excretion, when the supply had been lessened by three-quarters of an ounce, was almost identical with the diminution of the supply. Thus the loss was 322 grs., and the diminution in the supply was 328 grs. As the lessened elimination during the experiment with extra fat immediately followed the last experiment, it may perhaps be doubted whether some portion of the salt supplied had not been retained, to meet the deficiency previously existing; but the increase with tea and coffee, and par-

ticularly with the former, cannot admit of doubt. The very large diminution in the excretion of chloride of sodium under the action of alcohol corresponds with the lessened excretion of urinary water, so that the former fell 27·5 and the latter 28·7 per cent. The diminution was much greater than that of urea, which was only 5 per cent. on the average of the three days.

9. *Relation of the Excretion of Urea to headache, deranged stomach, and a sense of general malaise.*

I was not subject to any serious illness during the year, but on various occasions, for a day or two at a time, I had derangement of the stomach accompanied by headache, and much nervous irritability and depression. The observations which were then made show that this condition was commonly accompanied by lessened excretion of urea and urine, and the relief of it with increased excretion; but the smaller amount of urea was not necessarily much below the average. I have tabulated the observations in the following manner for convenience of analysis:—

TABLE XLIV.

Date.	State of health.	Urine.	Urea.	Urea. Sequence of days.		
				Previous.	Actual.	Succeeding.
June 4.	Headache; stomach derangement.	Much (69·8 oz.)	Lost 20 grs.; more next day...	517	**497**	506
Oct. 18 & 19.	Headache; took aperients...	Less	Less (190 grs.); increased afterwards.	546	**462**	**350**
December 25.	Headache, severe; aperients.	Less (46 oz.)	Much less (208 grs.)	528, 506	**298**	
January 6.	Headache, little	Less (38 oz.), followed by increase (47 and 68 oz.).	Less (51·5 grs.)	503	**451**	
February 25.	Headache	Much (65 oz.)	Less on two following days...	561, 477	**483**	418, 360
July 19.	Headache	Little (47 oz.); increased ..	Low; increased 35 grs. when relieved.	.....	**464**	500
December 9.	Headache; aperients	Less, and then more	Very low; increased afterwards.	380	398	455
February 21.	Food disagreed; fish	Very much (87 oz.)	Increased three days 87·7 grs.	474	**543**	552, 561
Mar. 14 & 15.	The same; chawl	Greatly increased (92·6 oz.).	Much less (163·2 grs.)	574	**460**	**411**, 455
March 6.	Very tired and oppressed ...		Lessened 71 grs.	523	**456**	527
January 17.	Tired; heavy; full; with the thaw.		Increased 56 grs.	495	**552**	444
Feb. 25, 1860.	Full (Sunday)		Increased 77 grs.	502	**580**	471
May 10.	Oppressed		High; fell next day 141 grs., and increased following day.	.....	**595**	452, 505
October 23.	Oppressed; full	50 oz.	Fell 79 grs.; increased next day.	540	**461**	521
May 2.	Irritable; depressed	Very low (25 oz.); had been purged.	Very high, as preceding days, and fell to normal quantity.	713, 684	**545**	528

It is very likely that all the conditions above mentioned were not identical; but they show that in all the attacks of illness which I had the elimination of urea was disturbed. Commonly the elimination of urea was temporarily lessened, and had been above the average amount. The effect upon the urine was not uniform; but the very largely increased elimination of urine, with lessened elimination of urea, in the two instances in which food disagreed were very striking. In every instance of headache the relief was attended by a return to the usual rate of elimination of urea. There was not, on those

occasions, any material change in the quantity of food taken; for aperients and emetics were had recourse to, and relief was soon obtained.

[The years 1861 and 1862, Table I. p. 759 *et seq.*, offer numerous records in which the quantity of urea eliminated was temporarily lessened under the influence of oppressive weather or of internal conditions, causing headache and general malaise. Such are May 14 and 21, and 28 to 31; June 14; July 2 to 5 and 20; October 8; November 3 and 4, 6 and 8; December 6 to 10, 1861; and January 26, 1862.]

The effect of purging over the elimination of urea was commonly inconsiderable; but in one instance there was a great diminution of both urea and urine.

10. *Relation of Urea and Carbonic Acid.*

Scarcely any observations have hitherto been made in reference to the relation of urea and carbonic acid; but BECHER (HENLE'S 'Zeits.' 1855) observed a general relation existing between these two excretions, and particularly in their relation to food, but the two did not move in parallel lines. My inquiries into the evolution of carbonic acid and urea enable me to show some of their relations, as follows:—

1. In both alike the period of production is not identical with that of elimination. Tea and coffee cause a larger elimination of carbon than they supply; and in a paper published in the 'Philosophical Magazine' for December 1859, I showed it to be probable that the immediate action of all food is elimination, whilst the act of production is more remote. Hence carbonic acid may accumulate to a certain point, and be discharged on the application of an efficient cause. So, in like manner, water alone largely eliminates urea, and therefore urea must have accumulated, unless we admit it to be possible that it promotes the production of urea by increasing metamorphosis of tissue.

2. Water has a relation to the elimination of urea analogous to that of air in the elimination of carbonic acid, and an increase of both will equally cause an increase in the elimination.

3. The progression in the rate of hourly excretion of both substances after food is very similar, in so far that the largest excretion follows the breakfast and the tea meals, and the least follows the early dinner; but the proportionate excretion of urea in the middle hours of the day is less than that of carbonic acid. In both the lowest rate occurs in the night.

4. I cannot contrast the effect of season upon these excretions with certainty, since the experiments in reference to carbonic acid were limited to the excretion in the absence of food and exertion, whilst those on urea extended over the whole day, and comprised the whole influence of food and exertion. Hence these are, perhaps, not parallel conditions. Season lessens the production of carbonic acid before breakfast as the summer advances, and the greatest emission occurs in the spring and winter; but the production of urea with ordinary food and exertion increases with the summer, and is the least in the cold season.

5. Temperature, as it increases, lessens the production of carbonic acid, and lessens

it in an increasing ratio; and in like manner the action of barometric elevation was inverse; but in reference to urea with food and exertion both relations are direct, and the urea increases as they increase. The effect upon the elimination of carbonic acid was immediate, but upon the excretion of urea it was not usually evident until the following day.

6. Both excretions are influenced by excess of food, as has been shown by the "basis quantities;" but the influence upon urea was much greater than that upon carbonic acid.

7. Exertion influences the excretion of carbonic acid much more than urea.

8. Hence the relation of urea and carbonic acid is one of a general character only.

11. *The Relation of Urea to Nutrition.*

This is associated with both the ingestion and the egestion of nitrogen.

The Ingestion of Nitrogen.—The ruling theory in reference to the ingestion of nitrogen is, that the elements of food are absolutely divisible into two classes, and that the nitrogenous are expended in repairing nitrogenous tissues, and the hydrocarbons in supplying heat. But I proved in my former paper on carbonic acid *, on the one hand, that starch and fat, apart from nitrogen, do not increase the emission of carbonic acid (the accepted evidence of the activity of the heat-forming function), and, on the other, that nitrogenous foods do increase the emission of that substance, and are thereby respiratory and heat-forming excitants. It had also been shown by several eminent chemists, that the fattening properties of fodder are in proportion, not to the carbon, but to the nitrogen which it contains; and thence I infer, in accordance with my experiments, that, in the experiments on animals, the fat was due in some way to the nitrogen. It is therefore, I think, impossible to defend this theoretical division of foods, and nitrogen should probably be regarded both as an element of tissue and an excitant of vital actions.

The Excretion of Nitrogen.—The views in reference to the excretion of nitrogen are three:—

1. That of Liebig and his pupils, as Bischoff and Voit—that the excretion of urea is in proportion to the metamorphosis of tissue, and is as the bulk of the tissue, the nitrogenous fluids contained in the tissue, and the oxygen supplied, or, in other words, as the wants of the tissues, the supply of nitrogenous food, and the activity of the vital processes. They affirm that in every condition in which the elimination of urea is increased there is increased vital action; and the following Table, derived from my own experiments, shows that even the ingestion of fluid on a day of entire abstinence from food causes temporary increase of pulsation.

* Philosophical Transactions, 1859.

TABLE XLV.—Showing the effect of Water drunk during fasting over pulsation and respiration.

	Water.			Water.				Water.		Water.		
Hour.	9.	10.	11.	12.	2.	3.	4.	5.	6.	7½.	10.	10½.
Pulsation ...	67	64	64	69	72	64	60	64	71	70	73	72
Respiration...	14	...	...	12½	13	...	...	13	...	13½	13½	

I believe that this statement is true.

2. That of BIDDER and SCHMIDT, FRERICHS, and LEHMANN—that the excretion of urea results chiefly from excess of food, and represents "luxus consumption," and has no *necessary* relation to tissue action or tissue metamorphosis. This is based upon the fact that with excess of food, and with certain kinds of food, as gelatine, there is an almost immediate increase in the elimination of nitrogen; and this is abundantly supported by the experiments of LAWES and GILBERT, and by those recorded in this paper.

3. That of LUDWIG and FUHRER—that urea is due neither to the immediate changes from food nor to changes from tissue, but to the destruction of blood-cells. This view is, I think, of little value, since it simply indicates, in a general manner, that the urea is due to activity of the vital functions.

The first view is in accord with LIEBIG's theory of the use of nitrogen, and refers essentially to the *formation* of urea; whilst the second view, regarding the close connexion of excess of food with urea, refers chiefly, or at least in great part, to the *elimination*, and only by theoretical reasoning to the *production* of urea; and as the acts of production and elimination are not identical in nature or time, the two sets of views probably do not refer to parallel actions, and may both so far be true.

It appears to me that there is much truth in both of these views, and that the difference is very much more one of terms than of things, as I will now endeavour to show.

An idea which leads to divergence of opinion in reference to the first view is that connected with tissue-change, representing the tissue as something solid, and quite apart from the circulating mass. But a muscle, besides its tissue framework, and perhaps its fat, consists of fluids which are in the closest relation to the circulating fluid, for there is a perpetual interchange proceeding between them; and in reference to nutrition, the fluids in a tissue (and therefore nearly the whole of the soft tissues) are truly parts of the circulating fluid.

Another idea, having a similar tendency and connected with the second view, is that which limits the changes proceeding in the blood from food, as if they were apart in space, and different in kind from the changes which proceed in the tissues; but the perpetual circulation of the blood through every part of the body, which is completed every few minutes, makes this a distinction almost without a difference, for at every circulation of the blood it receives from and distributes to every tissue.

If, therefore, we consider the tissue-fluids as a part of the circulating medium, and the products of food as perpetually circulating amongst the tissues, we take away the distinctive peculiarities of these two views.

All experiments have shown (and none more clearly than those of BISCHOFF and VOIT) that there is the closest correspondence between the amount of nitrogen which has entered the blood from food and that excreted as urea in flesh-feeding animals, and that the former might in these animals really measure the latter. In man and other animals there is a small elimination of nitrogen by other sources, as certain organic compounds passing off by the skin and lungs, by the uric acid, creatine, and hippuric acid, by which the urea in the urine does not fully represent the nitrogen contained in the food which has entered the blood; but, on the whole, as it does not probably exceed 5 grs. of nitrogen in man, or its equivalent of about 11 grs. of urea, we need not consider it in the following observations. The prison experiments recorded in this paper, when compared with those on myself, explain the cause of some discrepancies in the results heretofore obtained. In prisoners the urea was lessened on Sundays, when, with no change of food, there was no exertion, and thereby there was relatively an increase of food; whilst in myself there was great increase in the urea, with no exertion and a small increase of food. Two explanations offer themselves:—1st. I am over-fed, so that the full bulk of my tissues is at all times maintained, and therefore I most readily show the effects of increase; but they are rather under-fed, and their tissues have not therefore their full bulk, and hence they appropriate increased food, and do not show the full effects of increase. 2nd. In the absence of the powerful stimulus of violent exertion, the assimilation of food was lessened in the prisoners, and a less proportion of the food was admitted into the blood. Urea is a measure of exertion only so far as it measures food, if the nitrogen of the food be sufficient to maintain the vital actions; but if the tissues lose weight, the excess of nitrogen thus evolved will be added to the urea. If during muscular action there be waste of nitrogen in the muscle, there will be also appropriation of nitrogen by the same muscle, and the equilibrium will be maintained. If the appropriation proceed *pari passu* with the waste, there will be as much nitrogen consumed from the food as is excreted by the tissue, and the final quantity of urea will be always unchanged; and if the waste should exceed the appropriation during exertion, and appropriation exceed waste in the intervals of rest and at night in the same proportion, the balance will be maintained, and the amount of urea eliminated in the whole twenty-four hours will be unchanged. Hence, with sufficient food, there cannot be any increased elimination of nitrogen from exertion, unless the tissue weight be lessened, and the urea will, *de facto*, represent the nitrogen taken into the blood with the food. On the other hand, if the bulk of the muscles increase with sufficient food, there will be a loss in the excretion of nitrogen, because there has been an appropriation of it.

Hence the evident relation of urea must be with food, although it may be derived partly from tissue and partly from food; and the precise correspondence in quantity will be regulated by the relation of appropriation to excretion of nitrogen in the tissues. As the urea is derived from various sources, it is impossible that it can measure either tissue-waste or food separately.

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The true measure of exertion, and therefore, by inference, of tissue-waste also, is the evolution of carbonic acid, which, as I showed in my former paper, is increased immediately, and in a definite and almost proportionate degree by every degree of exertion, whilst no such relation can be traced between exertion and urea.

Hence I venture to submit—

1. That nitrogen is essential to vital transformation as well as to the constitution of certain tissues, and that all in excess of the latter quantity is after a short period cast out of the system.

2. That when the bulk of the tissue is maintained, urea represents nearly all the nitrogen which has entered the blood from the food; when the bulk of the tissue is increased, the urea represents the nitrogen in the food minus the nitrogen gained by the tissues; and when the bulk is lessened, the urea represents the nitrogen supplied by the food plus the nitrogen lost by the tissues; and as in a well-balanced adult system the tissues maintain a standard bulk with some uniformity, the first is the representative condition of urea.

3. That commonly the urea varies as the food.

4. That whilst it is probable that all vital transformations take place in connexion with tissues, it does not follow that vital changes (including the production of urea) do necessarily imply interchange of tissue-elements (solids).

5. The period and amount of elimination of urea is dependent upon the existence of water within the body in excess of that necessary to maintain the due bulk of the tissues and the blood. The urea may therefore accumulate, or, on the other hand, it may be rapidly eliminated. The bulk of the body regulates the *emission*, but the supply of food and the activity of the vital actions regulate the *production* of urea.

6. The effect of temperature and atmospheric pressure is probably that of food absolutely or the relation of food to the vital actions in the *production* of urea, whilst it is that of the statics of the body in relation to the *elimination* of urinary water and urea.

7. That daily variations are not necessary evidences of variations in the formation of urea. They are lost on a long average by a compensating power, which controls the emission of fluids.

Résumé.

The following is a *résumé* of the principal facts which are contained in the preceding communication:—

1. The inquiry was intended to show the changes normally proceeding in a healthy and abundantly fed system, under the ordinary and varying conditions under which men live in the course of the year, and also the effect of treadwheel [and other] labour upon prisoners who are well fed, but have no surplus nutritive material in them.

2. The inquiry upon myself was continued during [two years and two months, and embraced 635 days, and more than 1400 analyses for urea.]

3. The average daily evolution of urea was 519 grs. [on the average of two years 500 grs.], and the proportion to each lb. of body-weight was 2·73 grs. The extremes were 298 and [876] grs. daily, and 1·56 to [4·38] grs. per lb. of body-weight.

The quantity of between 400 and 500 grs. daily was found in 46 per cent., and between 400 and 600 grs. daily in 85 per cent. of the whole.

The weekly averages varied from 428 to 715 grs. daily, and the monthly from 451 to 665 grs. daily.

The average daily excretion of urinary water was 53·1 fl. oz. [on the average of two years 51·2 fl. oz.], or ·28 fl. oz. to each lb. of body-weight. The extremes were 23·5 and 92·67 fl. oz. daily, or ·123 to ·487 fl. oz. to each lb. of body-weight.

The quantities of between 40 and 60 fl. oz. were found in 52·7 per cent. of the observations, but those between 40 and 50, 50 and 60, and 60 and 70 oz. were found respectively in 26·2, 26·5, and 22·4 per cent.

Large quantities were always followed, or immediately preceded, by small ones. Daily alternations in quantity were observed. Waves of increase and decrease, or *vice versâ*, were common; and sometimes there was a progressive increase or decrease, or the quantity remained very high for some days. The daily variations were very great.

4. The relation in the quantity of urea and urinary water varied much; but with increasing decades of ounces of urine there was increasing quantity of urea. The average quantity of urea in 55 oz. of urine passed daily was 9·4 grs. to each ounce; but with 25 oz. it was double that amount per ounce, whilst with an equal quantity in the direction of increase, viz. from 55 to 85 oz. per day, the decrease in the urea per ounce was only 25 per cent. Hence, from a medium standard, the quantity of urea in an ounce of urine decreases much less with increase of urine than it increases with decrease of urine.

5. The average amount of urea per hour, in the twenty-four hours, was 21·7 grs., whilst that of the night was only 16·5 grs., and the basis quantity was 20·3 grs.; but that passed to midday was 25·5 grs. The average of the whole day being the standard, the decrease at night was 24 per cent., and of the "basis quantity" 6·4 per cent., whilst the increase to midday was 17⅓ per cent.

The greatest number of exceptions occurred with the "basis quantities;" so that, the higher the "basis quantity" on the whole average, the greater was the amount of urine evolved on the preceding day.

The "basis quantity" cannot be used as a measure of the total quantity of urea on the same day.

There were two maxima of elimination of urea and urine, at 1 P.M. and 9 P.M., after breakfast and tea, (the former being the highest,) and an intervening low period from 2 to 5 o'clock after dinner.

In the examination at each quarter of an hour, the maximum emission of 54·6 grs. of urea and 13·5 oz. of urine occurred at 12¾ P.M., three and a quarter hours after breakfast. There was a gradual ascent to, and descent from, the maximum; but during the hour of maximum elimination the increase was proportionately greater, and in one instance was at the rate of 21 oz. per hour.

6. The relations of urinary water to period of the day are similar to those of urea. The average rate of the whole day was 2·21 fl. oz., of the night 1·19 fl. oz., the "basis quantity" 2·65 oz., and to midday 4·41. The decrease of the night and the basis quantity from that of the whole day was 46 and 7·2 per cent., and the increase to midday 100 per cent.

The lines representing the quantity of urea eliminated per hour during the day, and those in each fluid ounce of urine are opposed, and the most so in the morning, but they meet during the night.

7. When, during a fast from food, water is taken at the usual meal-hours during the day, it causes an elimination of urea and urinary water, precisely as if food had been taken, both in degree and progression. There were two maxima after the breakfast- and tea-hours, but no increase after the dinner-hour. The maximum was 34·5 grs. of urea, and 11½ oz. of urine in one hour and a half after having drunk the water at the breakfast period; and the minimum was 5·3 grs. of urea, and 1·36 oz. of urine at 5 P.M., before the tea hour.

With water taken at 8¾, 9¾, and 11 A.M., without any food, and the urine passed every quarter of an hour, there was a maximum elimination of urea in three-quarters of an hour after the first and third, and in half an hour after the second dose. The maximum elimination of urine occurred in one hour after the first, and in three-quarters of an hour after the second and third doses. The three maxima were, urea 35·14, 35·22, and 32 grs.; urine 21·6, 21·2, and 19·4 fl. oz. per hour.

When 8 oz. of water were taken alone at about 9 A.M., the maximum elimination was in one or one and a half hour; but when 3 oz. of bread, or 2 oz. of gluten bread were added, it did not occur in less than three hours. When tea or coffee were added, it occurred in one and two hours, and in two hours after taking black dose.

The maximum increase of urea within three hours from water was from 65 to 104 per cent., but from tea and coffee it was 59 and 58 per cent. The increase in the urine varied from 260 to 640 per cent. with water, 200 per cent. with black dose, and 200 and 350 per cent. with coffee and tea, the "basis quantity" being the standard.

8. 6 oz. of alcohol and water (1½ to 4½ oz.) caused within two hours an increase of urea from 38 to 108 per cent., and of urine from 246 to 554 per cent. The increase of the two was not parallel.

9. With a diet of 34 oz. of bread and 48 oz. of water alone, and then with 300 grs. of tea, and again with 2 oz. of coffee added, the hourly progression was the same as with ordinary food, except that with tea and coffee the maximum elimination was reduced, and the quantity evolved late at night was largely increased; in other words, the elimination of urea was deferred some hours. The maxima of urea were 29·3 grs., 24·6 grs., and 25·4 grs., with water, tea, and coffee, in their order. The total quantity of urea evolved was 442, 467, and 493 grs. daily, in the same order.

10. The largest average elimination of urea occurred on Sunday, with some increase of food and rest, in myself, in whom there is commonly an excess of nutritive material; but the least elimination was then found in the prisoners without change of food and with rest, in whom there is, with a good dietary, a tendency to defect of food; and then also an increased quantity of nitrogen was found in the fæces. In myself the excess of urea was 14·4 grs. when compared with all the days of the year, and 67·9 grs. when compared with 191 days on which there was no known cause of variation. The daily excretion on Sundays of between 700 and 800 grs. was 3·4, and between 600 and 700 grs. 13·1 per cent. more frequent than on all days combined. The elimination of urea decreased

daily to the end of the week, with regular work, from 574 grs. on Sunday, to 460 grs. on Friday; but this varied with the amount of labour and the dependent conditions.

11. The weight of the body after the emission of urine was the greatest on the Sunday, including the Sunday night, and increased from the Saturday from 1 lb. 6 oz. to 2 lb. 3 oz. It decreased progressively during a week of regular labour, but increased on any day with rest.

12. The solid ingesta, on the average of four months, was between $36\frac{1}{2}$ and $37\frac{1}{2}$ oz. on each week day, but on Sunday it was $41\frac{3}{4}$ oz. The total solid and fluid ingesta were $95\frac{3}{4}$ oz. on Sunday, and from $89\frac{1}{4}$ oz. to 95 oz. on the week days.

13. The quantity of urea eliminated increased in the summer; and, on the average, from May to October inclusive it was 570·1 grs., and from October to April inclusive 480·5 grs. daily. The increase from March to the maximum in August and September was 36 per cent. of the former; and the succeeding decrease to March was 29 per cent. of the maximum. The difference between the maximum and minimum was 46·1 per cent. of the latter.

14. The elimination of urinary water followed the same course, and was 55·7 fl. oz. in the warm, and 51·9 fl. oz. in the cold half of the year. The excess of the maximum over the minimum quantity was 40·2 per cent. of the latter.

15. The elimination of urea increased with increase of temperature and barometric elevation, but it occurred usually on the second day. When the direction of the two influences was different, they neutralized each other. The proportions of urea to one degree of temperature, in the two seasons of the year, were nearly identical, viz. 10·36 grs. in the hot, and 10·9 grs. in the cold season. The months of marked increase and decrease of temperature were those of marked increase and decrease in the elimination of urea. The proportions of urea to one degree of temperature in the two highest and the two lowest months were the same, viz. 11·5 and 11·8 grs.

The elimination of urine increased with sudden cold, and decreased with sudden warmth on the same day, and then oscillated to maintain the necessary equilibrium of the fluids in the body.

16. [There are variations in the quantities according to the conditions of each year; but the seasonal-conditions progression remains the same. It is now important to ascertain the influence of the several conditions attending a temporary summer residence at the sea-side, in reference to the increase of urea which then occurs.]

17. With the labour of the treadwheel there was no increase in the elimination of urea in one hour and ten minutes before breakfast, but an average decrease of 2·4 grs. per hour. There was an increase on the whole day over the day of routine labour of 16 grs., and over the Sundays of 34 grs. per day. The increase of the former was 3 per cent. The prisoners lifted from 354·81 to 413·89 tons through 1 foot per day, with treadwheel labour. The increase occurred on the treadwheel day alone, except in a few instances, when, with meteorological disturbances, it was deferred to the second day in two cases; and then by the necessary alternations the largest excretion occurred on the days of rest, viz. on the alternate days.

18. The rate of excretion of urea to each lb. of body-weight varied only from 4·39 to 4·74 grs., and was 59 per cent. greater than in myself.

19. [The labour of cocoa-matting weaving, as compared with that of tailors, caused the weavers to consume more bread, milk, and water; to lose weight; to emit more urine, urea, chloride of sodium, and fæces, and consequently more nitrogen; to exhibit much less diminution in the amount of urea evolved on the Sunday, and a little less urea to body-weight.]

20. The elimination of urea was always increased after additional and unusual food, as at dinner and evening parties; and the increase sometimes continued for some days.

The addition of extra fat to the dietary of the prisoners did not vary the amount of urea or urine. Tea, coffee, and alcohol largely decreased the urea on the first and second day, but not on the third day. Tea and coffee did not vary the amount of urine; but alcohol lessened it nearly 20 oz. per day.

21. Chloride of sodium was largely eliminated under the influence of tea, and as largely retained under that of alcohol; and in the latter it corresponded with the reduction of the urine, viz. 27·5 and 28·7 per cent. When $\frac{3}{4}$ oz. of salt was withdrawn from the dietary, there was that precise amount deficient in the urine.

22. With headache and stomach-derangement there was commonly lessened excretion of urea, followed by increased excretion on recovery.

23. Urea and carbonic acid accumulate in the system; and the period of production is not that of elimination. Food first causes increased elimination of the stored-up excretion, and then supplies material to be transformed.

The hourly rate of excretion of both is very similar with food; but there is no increase of urea after early dinner. The effect of season, temperature, and pressure of the atmosphere is reversed if the *total daily emission* of urea may be compared with the *basal quantities* of carbonic acid.

24. The direct relation of urea is with food, and, under certain conditions, varies precisely as the food if the tissue-weight remains unchanged; but when the latter varies, the relation of urea to food will vary in the same proportion.

25. Exertion cannot cause increase of urea over the corresponding quantities of nitrogen contained in the food, unless the muscular bulk be lessened. Commonly the reparation and the waste proceed *pari passu*, or the reparation is in excess during the periods of rest, and restores the equilibrium on a long average. When there is deficient muscle-bulk, increase of food does not induce a corresponding increase in the urea, but a part is retained by the muscle.

The following are the points to which I desire more particularly to direct attention:—

1. That every question connected with urea is one of great complexity.

2. That observations made at different hours of the day, and in different seasons of the year, cannot be truthfully compared.

3. That no inquiry less than that involved in the collection of the whole urine evolved in the day can determine the total daily elimination of urea.

4. That, as elimination and formation of urea are different acts, but incapable of separation, the influence of agents exerted over the production of urea must be examined on many successive days.

5. As urea is a mixed product from food and tissue, it cannot in any case (even in prolonged fasting) be taken as an absolute indication relative to the one or the other.

6. As the relations of urea are chiefly with food, and, other things being equal, urea, with the nitrogen in the fæces, will measure the amount of nitrogen contained in the food, its chief value in health and under ordinary conditions is in indicating the amount of food which has entered the blood as distinct from that which has passed through the bowel. In fasting, it will measure nearly the whole of the loss of nitrogen sustained by the tissues, minus an unknown amount which the tissues have received from the albuminous elements of the blood.

7. The relation of urea to weight of body is indefinite and intermediate, since the relation is true only so far as weight of body may represent the consumption of nitrogenous foods.

8. It is probable that the determination of the amount of urea eliminated under the ordinary conditions of life, is much less valuable than is at present supposed.

9. The relation of period of the day to the elimination of urea is that of fluids to the solids in food, and the necessity for fluid in the body, and is one of *elimination* chiefly, whilst the relation of season of the year and its coexistent circumstances is with the solids in food and the *production* of urea.

10. The appetite for food, and the amount of nitrogen contained in the food, chiefly affect the *production* of urea, whilst the statics of the body, and their relation to atmospheric pressure and to temperature, by regulating the quantity of fluid to be retained and emitted, affect the *elimination* of urea. Certain fluids, and perhaps solids, have the power of retarding the elimination of urea, by causing a retention or accumulation of fluids in the body.

11. The alternations in the quantity of urine emitted on succeeding days are due chiefly to atmospheric influences, acting with, or in opposition to, the statics of the body.

12. The excretion of urea is not affected by purging, except so far as the supply of food may be varied, and fluid be emitted by the kidneys in less quantity than is usual.

13. The relation of urea to such disordered states of the system as are found in derangements of the stomach and in headache is probably less than that of fluid retained; for relief is found rather with the emission of fluid by the bowel or by the kidneys, than with any remarkable and sudden increase in the elimination of urea.

14. The production of carbonic acid is the best measure which we have of the activity of the vital functions attending muscular exertion.

15. The variations in the amount of urine excreted at different seasons of the year are different from what (and not so great as) has been stated, when the amount of fluid drunk is not limited, and the inquiries extend over a lengthened period.

16. It is now most desirable that inquiries should be directed to determine the relative amount of nitrogen in the food which enters the blood to that which remains in

the bowel. It is probable that this would elucidate many states of disease, and explain some of the anomalies which still exist as to the relation of urea to food.

Explanation of the Plates.

PLATE XXXII.

Figs. 1 & 2. Examples of alternations and waves in the daily elimination of urinary water.

Figs. 3 & 4. The daily quantity of urea, and the amount of urea in each fluid ounce of urine with increasing decades of ounces of urine.

Fig. 5. Monthly averages, during two years, of the daily elimination of urea and urinary water, with the atmospheric temperature and pressure.

Fig. 6. Relations of atmospheric temperature to the elimination of urea on the same day and the succeeding day.

Fig. 7. Amount of urea excreted on each day of the week.

PLATE XXXIII.

Daily observations, throughout the year, on the excretion of urea at four periods of the day, compared with the mean daily temperature at Greenwich.

PLATE XXXIV.

Fig. 1. Hourly elimination of urea and urinary water, and the quantity of urea in each ounce of urine.

Fig. 2. Hourly elimination of urea and urinary water at each quarter of an hour.

Fig. 3. Hourly elimination of urea and urinary water, and the rate of pulsation during a day of fasting, except from water, which was drunk at four periods of the day.

Fig. 4. Daily weight which, with a constant of 13 stones, represented the weight of the naked body.

Fig. 5. Hourly elimination of urea and urinary water under the influence of water, tea, coffee, and bread only.

Fig. 6. The effect of drinking water before breakfast upon the quantity of urea and urinary water eliminated at each quarter of an hour.

Fig. 7. Relation of atmospheric pressure to the elimination of urea on the same day.

PLATE XXXV.

Urea evolved by four prisoners on days of treadwheel, light labour, and Sundays, compared with the mean daily temperature at Greenwich, with ordinary and special diets.

PLATE XXXVI.

Contrast of the daily quantities during two years in the elimination of urea and urinary water, and the atmospheric temperature and pressure at Greenwich, with the daily weight of the body during one year.

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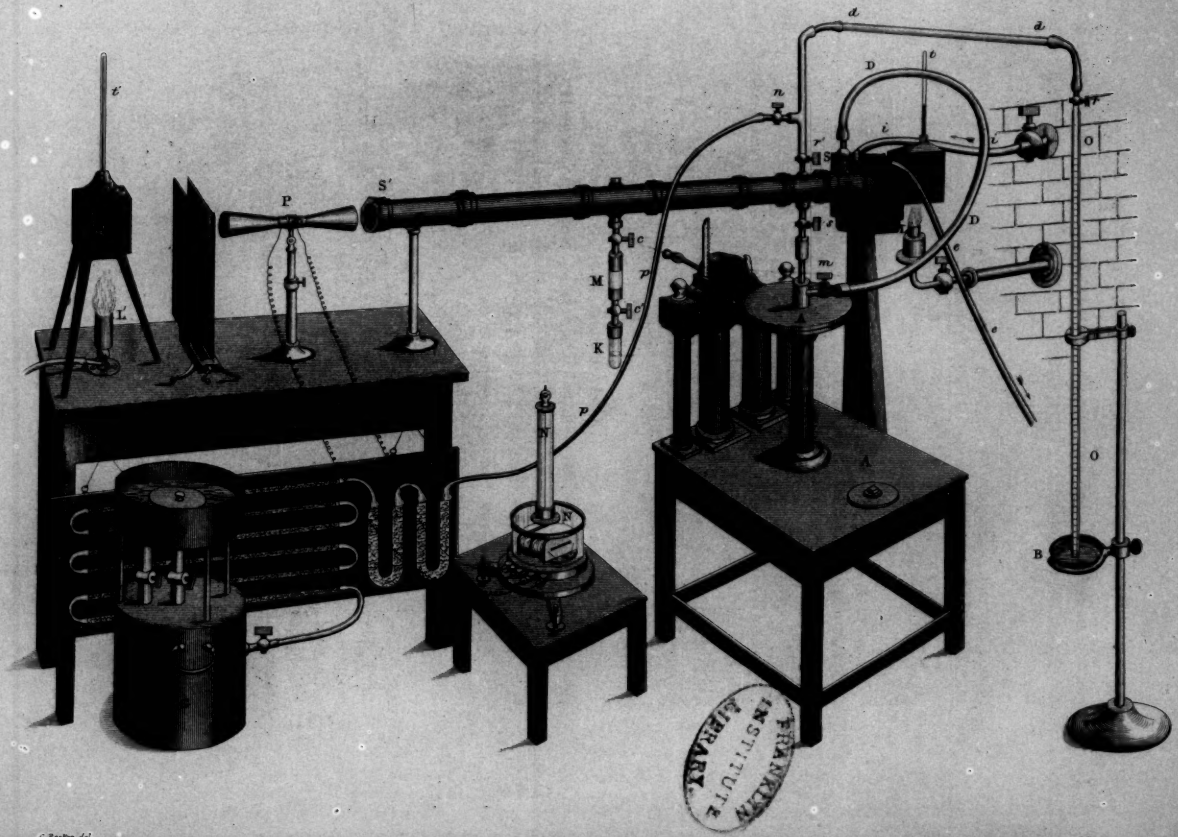

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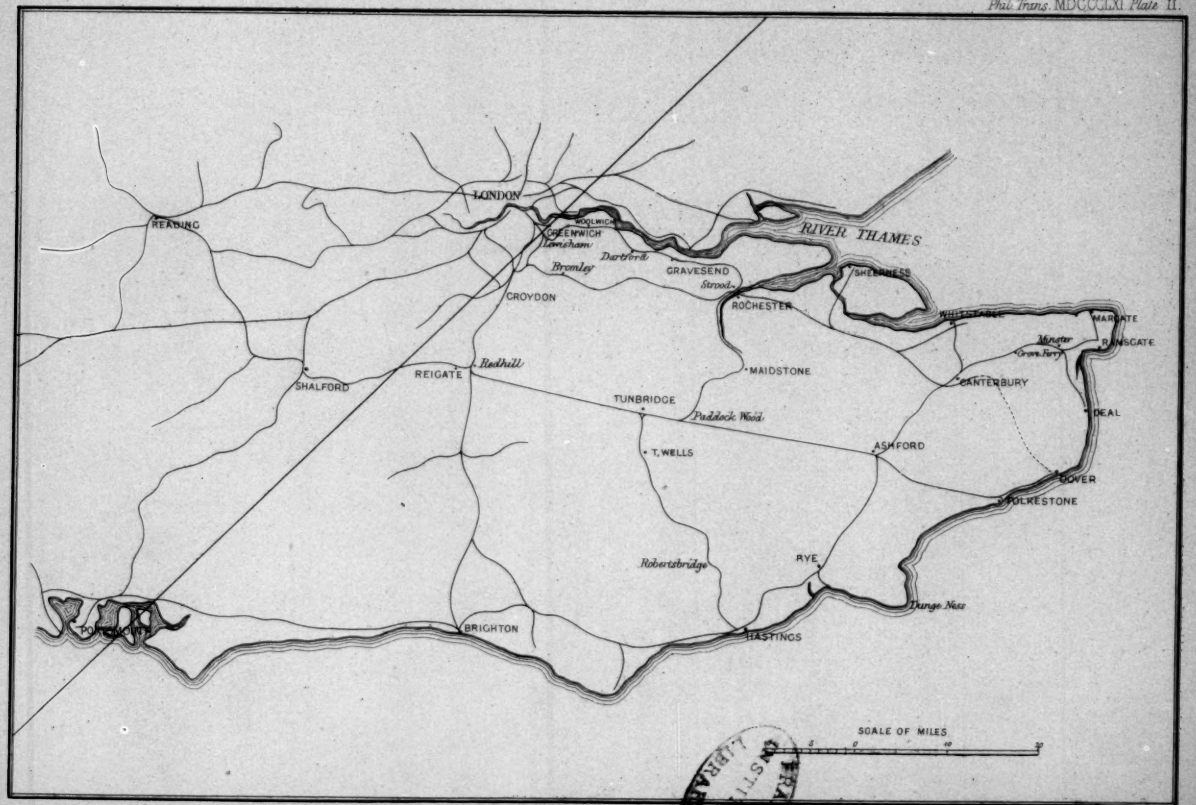

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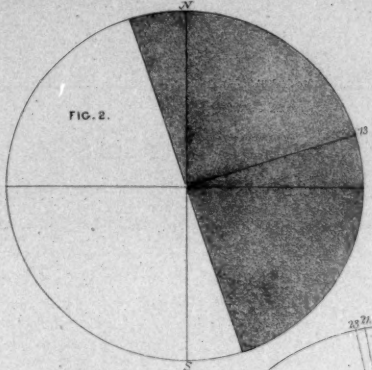

FIG. 2.

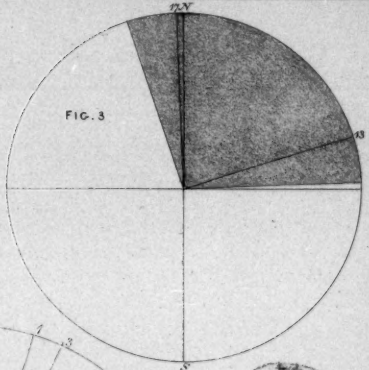

FIG. 3

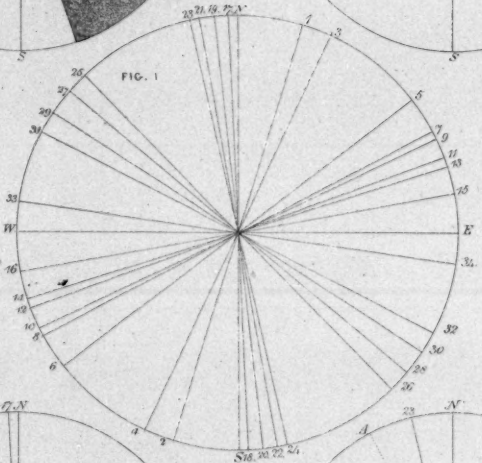

FIG. 1

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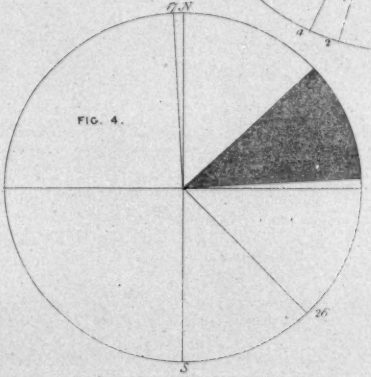

FIG. 4.

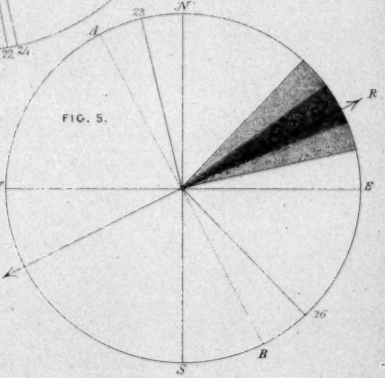

FIG. 5.

J. BAXTER LITH.

Fig. 6.

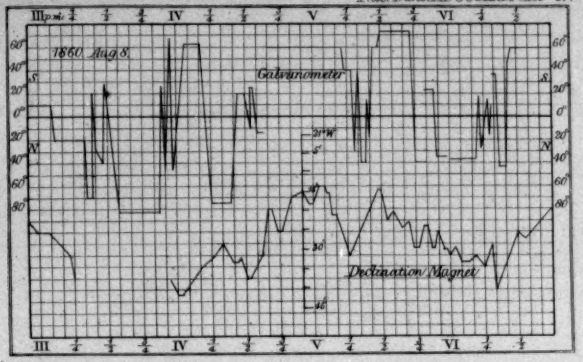

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Fig. 7.

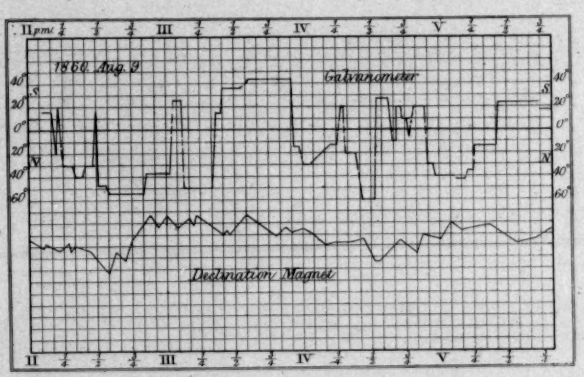
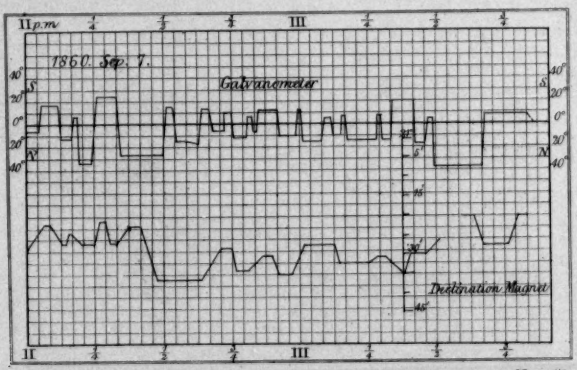


Fig. 8.

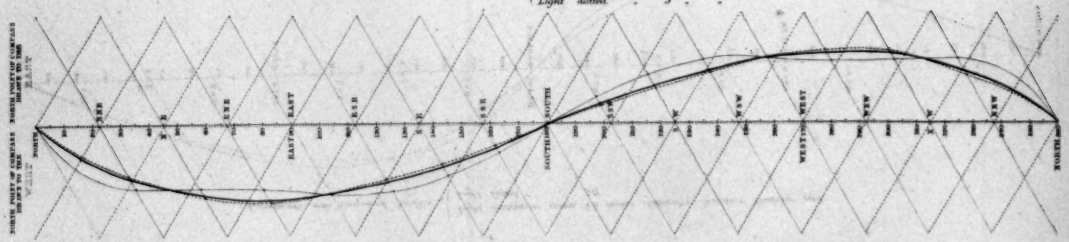


J. Basire sc.

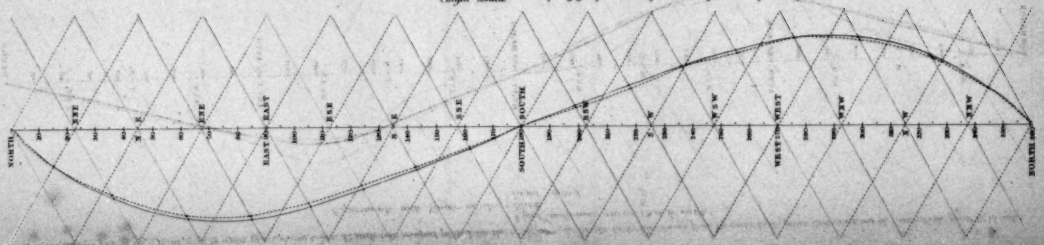
MAGNET ENDWAYS.

Compass needles placed on a fixed pivot in centre of revolving table: So: pole of 12 inch bar magnet 18.25 inches from the pivot. 2.75 inches below its level.

Experiments with Single Needles { Light continuous curve. 12 inch needle / Black . . 6 . . / Light dotted . . 3 . . }



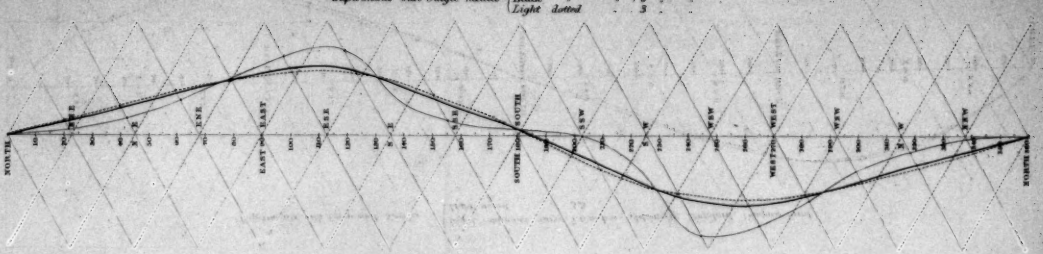
Experiments with Compound Needles { Light continuous curve. 7.6 inches. Admiralty Standard Compass card. / Light dotted . 3.8 . . . . . }



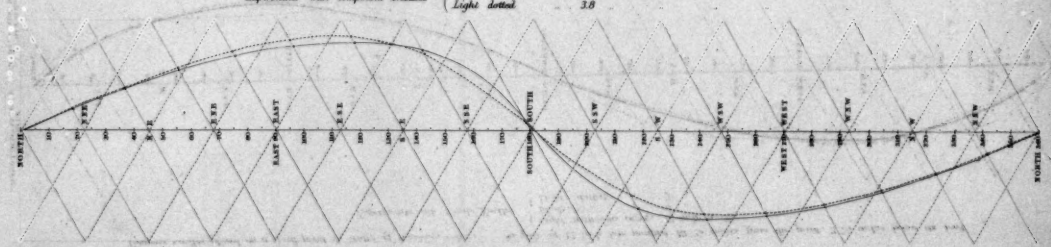
MAGNET SIDEWAYS.

Compass needles placed on a fixed pivot in centre of revolving table. 12 inch bar magnet placed on the table at right angles to the line drawn from its centre to the pivot. distance from the centre to the pivot 18.25 inches.

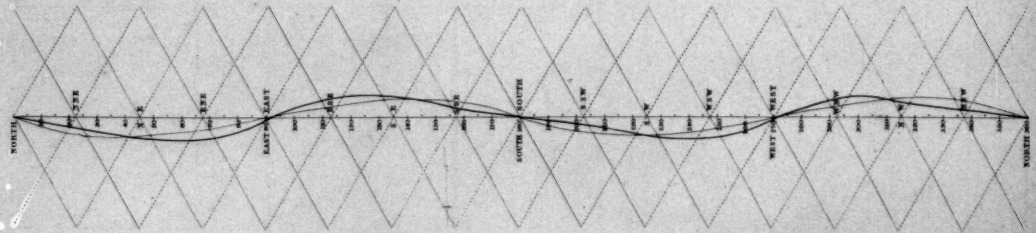
Experiments with Single needles { Light continuous Curve. 12 inch needle / Black . . . 6 . . / Light dotted . . 3 . . }



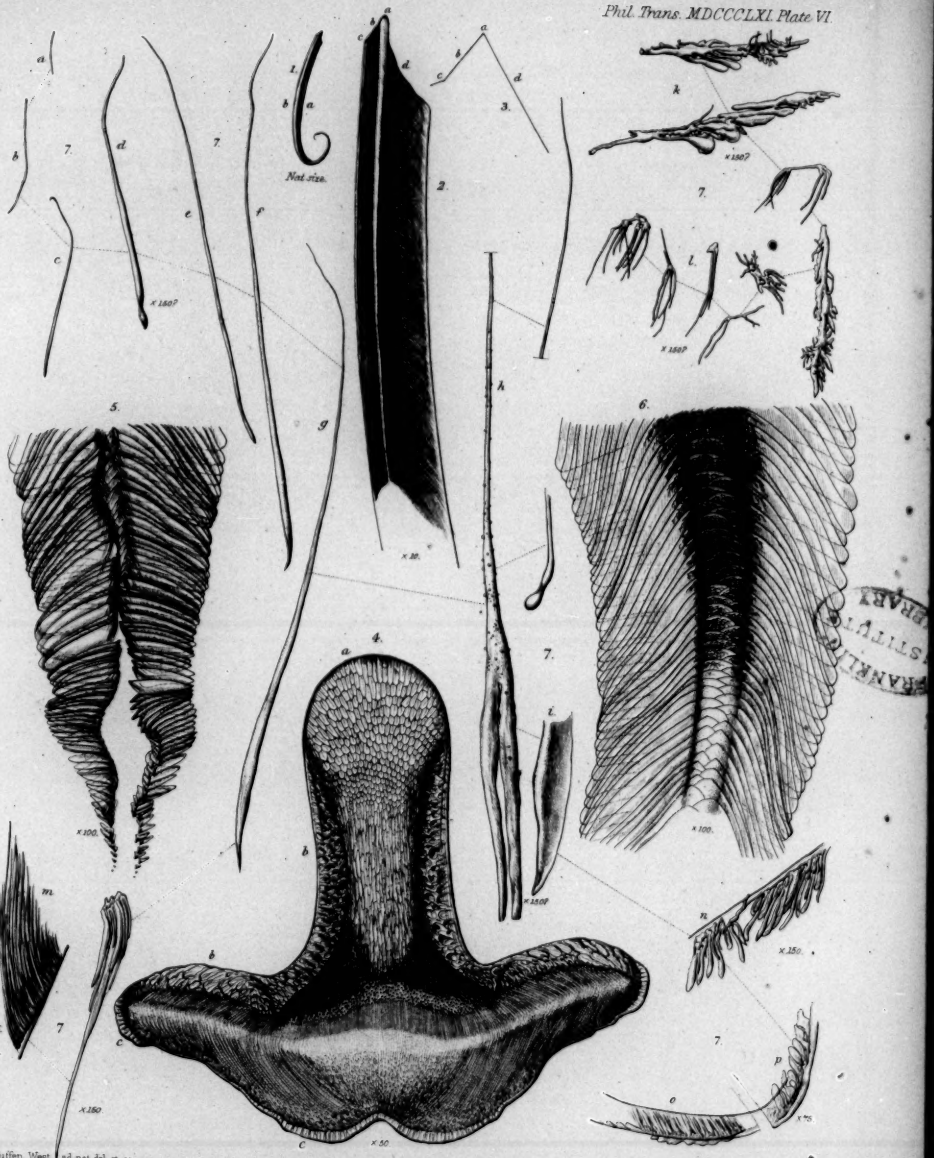
Experiments with Compound Needles { Light continuous curve, 7.6 inches Admiralty Standard Compass card.
Light dotted 3.8



SOFT IRON CYLINDERS.



Cylinders directed towards centre of Compass ends of Cylinders placed 46 inches from end of needles or in same position as adopted for correcting the Quadrantal deviation of an Iron Ship.
Length of Cylinders including hemispherical ends 12 inches diameter 3 inches.
Light Curve, Compound needles Admiralty Stand.d Compass card 7.6 inches. — Black Curve, Single bar needle, 7.5 inches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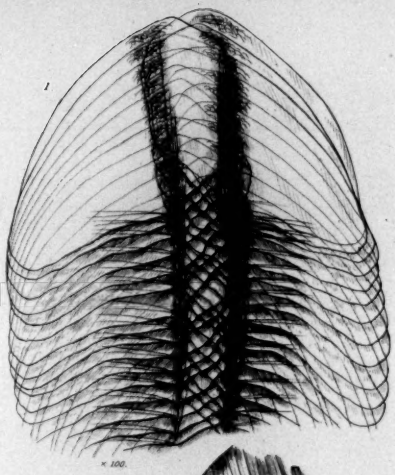

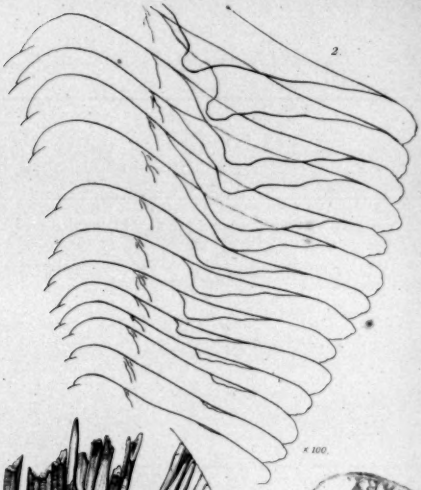

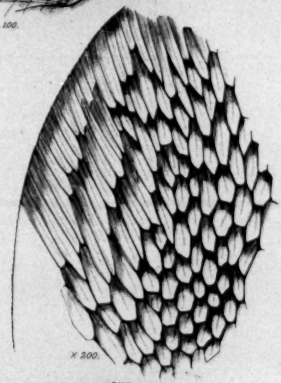

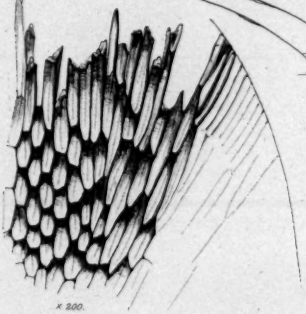

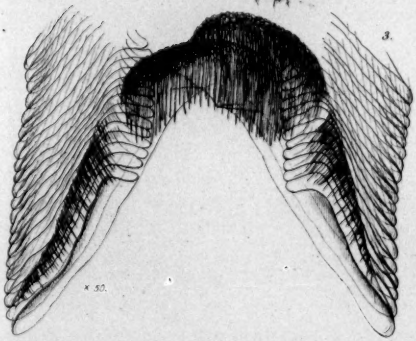

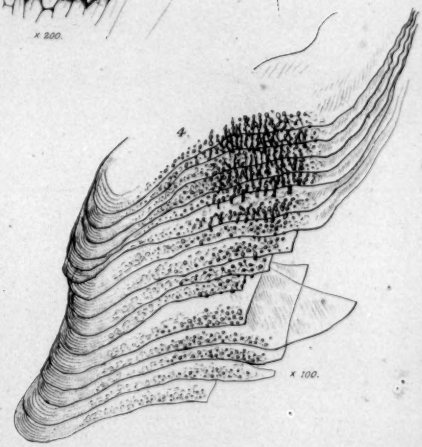

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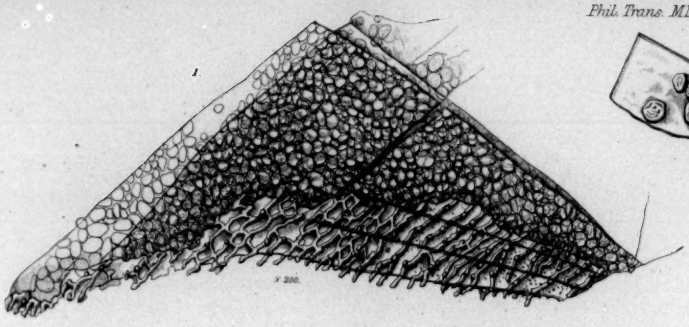

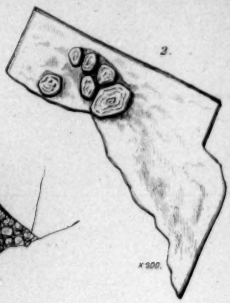

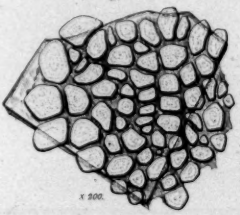

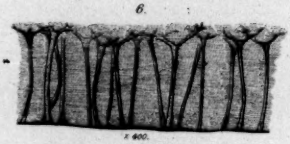

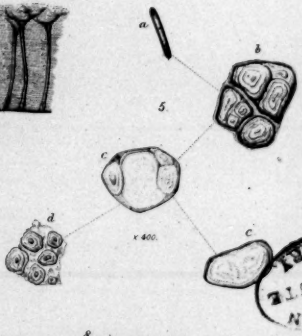

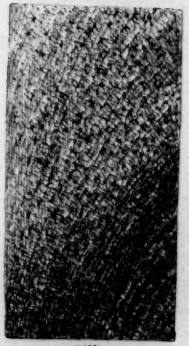

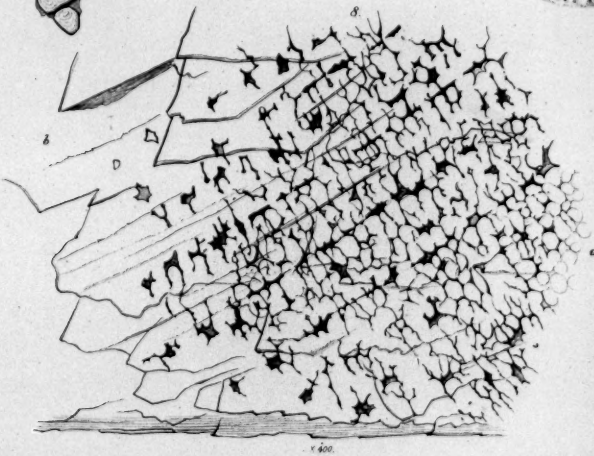

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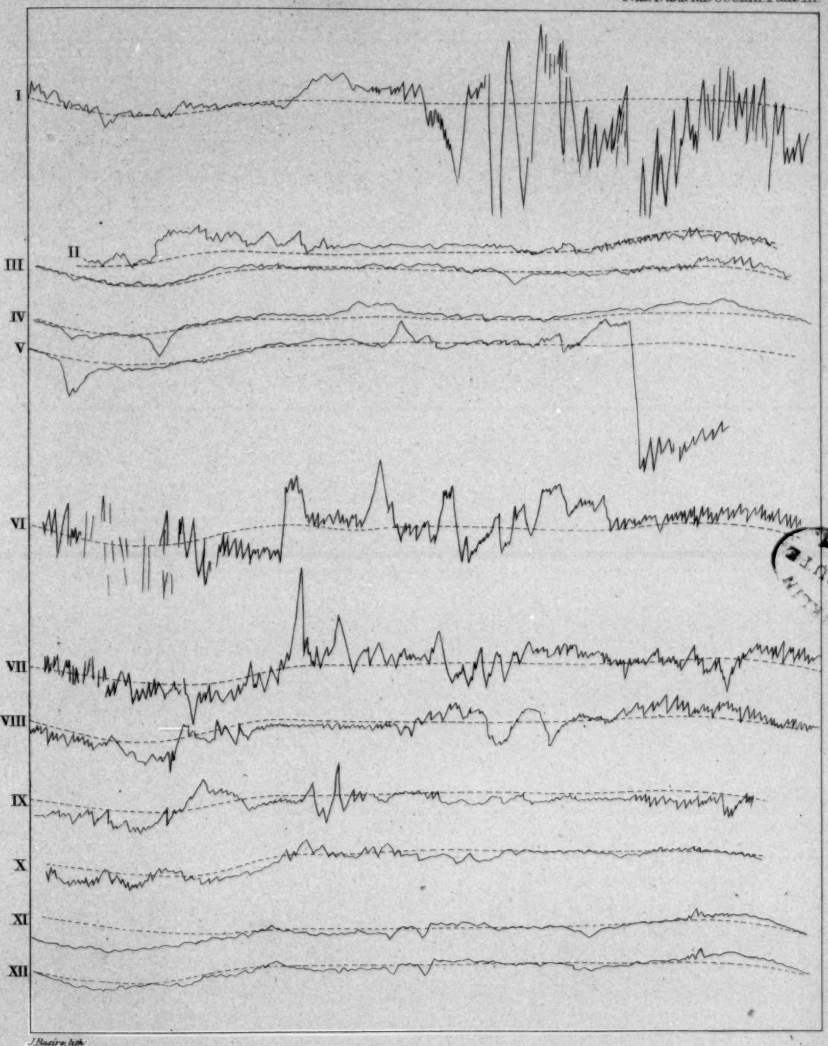

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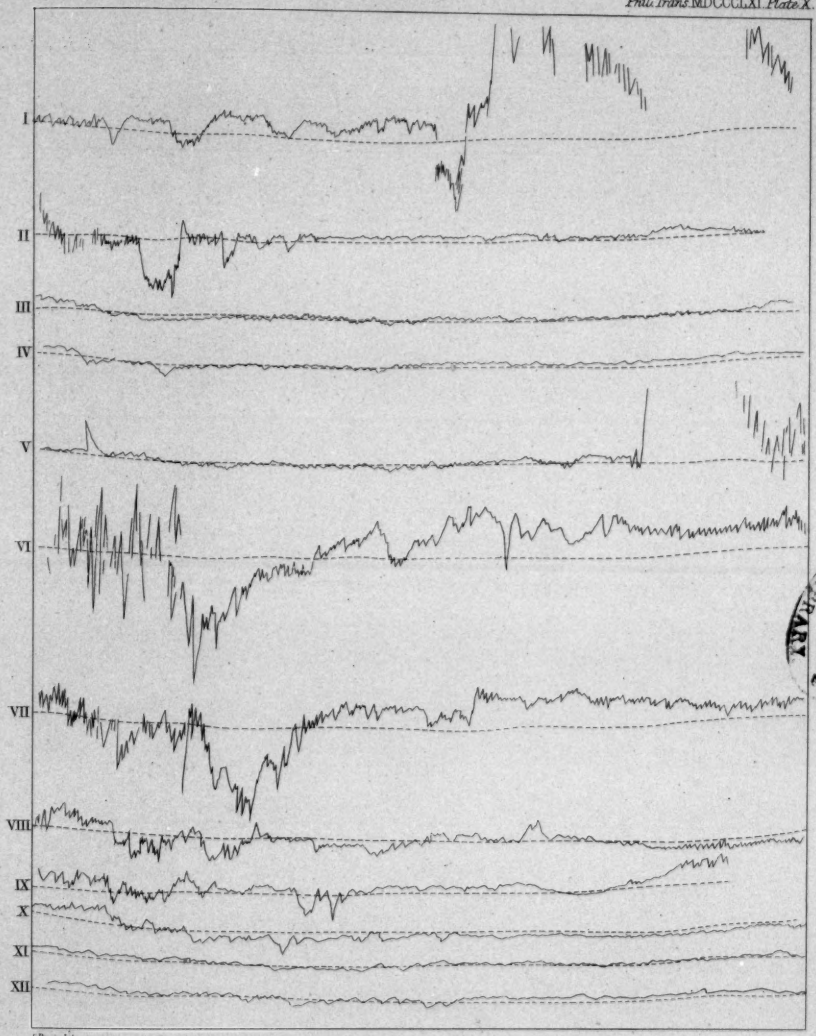

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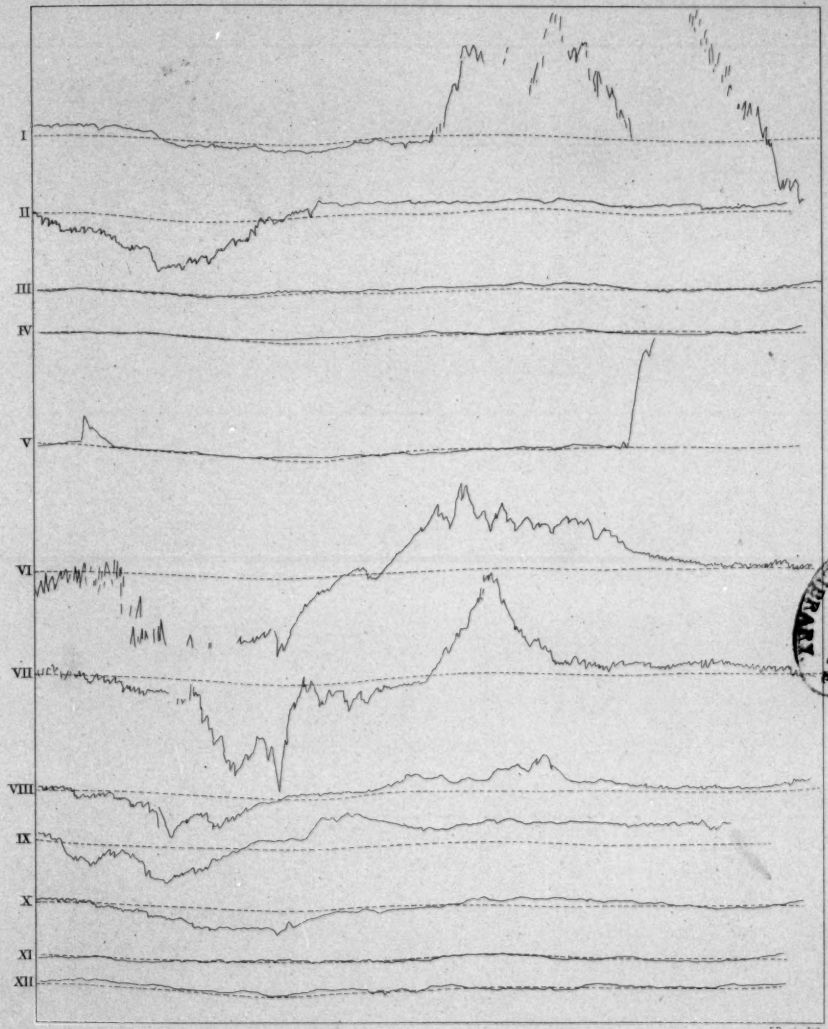
VERTICAL FORCE.

Phil. Trans. MDCCCLXI. *Plate XI.*



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J. Basire lith.

Fig. 4.

Fig. 1.
Plan of Pot.

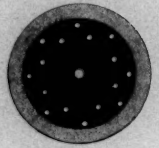


Fig. 2.
Elevation of Pot.

Fig. 3
Elevation of Pot & Pan.

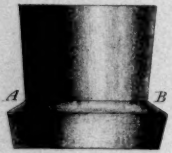
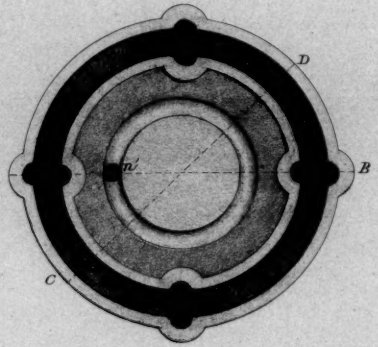
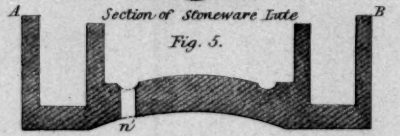
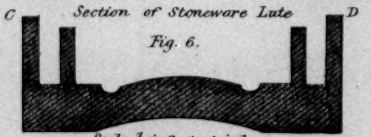


Fig. 4.
Plan of Stoneware Lute



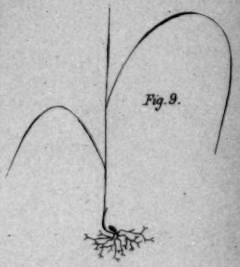




Scale ½ inch to 1 inch

Fig. 7.

Fig. 9.



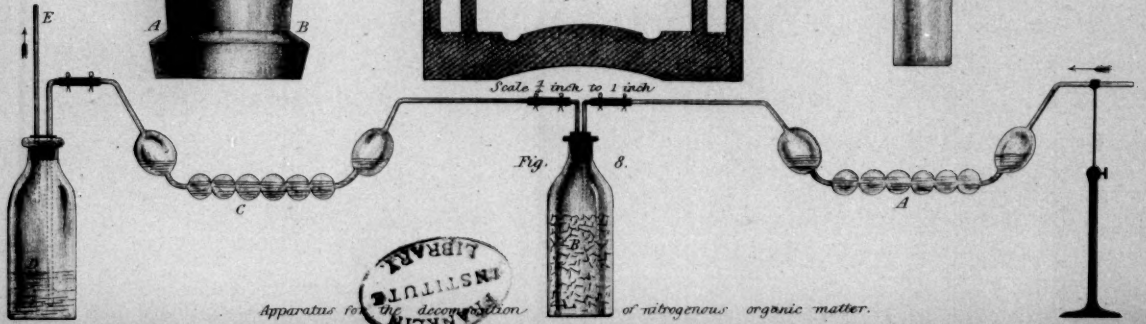


Fig. 8.

Apparatus for the decomposition of nitrogeneous organic matter.

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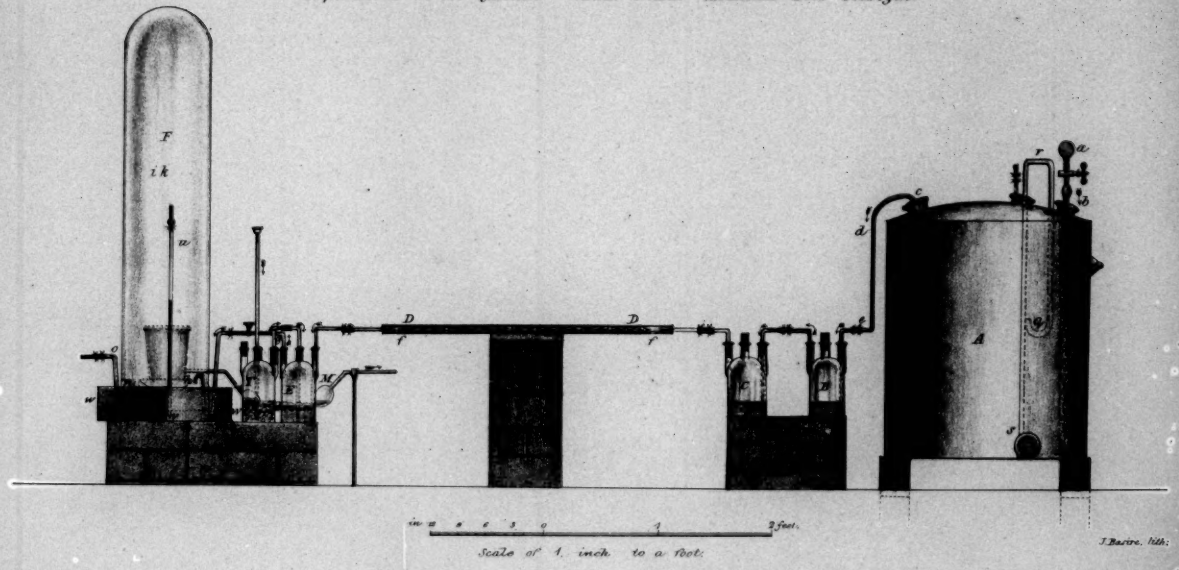
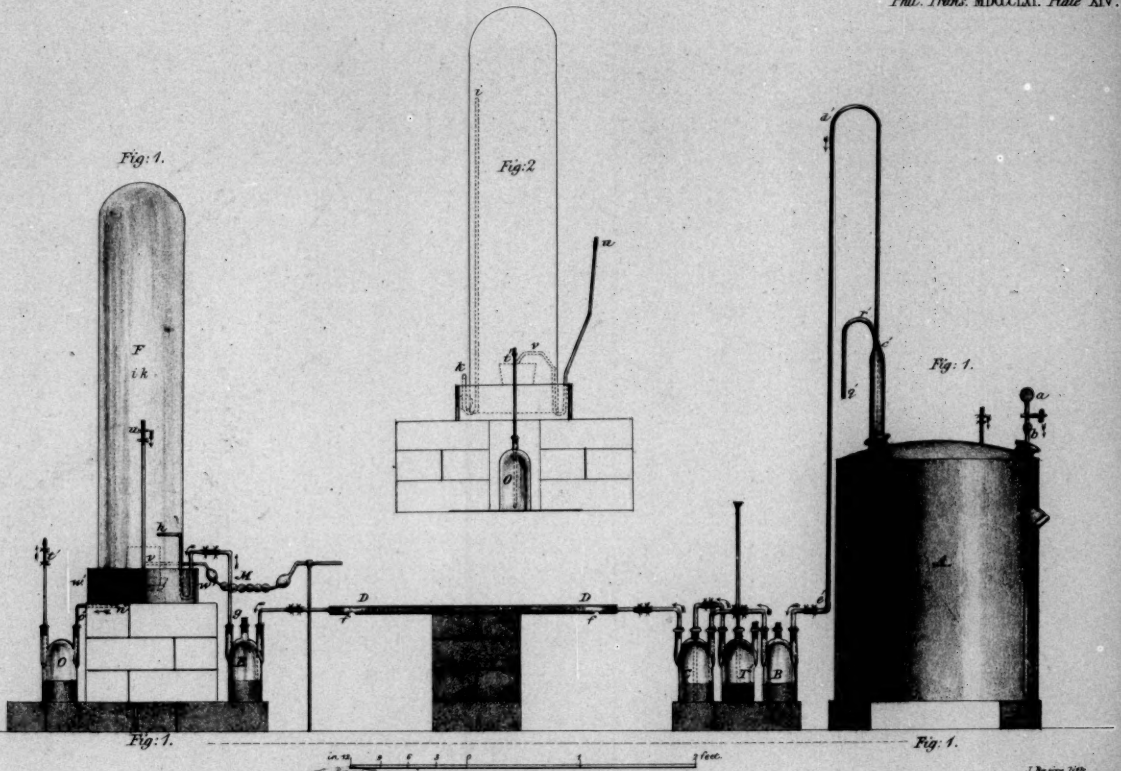
APPARATUS USED IN 1857,

in Experiments on the Question whether Plants assimilate Free Nitrogen.



Scale of 1 inch to a foot.

J. Basire, lith.



Apparatus used in 1858 & 59 Experiments on the Question whether Plants assimilate Free Nitrogen.

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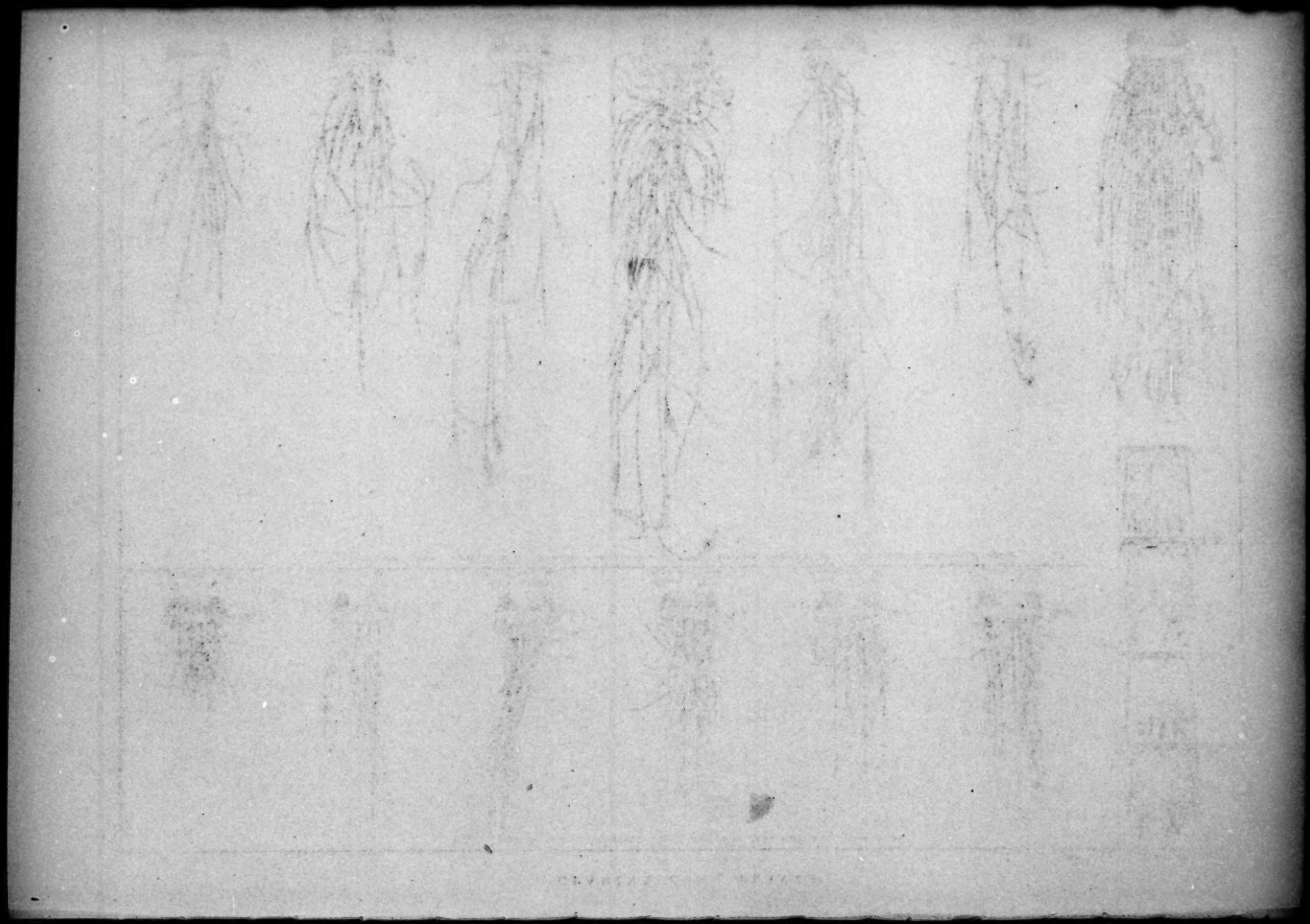


GRAMINACEOUS PLANTS

GRAMINACEOUS PLANTS

Phil. Trans. MDCCCLXI. *Plate XV.*

Supplied with no other combined Nitrogen than that contained in the seed sown.

Fig. 1. *Wheat — 1857 in prepared soil.*

Fig. 2. *Barley — 1857 in prepared soil.*

Fig. 3. *Barley — 1857 in prepared pumice.*

Fig. 4. *Wheat — 1858 in prepared soil.*

Fig. 5. *Barley — 1858 in prepared soil.*

Fig. 6. *Oats — 1858 in prepared soil.*

Supplied with known and limited quantities of combined Nitrogen beyond that contained in the seed sown.

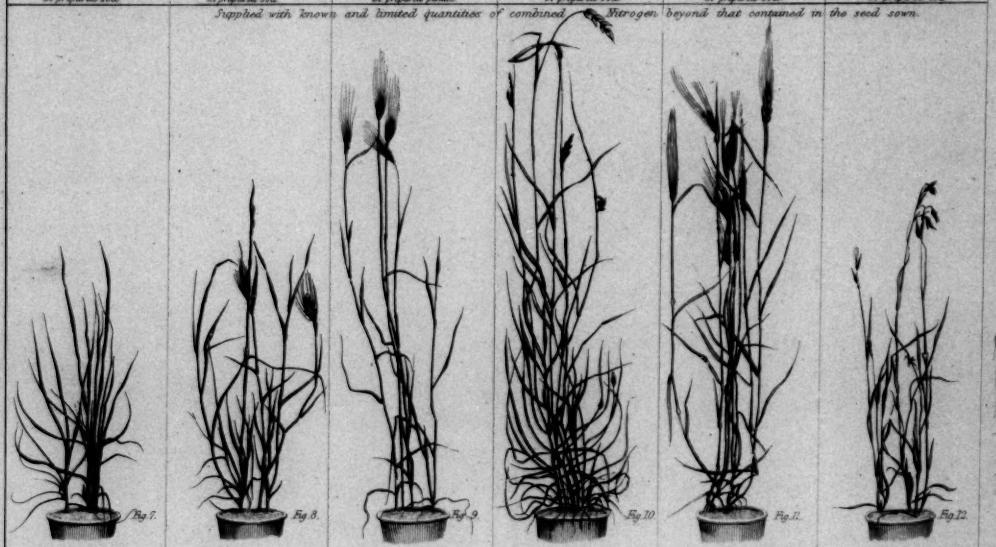


Fig. 7. *Wheat — 1857 in prepared soil.*

Fig. 8. *Barley — 1857 in prepared soil.*

Fig. 9. *Barley — 1857 in prepared pumice.*

Fig. 10. *Wheat — 1858 in prepared soil.*

Fig. 11. *Barley — 1858 in prepared soil.*

Fig. 12. *Oats — 1858 in prepared soil.*

Fig. 13. *Wheat Barley & Beans — 1857 in Garden soil.*

Fig. 14.

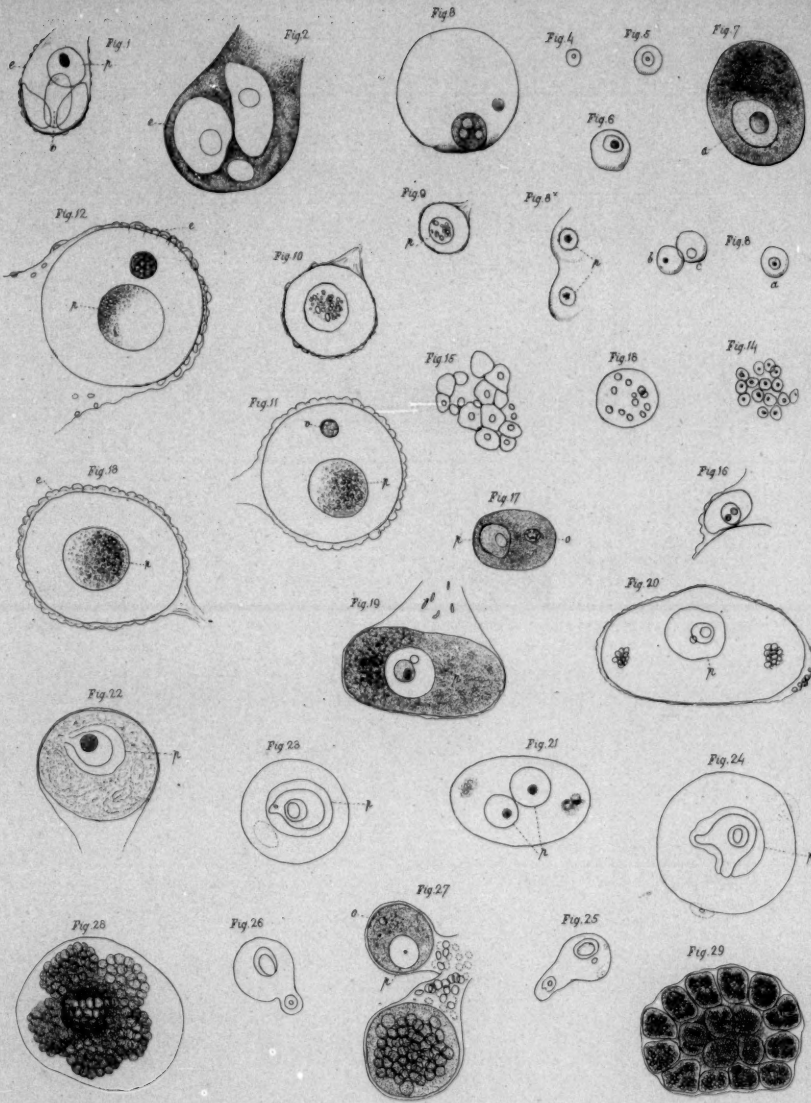
Fig. 15.

Fig. 16.

Fig. 17.

J. Basire Sc.

Scale 1½ inch to a foot.



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J. Lubbock. ad. nat. del.

Edwin. M. Williams. sc.

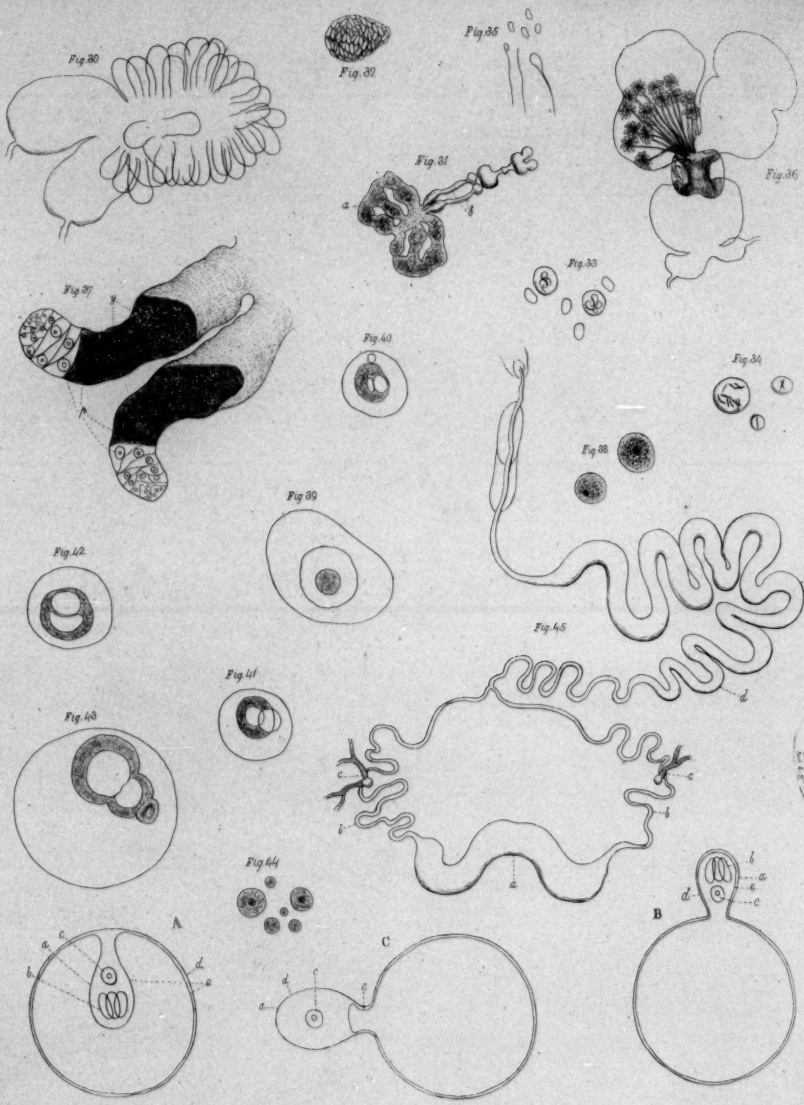

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Fig. 1.

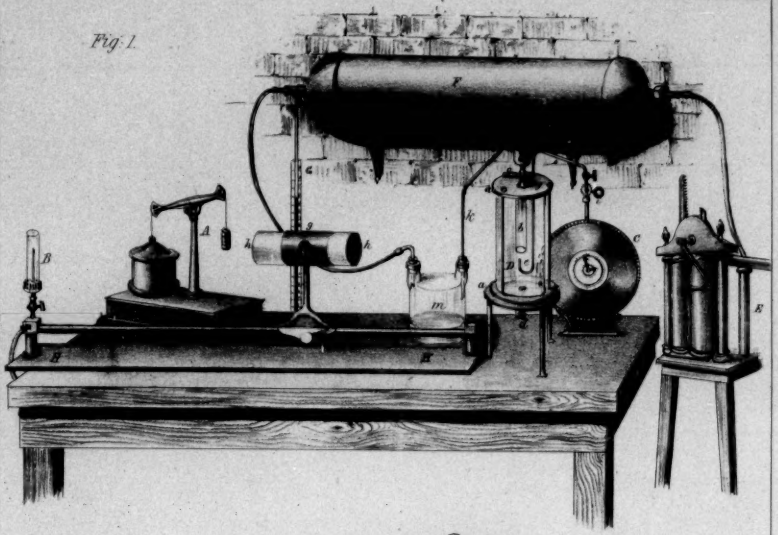
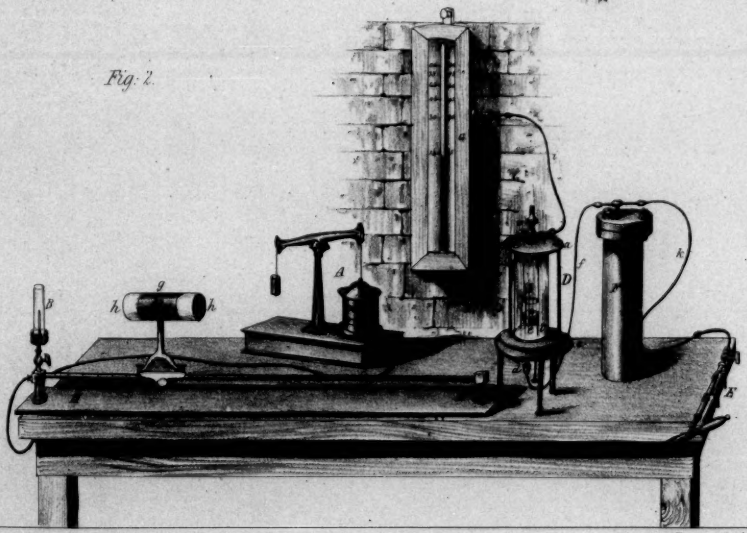


Fig. 2.



J. Basire lith.

Diagram № II.

Diagram N° I.

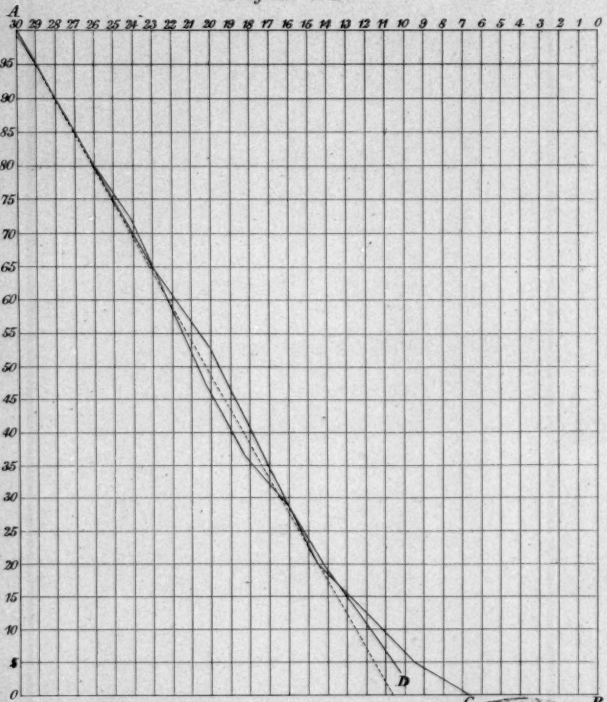


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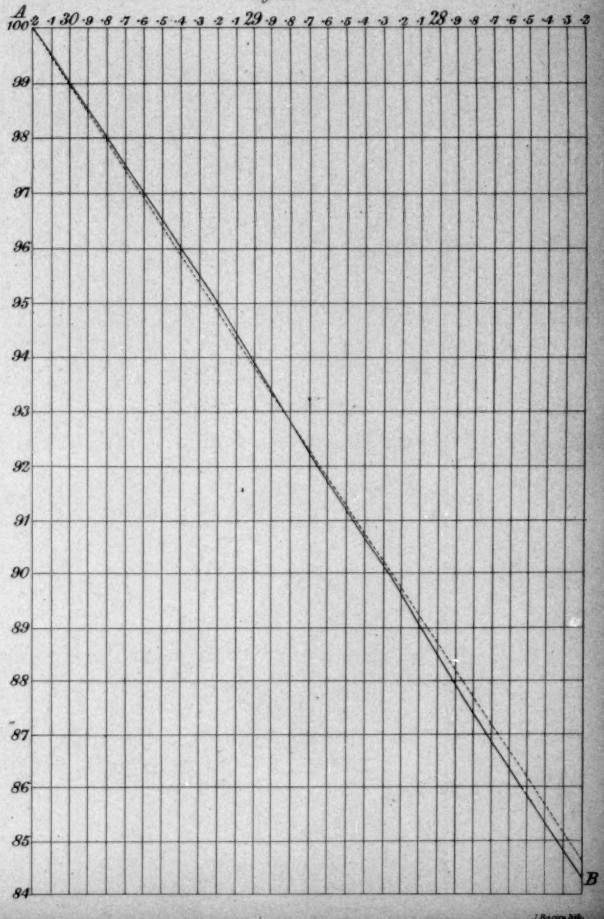
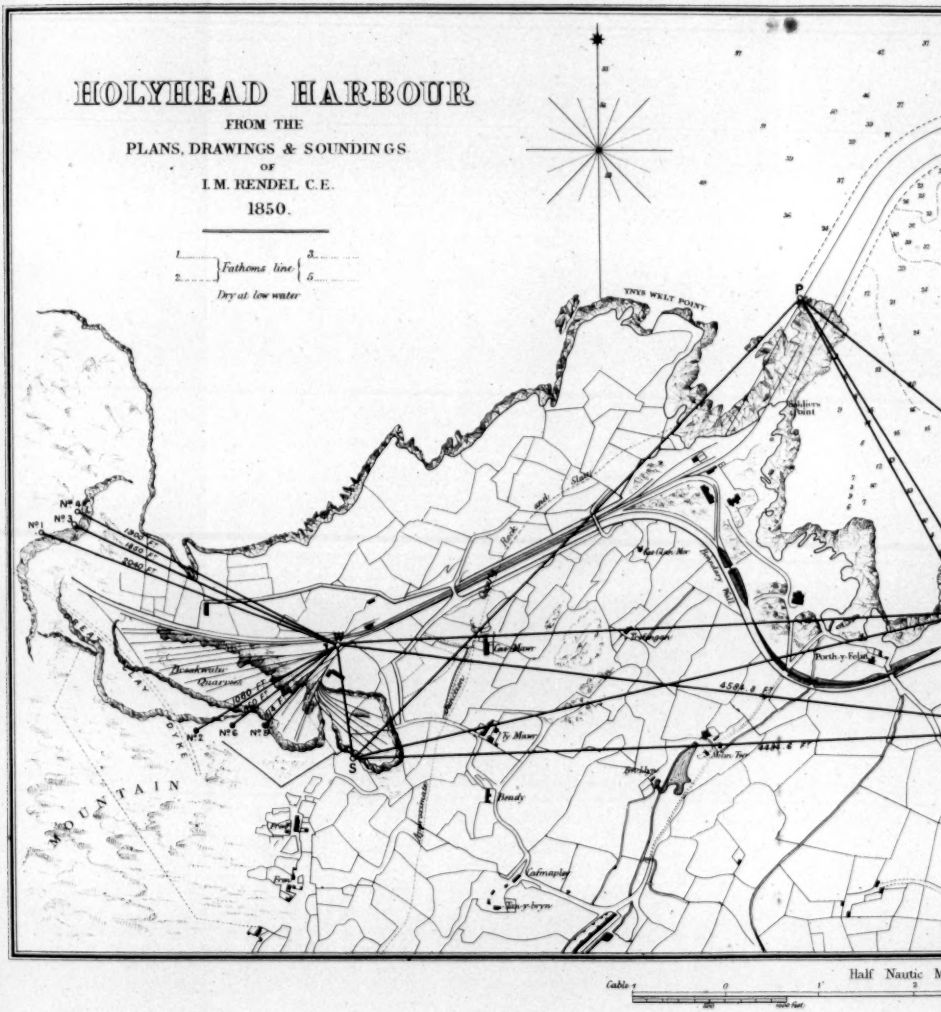
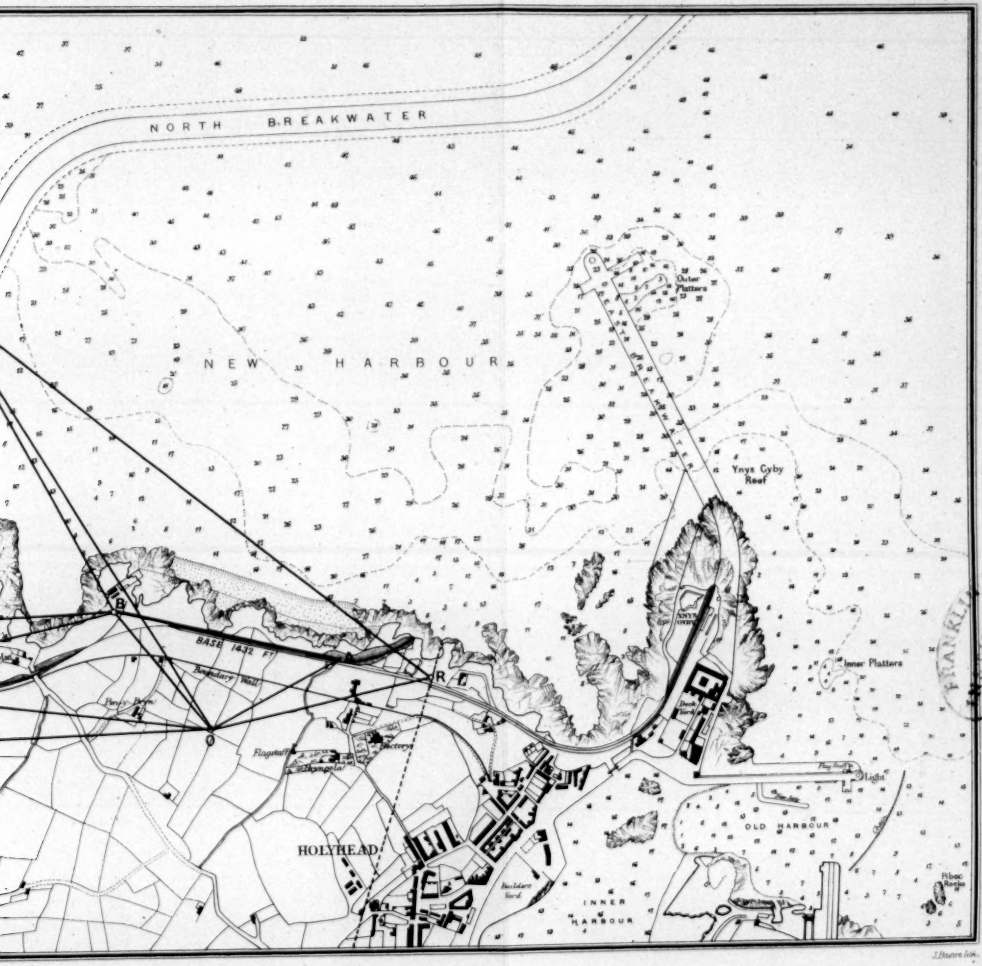
Diagram N° II.



J. Basire, lith.


HOLYHEAD HARBOUR
FROM THE
PLANS, DRAWINGS & SOUNDINGS
OF
I.M. RENDEL C.E.
1850.
1.
2.
Fathoms line.
3.
5.
Dry at low water
YNYS WELT POINT
Soldiers Point
Stanley Rock
Admiralty Pier
Breakwater Quarries
Ty Mawr
Penrhyn
Cafnarplas
Ty-yr-bryn
Mountain
Cable 1
0
1
2
Half Nautic M



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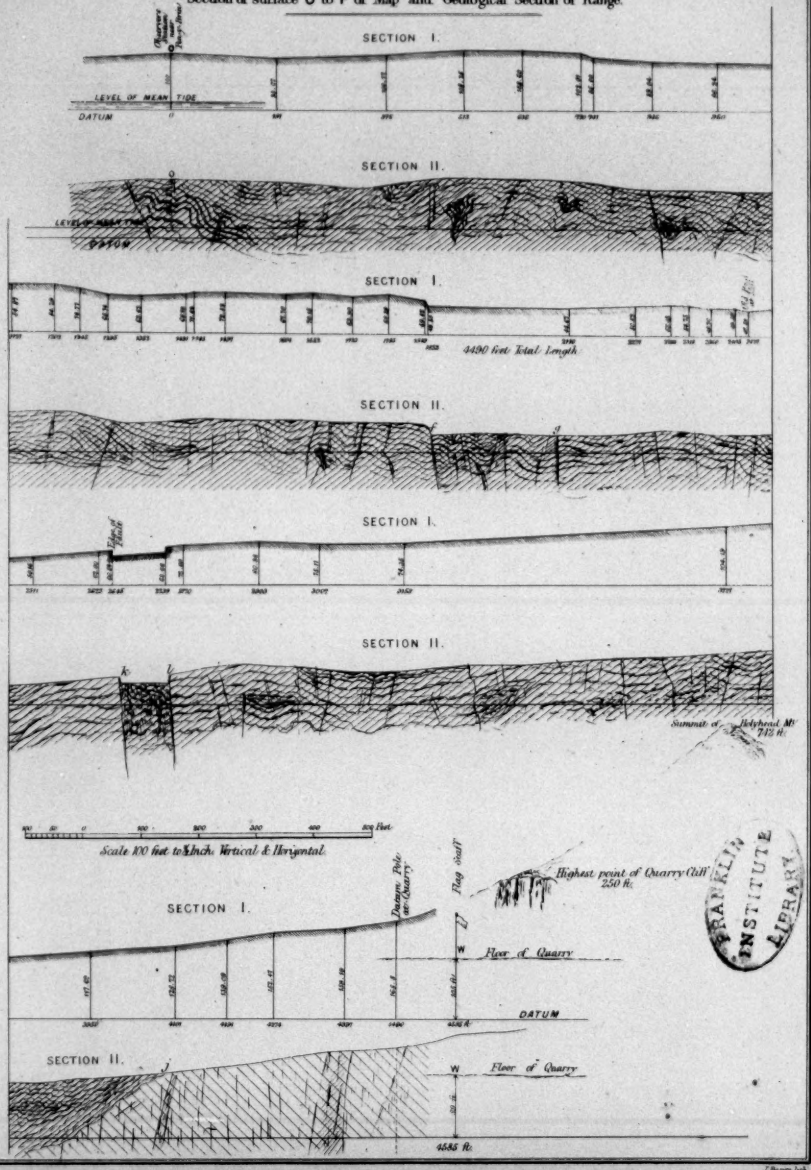
EARTHQUAKE EXPERIMENTS, HOLYHEAD QUARRIES.

Section of surface O to F of Map and Geological Section of Range.



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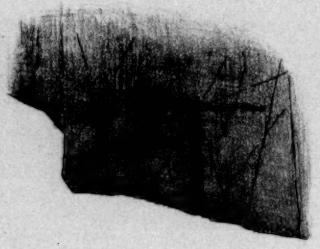
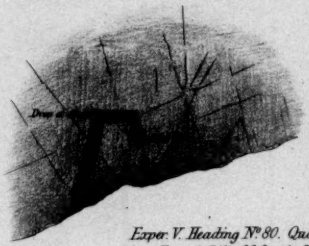
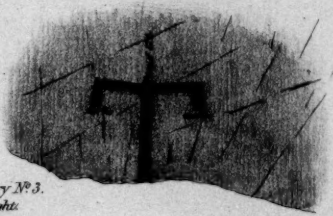
Exper. I. Heading N° 46. Fishers Quarry
Face of Cliff. 120 feet in height.



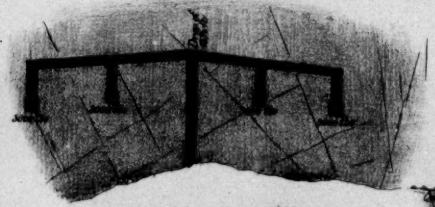
Exper. II. Heading N° 10. Quarry N° 3.
Face of Cliff. 100 feet in height.



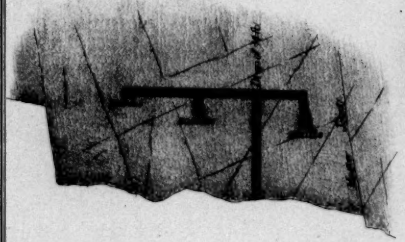
Exper. III. Heading N° 31. Quarry N° 9.
Face of Cliff. 60 feet in height.

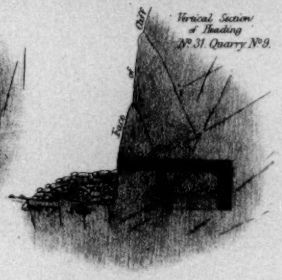


Exper. V. Heading N° 80. Quarry N° 3.
Face of Cliff. 90 feet in height.

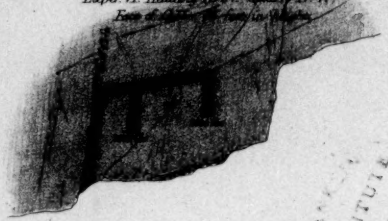


Exper. IV. Heading N° 33. Quarry N° 9.
Face of Cliff. 115 feet in height.





Exper. VI. Heading N° 44. Quarry N° 4.
Face of Cliff. 80 feet in height.



HORIZONTAL AND VERTICAL SECTIONS
OF THE
SEVERAL HEADINGS EXPERIMENTED UPON
AT THE GOVERNMENT QUARRIES,
HARBOUR WORKS, HOLYHEAD.

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J. Basire lith.

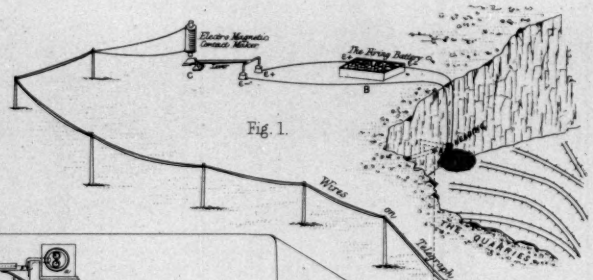


Fig. 1.

Elevation of the Contact Maker.

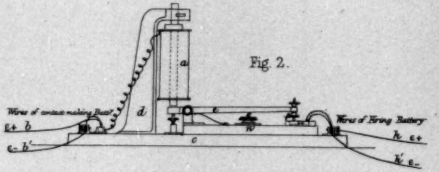


Fig. 2.

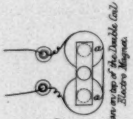


Fig. 3.

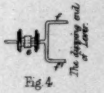


Fig. 4.

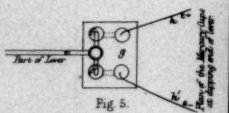
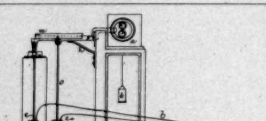


Fig. 5.



The Chronograph.
Fig. 1.*

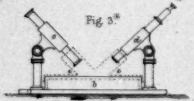
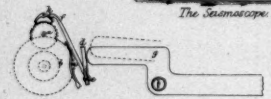


Fig. 3.*
The Seismoscope.

Fig. 2.*



Releasing Apparatus of Chronograph.
Fig. 4.*

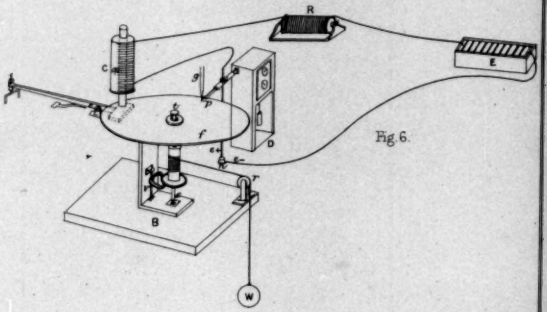
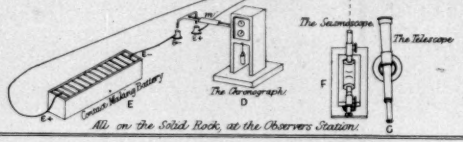
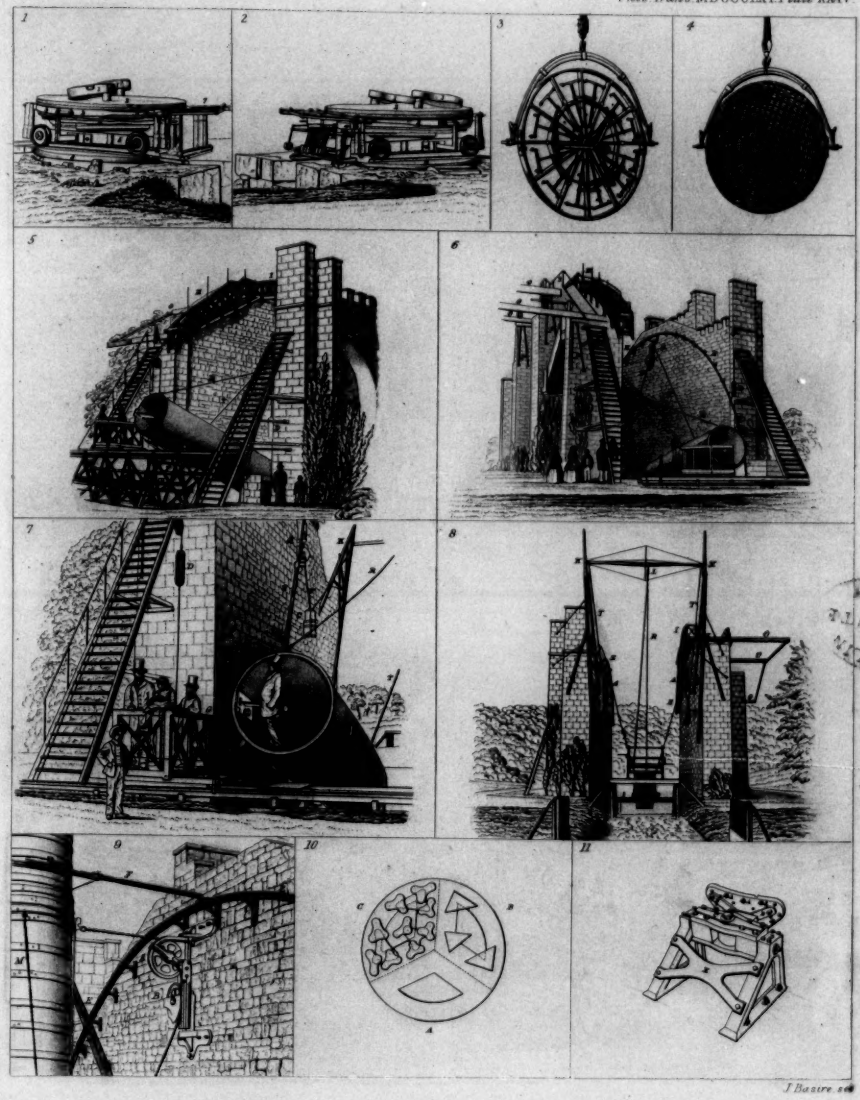


Fig. 6.



All on the Solid Rock, at the Observers Station.

J. Basire Sculp.



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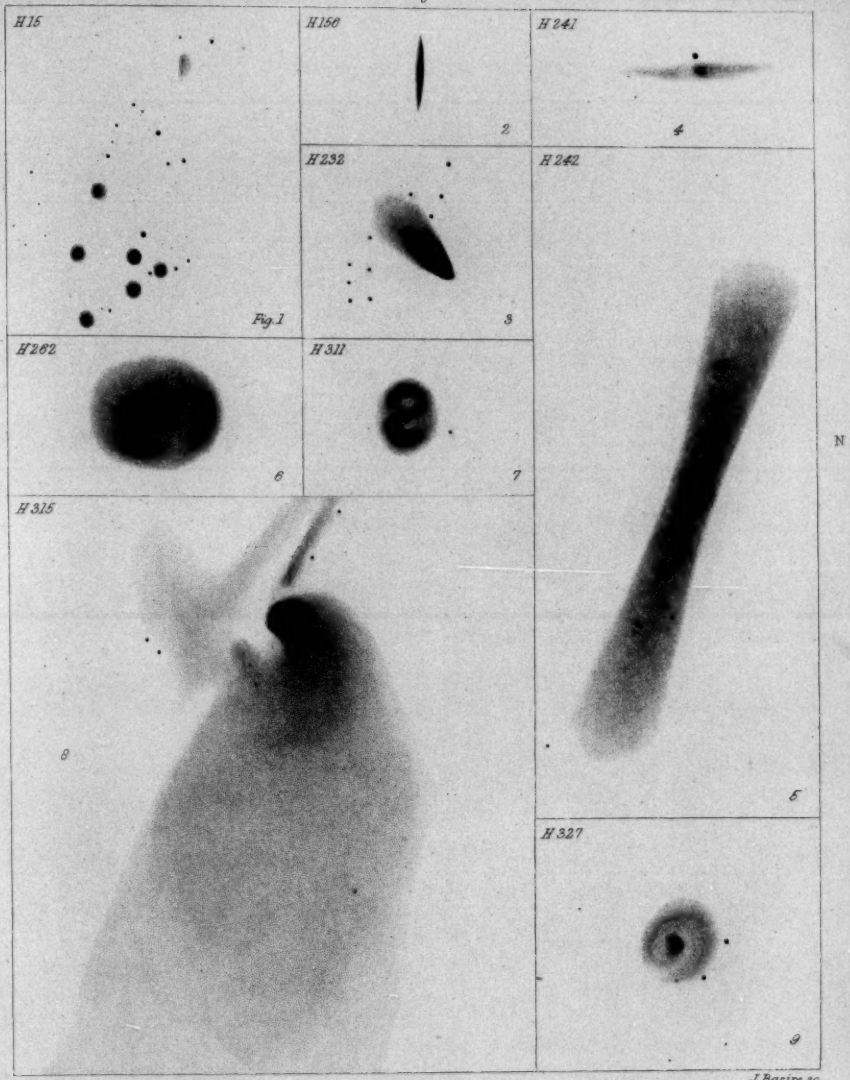
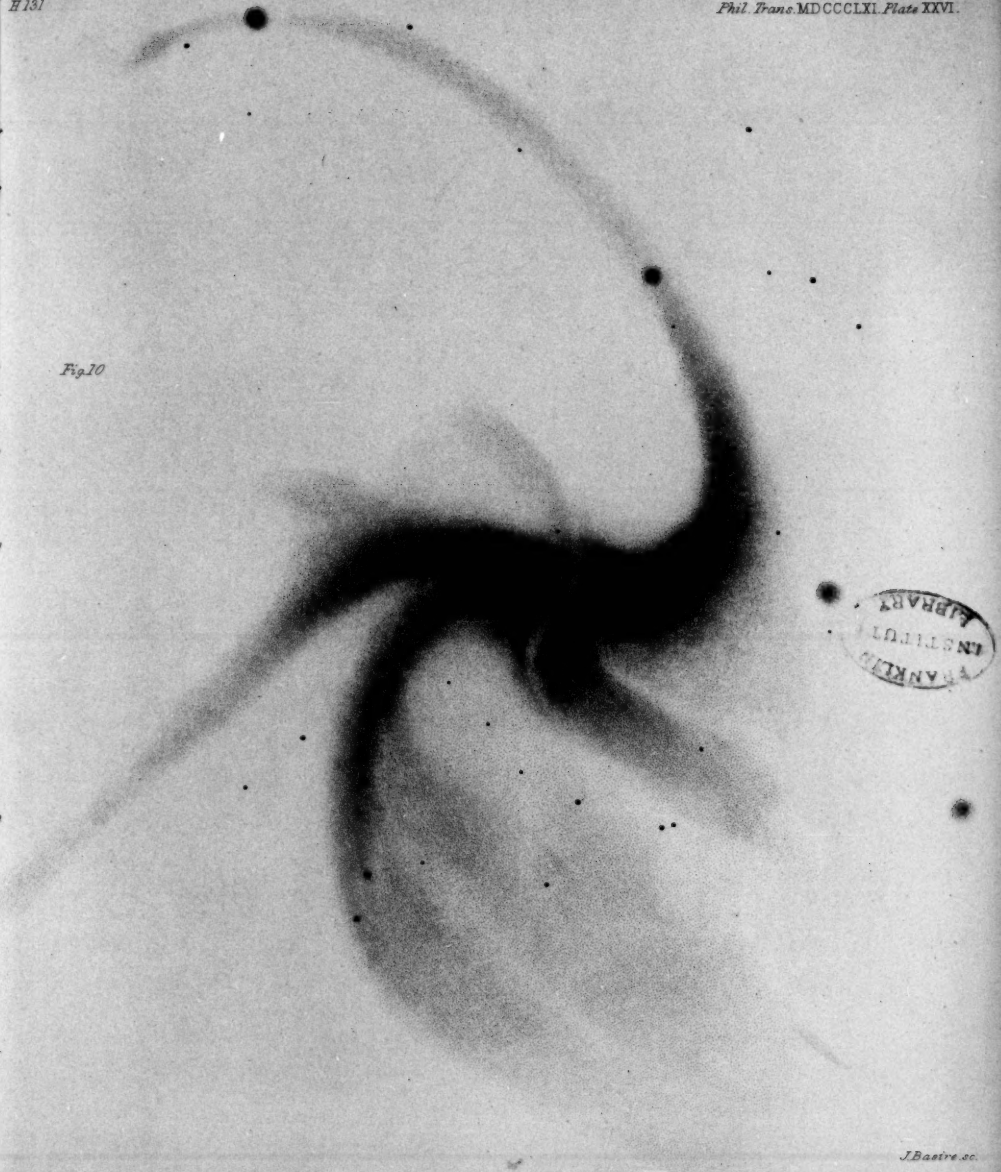


p

J. Basire sc.

Fig. 10



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J. Basire sc.

H 393

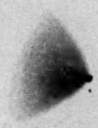


Fig. 11

H 421

12

H 689

13

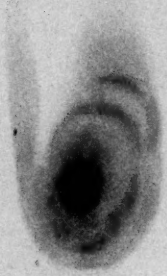
H 765 & 766

15

H 692

14

H 857



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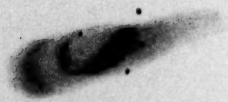
H 1011



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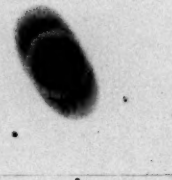
H 1052 & 1053

18

H 1061

19

H 1111 & 1113



20

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H 1196

21

H 1245

22

H 1306 & 1308

23

J. Basire sc.

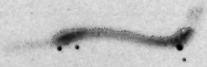
H 1337

Fig 24

H 1385

25

H 1414 & 15



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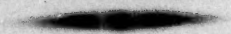
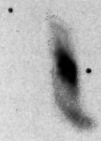
H 1441



27

H 1589

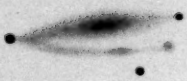


28

H 1650

29

H 1713



30

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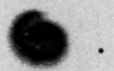
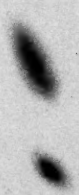
H 1946



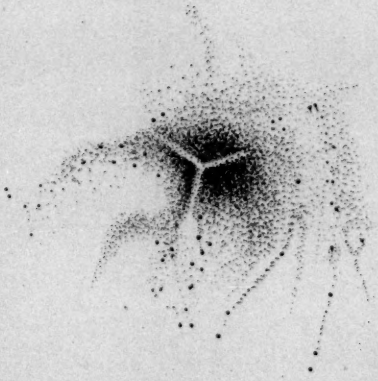
32

H 1905



31

H 1968



33

H 2075

34

J. Basire sc.

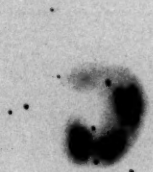
Fig 35

J. Basire sc.

H 2084

Fig 36

H 2090



37

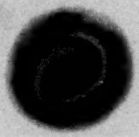
H 2139

38

H 2172

39

H 2241

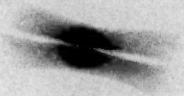


40

H 2245

41

H 2297



42

J. Basire sc.

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Fig. 43

J.Basire sc.

Fig. 5.

Monthly Averages.

Daily elimination of Urea & Urinary Water with Temp.ᵉ & Barometric Pressure.

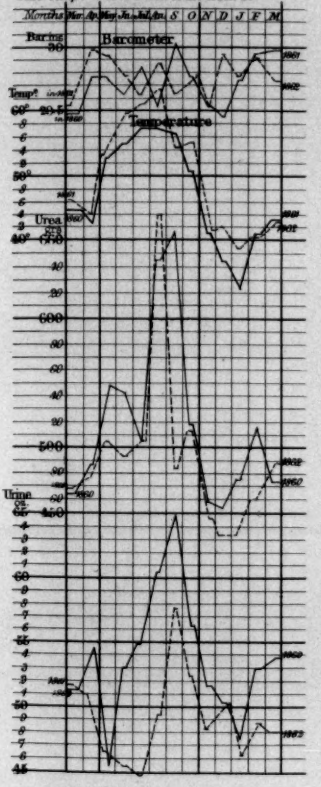


Fig. 2.

Urine ℥.

on succeeding days.

Waves.

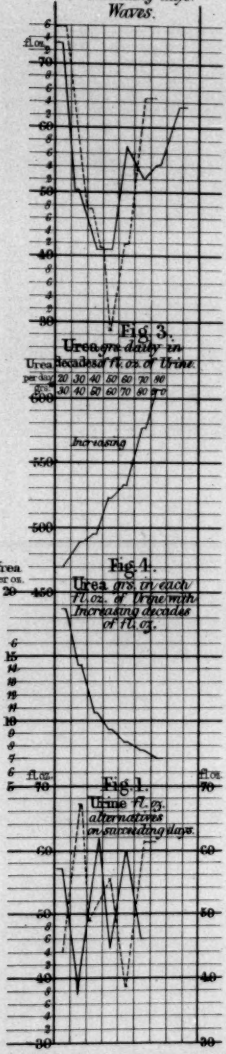


Fig. 3.

Urea *grs. daily in*

Decades of fl. oz. of Urine.

Fig. 4.

Urea *grs. in each fl. oz. of Urine with Increasing decades of fl. ℥.*

Fig. 1.

Urine *fl. ℥. alternatives on succeeding days.*

Fig. 6.

Relation of Temperature to Urea.

on the same day.

on the succeeding day.

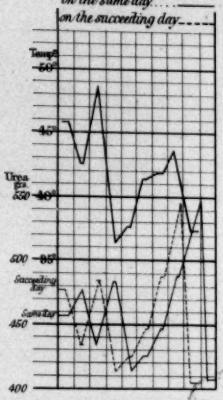
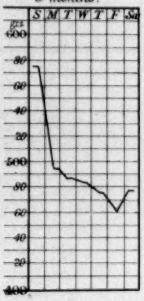


Fig. 7.

Amount of Urea excreted on each day of the week on an average of 9 months.



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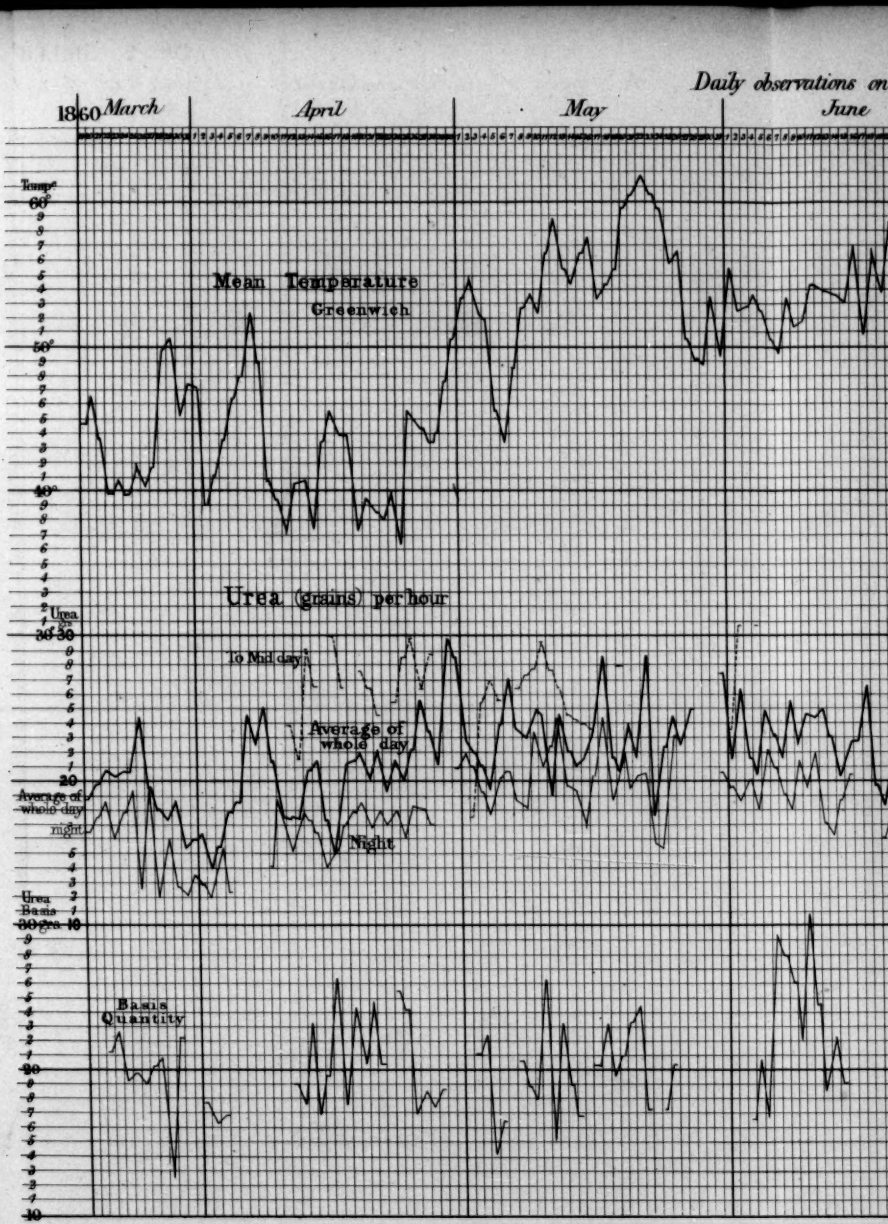

Daily observations on
1860 March
April
May
June
Temp.
60°
50°
40°
Mean Temperature
Greenwich
Urea (grains) per hour
Urea
30
20
To Mid day
Average of whole day
Night
Urea
Basis
10
Basis Quantity

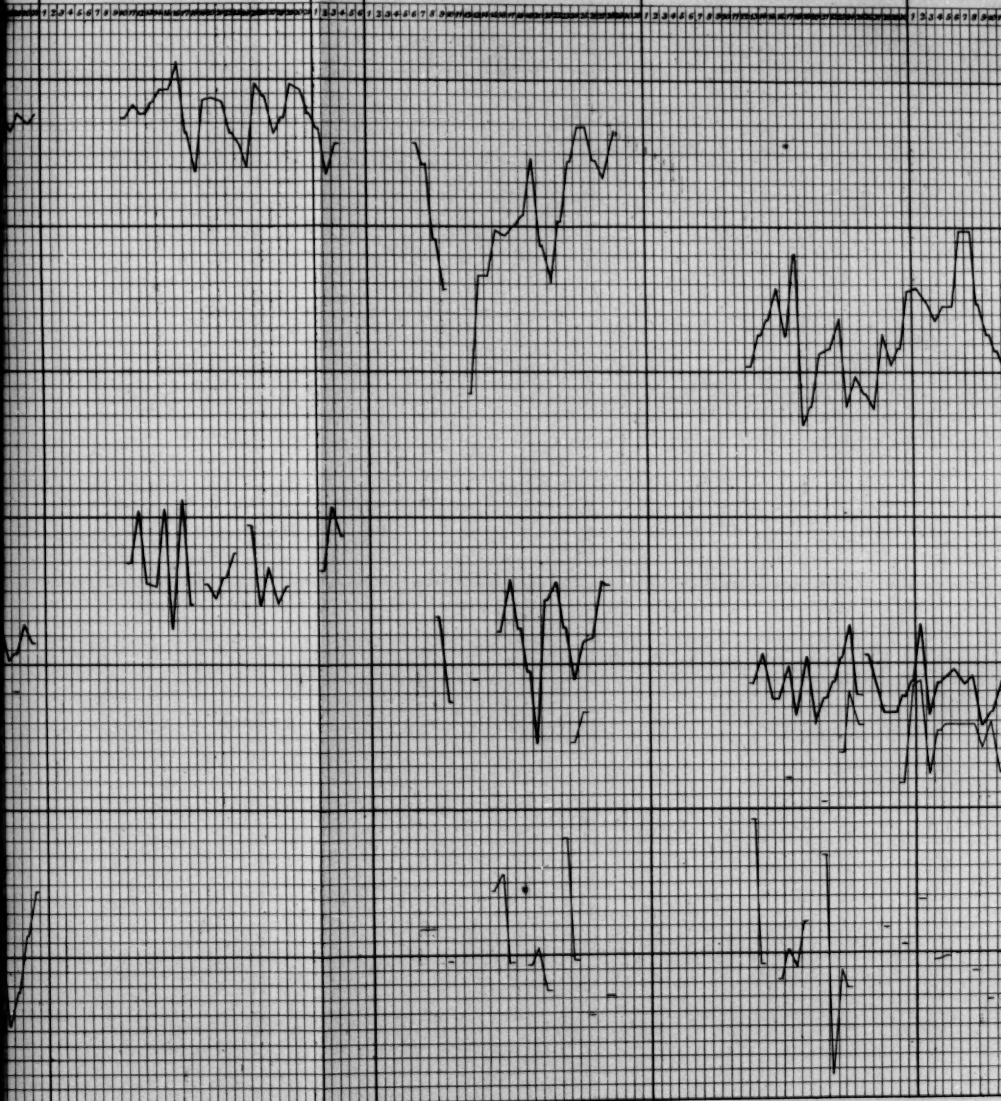
DR E. SMITH'S PAPER ON UREA.

ations on the excretion of Urea at four periods on the whole day the night before breakfast (basis quantities)

Dr E. SMITH'S PAPER on UREA.

riods on the whole day the night before breakfast (basis quantities) & before midday compared wit

August | Sept.r | October | November



ared with the Mean daily Temp.e at Greenwich.

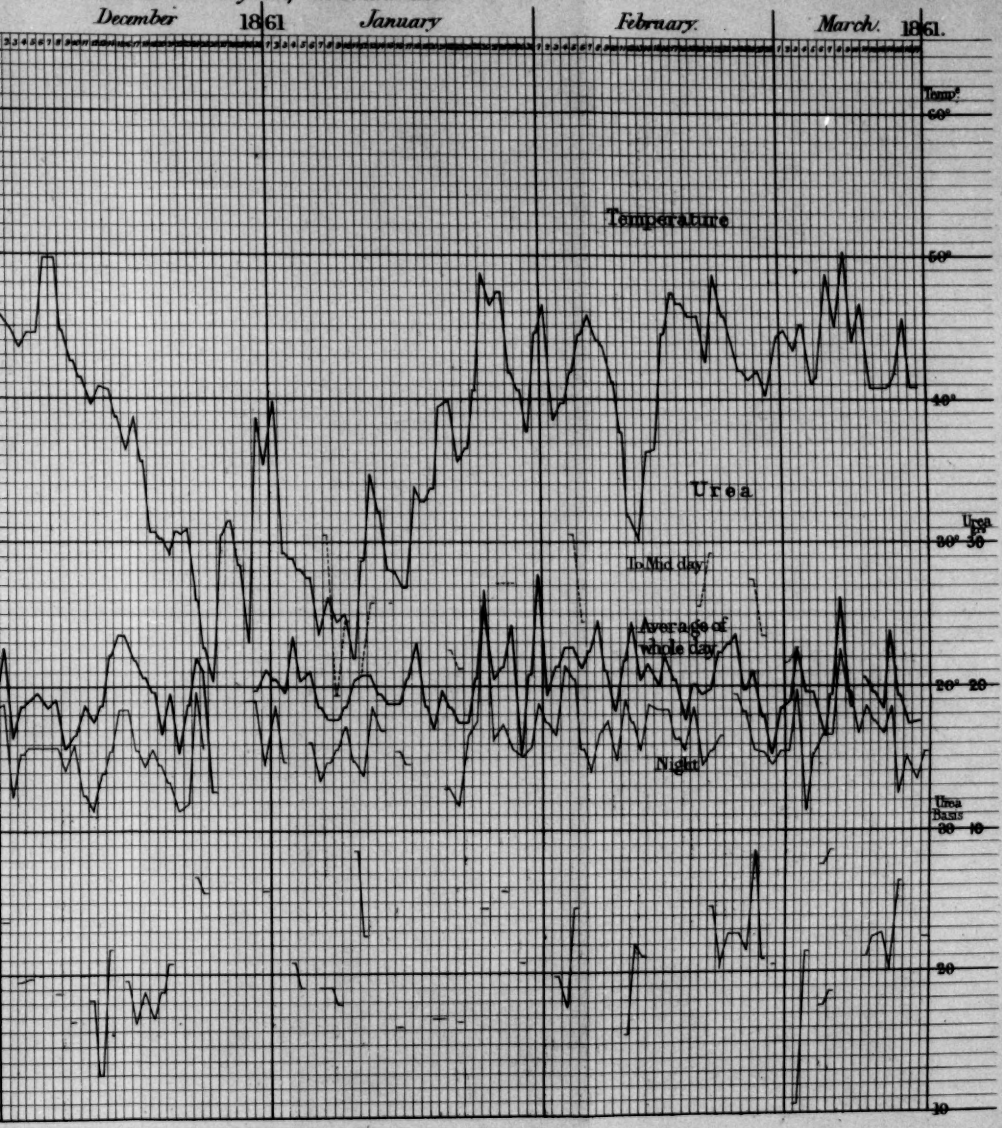


Fig. 1.

Hourly elimination of Urea & Urinary Water on Mar 1 & 2, 1860, with the proportion of Urea in each oz. of Urine.

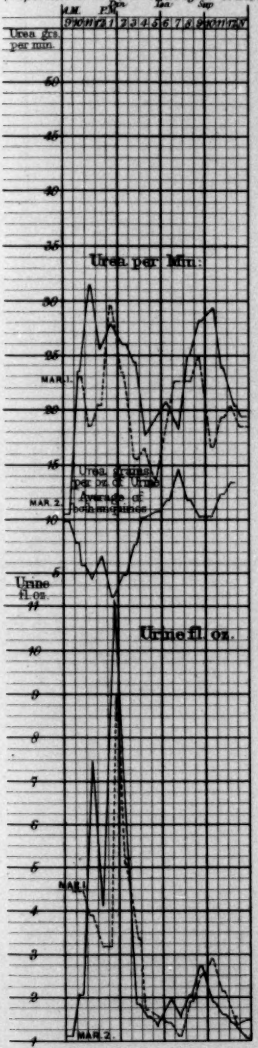


Fig. 2.

Hourly elimination of Urea & Urinary Water at each ¼ of an hour; Dec. 17 & 18. 1861.

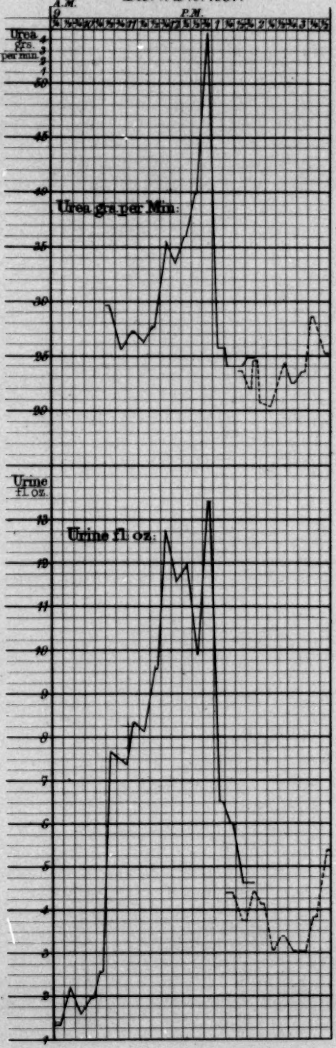


Fig. 3.

Hourly elimination of Urea & Urinary Water with the rate of pulsation when Fasting except from Water.

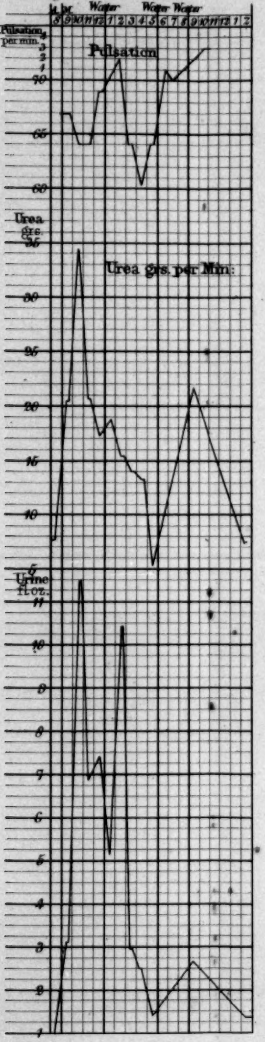


Fig. 4.

The daily weight of the body taken Naked & after the Emission of Urine on returning to bed & rising. Subtracting a constant weight of 13 Stones or 182 lbs

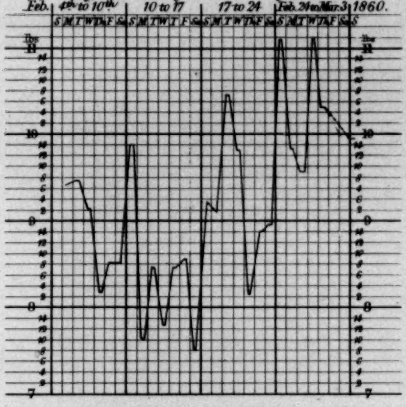


Fig. 5.

Hourly elimination of Urea & Urinary Water under the influence of Water, Tea & Coffee with Bread.

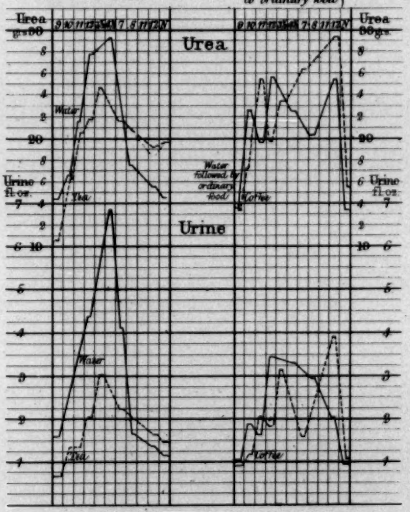


Fig. 6.

Shewing the effect of drinking water over the excretion of Urea & Urine.

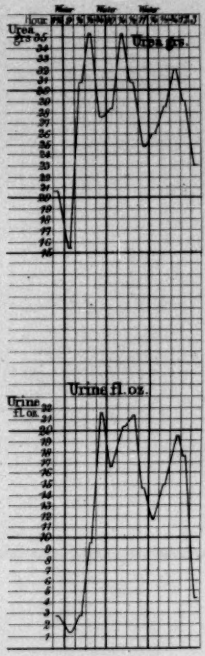
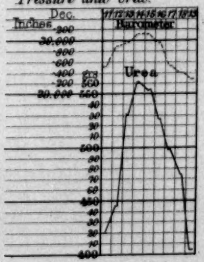


Fig. 7.

Relation of Atmospheric Pressure and Urea.



J. Basire lith.

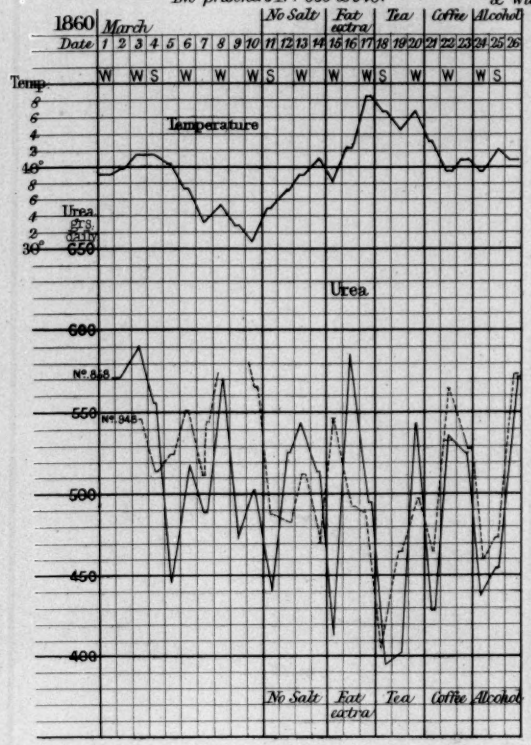
W. signifies Tread Wheel day.
S. " Sundays.

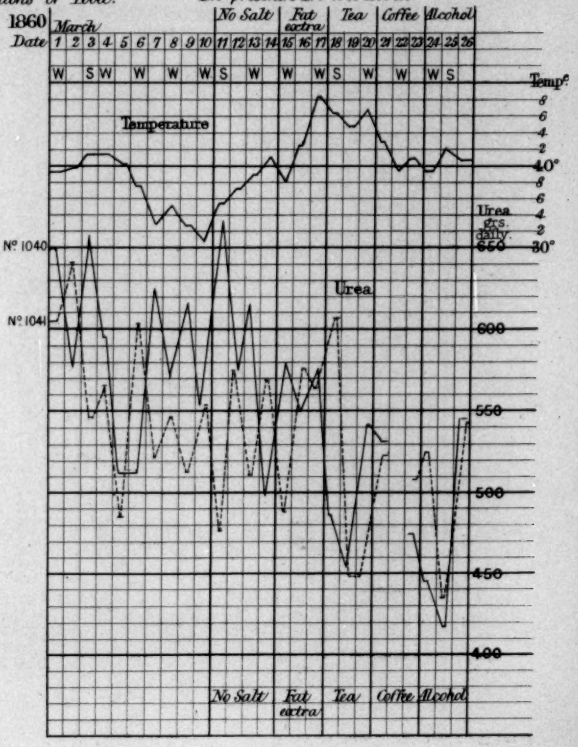
Dr E. SMITH'S PAPER on UREA.

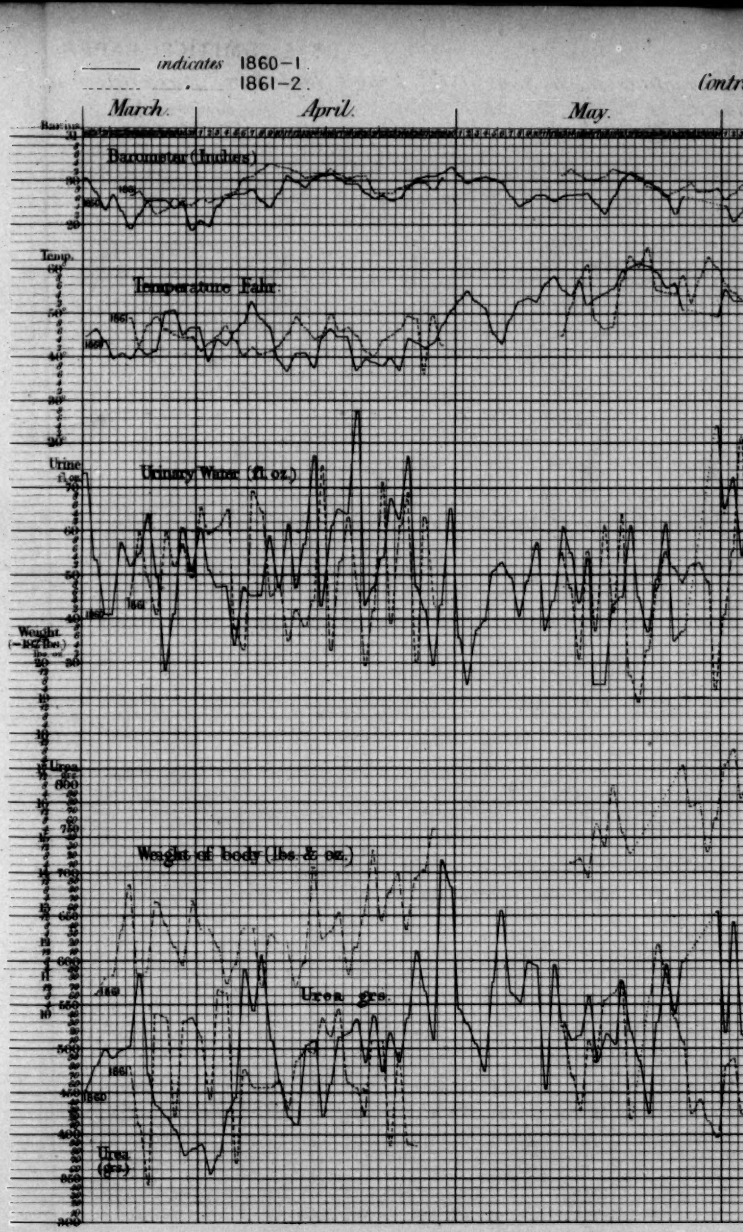
Phil. Trans. MDCCCLXI. Plate XXXV.

Urea evolved daily by four prisoners on days of Tread Wheel labour, light labour & Sundays, compared with the Mean daily Temp. at Greenwich, & with Variations of Food.

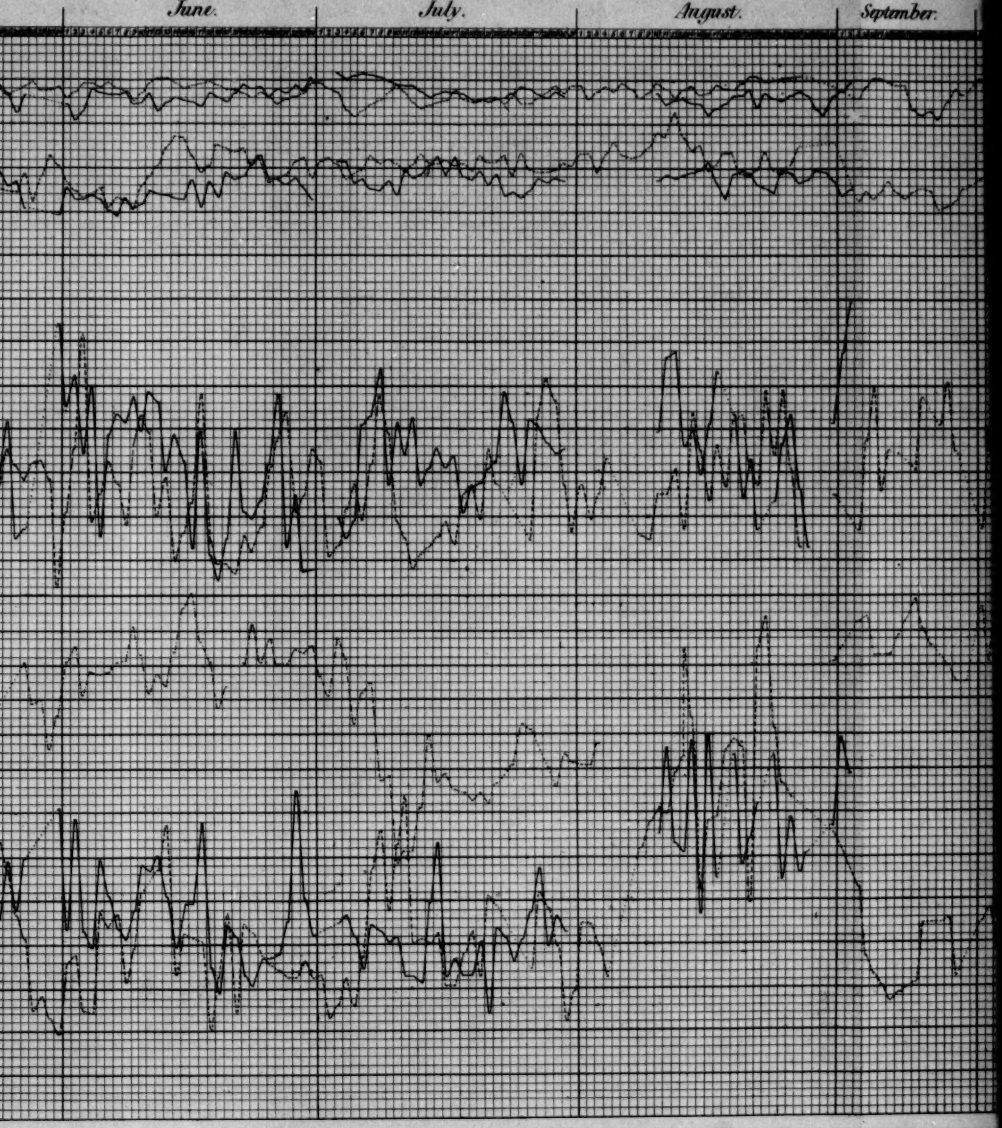
Two prisoners Nos. 858 & 948. *Two prisoners Nos. 1040 & 1041.*





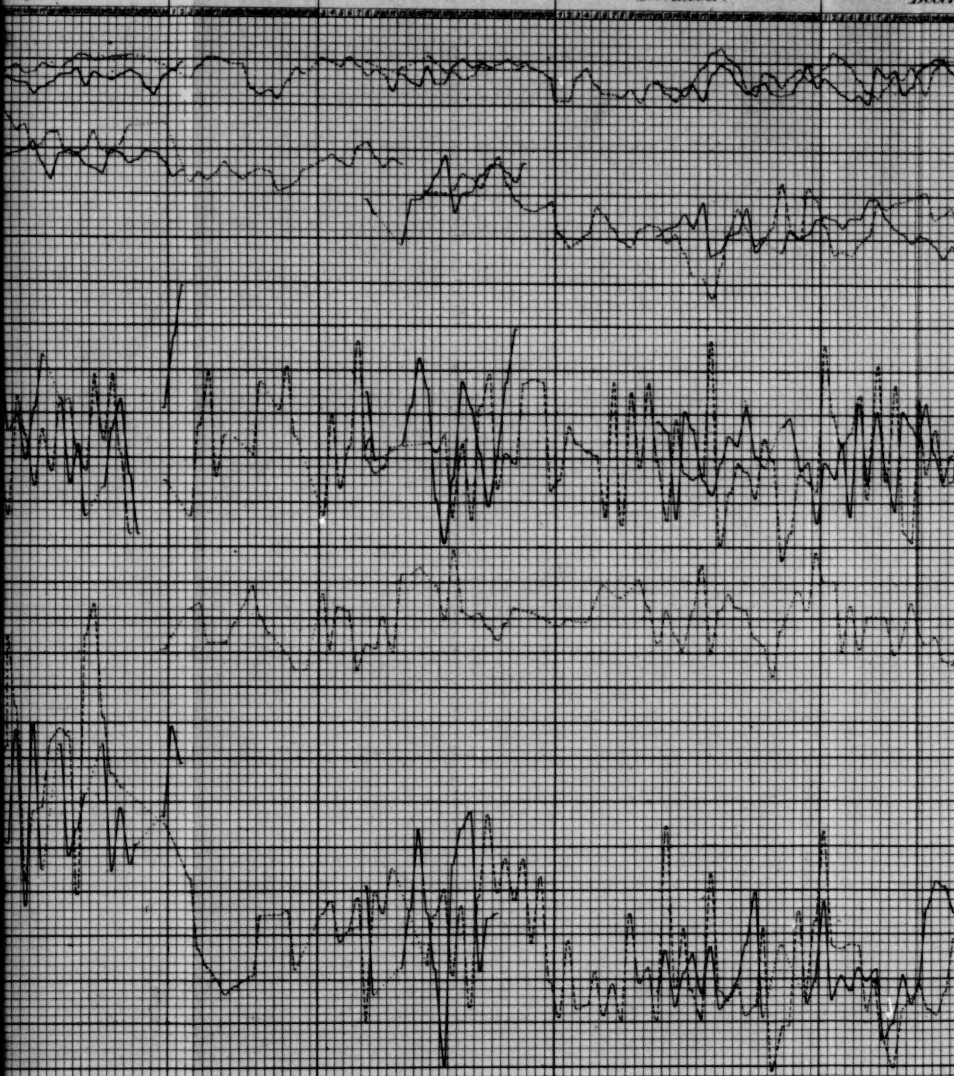

indicates 1860–1
1861–2
March.
April.
May.
Barom.
Barometer (Inches)
Temp.
Temperature Fahr.
Urine
Urinary Water (ft. oz.)
Weight (–182 lbs.)
Weight of body (lbs. & oz.)
Urea grs.
Urea (grs.)

Contrast of daily quantities in the years 1860–1 and 1861–2 in Barometric height & Tempera



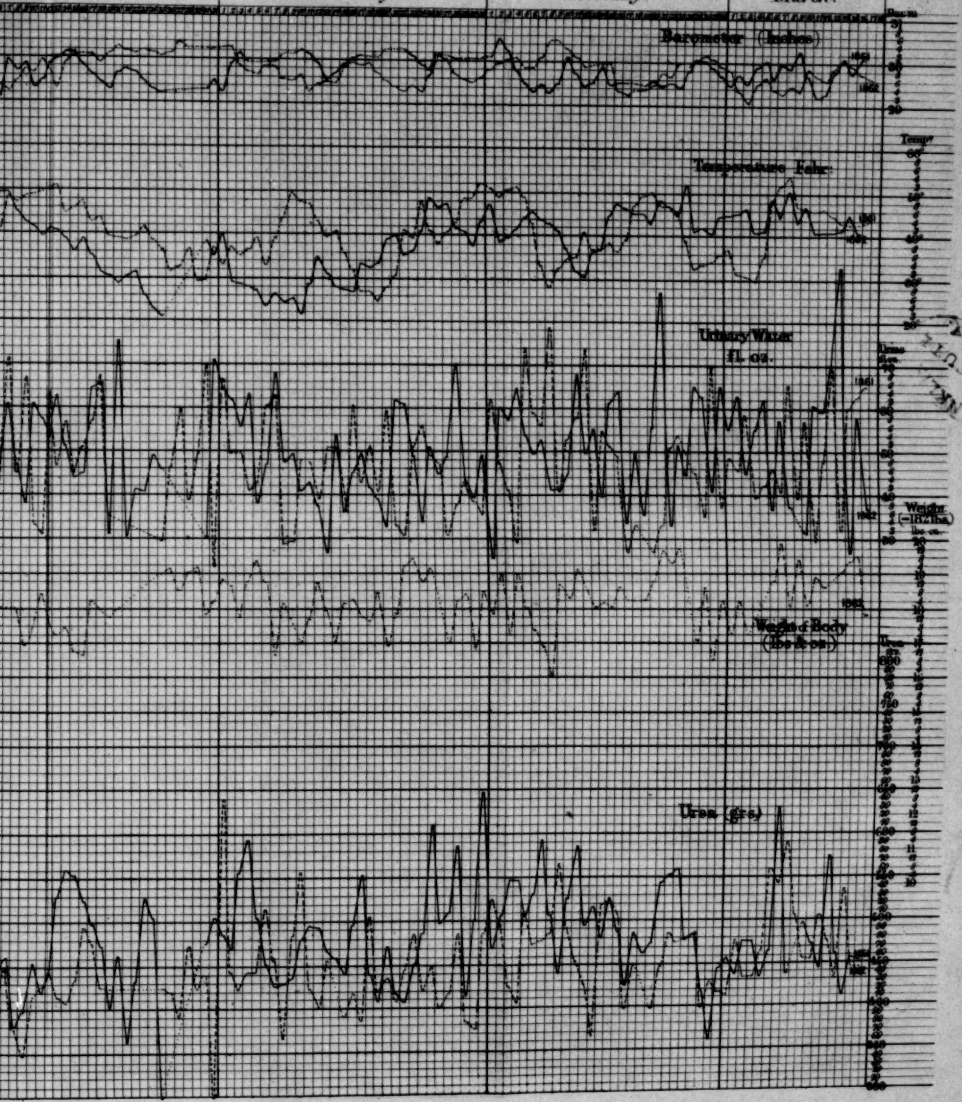
Barometric height & Temperature at Greenwich, & Urinary Water, weight of body & Urea in the

August. | *September.* | *October.* | *November.* | *Decr*



a in the Author.

December. *January.* *February.* *March.*



J. Beaire, lithe.